

RESEARCH NOTE

The economist's role in research on transnational corporations: or why the dogs have barked so softly*

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One of the many interests that link researchers in international business in a common undertaking is a desire to identify the driving forces that explain the behaviour of transnational corporations (TNCs). In the pursuit of that common interest, however, there are times when researchers seem to resemble the proverbial troop of blind people stroking the elephant. Those who come from a background in political science profess to see one kind of animal and those from sociology another, while those with a principal interest in marketing, production or finance sometimes seem located in another animal house in the zoo.

Like Sherlock Holmes, however, I am repeatedly drawn to the curious fact that economics, a discipline that might have been expected to make a powerful contribution to the analysis of the elephant, has had very little to say on the subject. This, despite the fact that so many of the faculty members of business schools think of economics as the principal discipline to which their work is related. As one who has carried a doctorate in economics as an academic trophy for over half a century, I find myself puzzling at times over this phenomenon, and wondering for how long the disjunction is likely to continue.

In describing the extent of that disjunction, it is possible to think of various qualifications and exceptions. But I intend to stick to large generalizations without following the usual academic practice of providing a string of exculpatory qualifications tucked away in footnotes. In any case, the disjunction has not always been complete and may even prove to be less so in the future.

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The economist's terrain

Let me observe from the outset that economists have defined the proper study of economics in many different ways. Still, in the decades since the 1950s, those who occupy the commanding heights of the profession have placed the study of neoclassical theory and the general equilibrium model at the core of the discipline. Accordingly, economists have typically studied the behaviour of markets rather than that of firms; and their principal objective has been to describe the behaviour of those markets in relation to an assumed equilibrium. As a rule, therefore, they have taken off from the assumption that the markets in which the firms acquire their inputs—such as capital, labour, and intermediate materials—can be reasonably efficient, and that the markets in which they sell their outputs can be efficient as well. And where “imperfections” have encumbered the market, such as barriers to entry or exit, taxes, or imperfect information, a characteristic challenge that researchers have set themselves has been to take these factors into account in reaching their conclusions regarding such prices and volumes.

True, some economists such as George Akerlof, William Baumol, Herbert Simon, Harvey Liebenstein, Richard Nelson and Martin Shubik have managed to remain honoured in the mother church while concentrating on the inadequacies of these central assumptions. And in this emphasis they have had the support of many other economists in the field of industrial organization who have always had a great deal to say about the limitations of the neoclassical model in explaining firm behaviour. There have even been a few outright revolutionaries in the profession, such as Nicolas Georgescu-Rogan, Albert Hirschman, Janos Kornai and Charles E. Lindblom who have not been totally shorn of reputation and status in economic circles despite their revolutionary ideas.

Moreover, the terrain occupied by economists has been substantially enlarged in the past few decades by the efforts of some economists specialized in industrial organization to apply game theory to the interactions of business firms. Models of this genre begin with the recognition that, in an effort to maximize their returns, rival firms have a choice of cooperating with one another or of acting autonomously. The well-known model of the “prisoner’s dilemma” epitomizes such approaches, a model reflecting the decision processes of a pair of rogues who are being quizzed by the police in separate cells; each then must think through whether to risk a strategy of cooperating with the other by maintaining a joint silence, or whether to defect from the partnership by spilling the beans.

These models, which have grown richer and more varied with the years, do not directly challenge the assumptions of the neoclassical model or the conclusions to be drawn from the use of the model. Like the neoclassical economists, the game theorists are typically concerned with defining a state of equilibrium. Indeed, the models offer some comfort to the neoclassical theorist by demonstrating that, in a prolonged series of games, the participants will conclude that cooperation is the superior strategy; so the problem of complex interactions between rival firms fades away. Still, by asking a set of questions about firm behaviour which the neoclassical model does not raise, namely, the strategies to be chosen by the market participants, game theory opens up a door to the analysis of TNC behaviour that has barely begun to be exploited.

Explaining the behaviour of transnational corporations

Why have economists been so slow to take up the challenge of explaining the strategies of TNCs? Charles E. Lindblom observed sagely that “. . . the boundaries of economics have been worked out with careful attention to those tasks to which theory can be applied. . .” (Lindblom, 1957, p. 253). Except perhaps for the latter-day developments in game theory, it has proved extraordinarily difficult to apply the existing body of economic theory to the behaviour of TNCs.

To be sure, Ronald Coase, a Nobel laureate in economics, gained distinction by exploring one question central to the existence of the TNCs, namely: why do enterprises often internalize transactions that could conceivably have been consummated between independent parties in an open market? His answer to that question was straightforward: to escape the costs that exist when dealing in inefficient markets. But the importance of Coase’s work to most economists—and, indeed, to Coase himself—was that the existence of an internalization option also offered economists a way of escaping a problem of their own, namely, how to deal with the theoretical problems posed by the existence of these inefficient markets. If business managers internalize the transactions that they would otherwise be required to undertake in inefficient markets, then it can be assumed that the markets which enterprises use are, as a rule, sufficiently efficient. Transnational corporations, therefore, can be seen as playing the role of the “medicinal leech”—cleansing the international business environment of markets that are less than efficient.

Ahead of his time in 1937, Coase was performing a critical function that would make it easier for economists to marginalize TNCs in their thinking. Even among that subset of economists who thought of themselves as specialists in industrial organization, only a very few (notably Richard E. Caves) had much to say about TNCs. Most targeted their efforts on amplifying and extending the neoclassical model of market behaviour; and many of them soon recognized that the complexity of incorporating TNCs in that model was utterly daunting. So the pursuit of the underlying principles associated with the behaviour of TNCs was left to the occasional aberrant.

For several decades after 1950, the trends in mainstream economics pitched the odds heavily against any effort to expand the circle of such aberrants. As many observers have noted, economics gradually narrowed its focus in those decades so that an exploration of the properties and potentials of the neoclassical model became even more dominant. The profession's Holy Grail was to produce an explanation of the economic behaviour of society in terms consistent with the model, albeit obscured from time to time by "imperfections", "externalities" and "inefficiencies" affecting the behaviour of the market.

This leads to a critical point, namely, the fundamental distinction between the focus of economists, who build their work on the exploration of the properties of the neoclassical model, and the focus of those who try to understand the business behaviour of TNCs. Those who concentrate on the neoclassical model are usually trying to understand the behaviour of markets in relation to an assumed equilibrium; those who concentrate on TNCs are usually trying to understand the behaviour of firms, whether in relation to an equilibrium or otherwise.

It is instructive to observe what this difference in perspective means in the context of TNCs. These corporations are principally found in oligopolistic markets with substantial barriers to entry. The barriers take a variety of forms, including those created by economies of scale and scope, by learning curves and by proprietary technology and trade marks. For the neoclassical economist, bent on explaining the market's behaviour, each of these barriers creates an opportunity for a seller to capture a monopoly rent in the next transaction; with that monopoly power taken into account, the transaction can be adequately modelled. There was a time, not long ago, when neoclassical economists typically dealt with that problem simply by belittling it, that is, by contending that monopoly power was of no great significance in the real world. But, eventually, such reactions have given way to a recognition—often a reluctant recognition—of the pervasive importance of that factor.

The names of Antoine Cournot, John Nash and Heinrich von Stackelberg are associated with one set of models pitched at dealing with monopoly power. These models assume a market composed of a limited number of sellers, that is, an oligopoly. In their more elaborated form, they assume that each oligopolist, when fixing the level of its sales, analyses not only its own situation, but also that of every other oligopolist, making the assumption that each intends to maximize its revenue in the next sales period. That calculation, undertaken by each of the oligopolists and reflecting a pooling of their joint monopoly strength, produces a market in equilibrium.

Those of us who are interested in the behaviour of TNCs, however, have only a passing interest in modelling the shape of a market in equilibrium. Moreover, we tend to see oligopolistic markets in terms that differ substantially from models of either the monopoly markets or the competitive markets that are at the heart of neoclassical and game-theoretic models.

The difference in the two approaches is easily described, but its implications are profound. In oligopoly markets, the growth and survival of any firm depends on its ability to wage a campaign against known rivals, attacking or defending as necessary. Like the game theorists, it is recognized that oligopolists may have a choice of cooperation and warfare. And, freed from exploring the characteristics of a market in equilibrium, we assume that the campaign will be waged in a world in which equilibrium is never achieved, as changing "imperfections" in the market persist in shaping its behaviour. So, as instructors in business schools constantly remind their students, the strategy to be chosen must be based on a thorough understanding of other oligopolists' capacities to attack and defend, all considered in a dynamic external environment. The appropriate model describing oligopolistic behaviour, therefore, is not one whose object is to maximize the yield from each individual transaction, but one whose object is to conduct a successful campaign. Such campaigns encompass possibilities of market interactions of the most complex kind, including substantial deviations from profit-maximizing behaviour over the short run.

None of this has escaped wholly the attention of economists. But the technical complexity of recognizing these factors in formal models consistent with the neoclassical structure has been forbidding. Nevertheless, by the late 1970s, some economists were already beginning to make the attempt. By that time, it had become strikingly evident that neoclassical theory was of no help in explaining the flows of foreign direct investment (FDI) which accompanied the creation of TNCs. For instance, while United States-based firms were expanding their stakes in Europe, Europe-based firms in the same in-

dustries were increasing their commitments in the United States, a development that neoclassical theory could hardly have suggested. Economists in the field felt some pressure to protect their dominant economic doctrine from its apparent nakedness.

At that point, by adopting Cournot-Nash assumptions about oligopoly behaviour, a few creative economists mastered the mathematics that would permit them to integrate scale economies into the neoclassical models for international trade. Conforming to Lindblom's Law, therefore, economists at once began to acknowledge the importance of scale economies in international trade and began cautiously to develop models that purported to capture the influence of such scale effects. In that process, they were moving a small step closer to exploring the strategies of TNCs.

For my part, I have substantial doubts whether models that incorporate the Cournot-Nash assumptions about the behaviour of oligopolists can carry us very far in our attempts to model the behaviour of TNCs in real life. Implausible enough in oligopolies that exist in a single national market, the Cournot-Nash assumptions seem even more strained for oligopolies whose principal actors have been spread across the globe. But we have at least crossed an important threshold; and it could well be that increased interactions between the neoclassical theorists and the game theorists will generate insights helpful to those of us who are intent on understanding the strategies of TNCs.

So far, however, the neoclassical wing dominates in the interaction. And, true to its training, that wing usually finds it easier to scan the behaviour of countries than the behaviour of firms. Besides, because data on the behaviour of individual firms are so limited, it is far easier to study countries (or industries within countries) as the unit of analysis than to deal with the behaviour of individual firms. But contributions to an understanding of the behaviour of TNCs will be limited until the focus shifts to the study of the firm.

Filling the gap

Economists who are responsible for teaching and enriching microeconomics as a discipline have a much easier task in many ways than economists who are responsible for adding to the capabilities and understanding of putative business managers or government officials operating in the real world. Economists with their focus on economics as a discipline can often

afford to assume the existence of efficient markets; indeed, when they fail to do so, their peers are likely to demand that they justify the departure. Economists who hope to guide business managers or government officials, however, run substantial risks when they ignore the "imperfections" in existing markets or disregard the intimate links between the structure of the enterprise and the strategy it pursues in its perennial battle with known adversaries.

Of course, anyone who claims to be contributing to scholarship, irrespective of the setting in which that person operates, is never relieved of the obligation to look for consistency with the available evidence and to pay appropriate respect to the laws of logic. When analysing the strategy of firms, however, neither of these requirements makes it obligatory to favour neo-classical theory as the departure point in the analysis.

When thumbing through contemporary journals that deal with international business, I often wonder whether the economists among us are taking full advantage of the freedom we have in this regard, and the extent to which we can deviate from the strictures to which our discipline-oriented brethren are held. Some of the factors that make such departures difficult are, of course, obvious. One is training; schooled in a set of well-developed concepts that are not irrelevant to our needs, economists naturally turn to them for a first approximation to an appropriate response. That tendency explains why economists in business schools are so persistently drawn to internalization theories as their preferred explanations of the existence of TNCs, while slighting theories that emphasize risk-hedging and firm-to-firm warfare as the key motivations. Whereas the internalization motive allows the economist to remain in the comfortable cocoon of a neoclassical world, the motives of risk reduction and head-to-head warfare are not quite so accommodating.

Still another motive that keeps economists in business schools earth-bound is the desire to retain their valued status as economists. This is a particularly important consideration for young faculty members in business schools, who often are unsure if their business-school affiliation will, in the end, provide a path to the security of tenure.

Finally, there is the problem of securing the appropriate data for analysis. Governments are the source of most of the descriptive data that provide wide coverage over the activities of TNCs. As a rule, these data are provided for a given country by broad categories, thus preserving the anonymity of individual firms and offering only hints of the struggles among them over time. Data of this kind are often sufficient for testing hypotheses about the behav-

our of a national market, but rarely adequate for testing hypotheses about the interaction among oligopolists in their extended battles. For studies of the latter, one has to fall back on the occasional industry study generated by specialized government agencies, or has to engage in the difficult and expensive process of building up the desired firm-by-firm data from scratch. To be sure, the possibilities for assembling relevant firm-by-firm data are increasing and the relevant costs declining as Internet extends its reach; but the relative ease of obtaining the requisite data still propels researchers in the direction of hypotheses about the behaviour of national markets rather than the behaviour of firms.

The tendency of economists in business schools to stick to the issues that will best preserve their professional credentials is visible, too, in the methodologies they tend to adopt. Regressions, complete with tests of significance, are *de rigueur*. From the viewpoint of the discipline-oriented economists, unconcerned about the application of their discoveries by business firms or government agencies, anything is germane that survives the econometrician's test of significance, a test purporting to establish the probability that observed relationships between variables was not the result of pure sampling error. Such tests are widely abused and misinterpreted, being credited with far more power to test an hypothesis than they are capable of providing; but that problem is common to many users of such tests today, not only to those in business-school faculties.

In the clinical context such as confronts the faculty of a business school or a school of public policy, "significance" has an entirely different meaning from "significance" for the econometrician. For the clinician, relationships are "significant" only if they are strong enough to affect firm behaviour with a force and in a manner that requires them to be taken into account in the formulation of business strategy or public policy. One may be able to demonstrate that the mean winter temperature will affect the activity of Japanese business managers searching for sites in Minnesota; but if temperature variation explains only 2 per cent of the variation in that activity, it is of no practical relevance either to the Japanese manager or to the State of Minnesota.

Finally, I am a trifle repelled by the formalism we impose on our young colleagues in the presentation of their findings to the profession, typically demanding that they present an hypothesis, to be followed by an effort to falsify in the form of a statistical test. In reality, while presenting their results in a form that is consistent with such a sequence, researchers commonly formulate their hypotheses from the very data that purports to provide the ba-

sis for a test; so no independent testing in fact takes place. This does not mean that such work is without value if properly used. The point that is overlooked or dismissed is that the formulation of an hypothesis may prove to be the most creative part of the exercise, and in the end its most valuable product. Provocative hypotheses can, for instance, be valuable for clinicians when they have nothing more solid to guide them, particularly if the hypothesis resonates with their own experience. Recognizing the creativity of hypothesis-building as an indispensable activity in the process of learning about relationships, there is room for a much more varied output in business-school journals than what currently turns up in many such journal. I can see hypotheses about the behaviour of TNCs falling out of some of the models that game theorists have developed in other contexts. I can see well developed accounts of the tactics and strategies of actual TNCs leading to generalizations that authors are prepared to entertain for further study. Whether scholars trained in economics will be ready to provide such articles I cannot say. But at this early stage in the study of TNCs, the field would be much advanced if they did.

Moreover, in an effort to understand the behaviour of TNCs from the viewpoint of business managers or public policy makers, there is a need for much more emphasis on longitudinal data covering individual firms. All firms, of course, operate under the heavy influence of their own histories, which affects their structure, their culture and their perceptions of opportunity and risk. But the changes in their behaviour seem to me to be critical in understanding their strategies. This is hardly a new thought; that is what economic historians have been telling us in a century of writing. But perhaps it is time for the economists among us to take their lessons more seriously. ■

Reference

Lindblom, Charles E. (1957). "In praise of political science", *World Politics*, 9, 2 (January), pp. 240-253.