
COMMUNICATIONS

THE NEGATION OF SOCIOLOGY AND ITS PROMISE: Perspectives of Social Science in Latin America Today

Orlando Fals Borda

During the last decade, there has been a great debate on the social role and political function of the social sciences. This debate has brought into focus certain types of research that had not been considered valid before, each with its respective theoretical framework. As I see them, the most interesting and productive of these new perspectives for the social sciences in Latin America today are (1) modesty in research, (2) primacy of the qualitative, (3) autonomous development of theoretical models, (4) interdisciplinary research, and (5) broader acceptance of individual action and commitment as validating elements for research. None of these is strictly new: on the contrary, some of them (e.g., 5) are cyclical and have quite respectable ancestors; some have been presented by other colleagues, and myself, in many places and at many times. Nevertheless, they are worthy of repetition in view of their considerable implications for research policy and social action.

MODESTY IN RESEARCH

What is modest is not necessarily insignificant, second class, or inept. Many modest investigations have led to momentous discoveries: the earth's gravity, the circulation of the blood, X-rays. Exceptional researchers such as Marx, LePlay, Fanon, and Mills made full use of the best and most economical of social research instruments: the human brain. When a modern researcher conceives his

work mainly on the basis of overblown budgets and sophisticated computers, he may go beyond the economic, ideological, and practical limits of underdeveloped countries. Neither research elephantiasis nor data accumulation has shed much light on basic contemporary problems, and when these superprojects are implemented indiscriminately in our poor countries, or are converted into multinational projects, the result often reflects the intellectual chains of cultural and economic dependence. We are shackled to wealthy colleagues from abroad and subjected to their ideas. These projects serve their bearers and institutions well; the same cannot be said for us and our people. We cannot criticize foreigners for acting in their own interests, but it is our fault when we let ourselves be dazzled by the formulae, the numbers, and the money.

Our social and economic problems are so self-evident that they may not need any precise statistical confirmation, nor complicated surveys and long schedules, nor sophisticated sampling designs to be understood. Social scientists should not compete with mathematicians and natural scientists; they deal with another kind of matter and universe. We should concern ourselves with the open, changeable, relative, and conjunctural systems that make up the world of the social—the analysis of which requires, above all, keen observation, the power of theoretical inference, and, perhaps, a good dose of common sense.

Fortunately, the genetic distribution of intelligence is not biased so that “geniuses” are found only in the temperate zone. A good use of our own resources, no matter how limited, could be fruitful in the same measure as our intellectual colonialism is diminished. Then, with our real strength, which is not negligible, we could promote projects of our own choosing and under local control.

PRIMACY OF THE QUALITATIVE

As a consequence, it can be seen how important it is to undertake in-depth qualitative studies rather than diffuse quantitative ones. It is not my intention to undervalue statistics and mathematics, nor do I propose to eliminate them from the classroom; but we should place the measurable and quantifiable in their proper perspective as evidence, not final proof, of the existence of things.

We should not expect that the mere accumulation of quantitative data will be transformed automatically into qualitative knowledge. An immense power of abstraction is necessary to convert one into the other, and success is not always assured: in the United States, social scientists are still trying to take this step after a half century of micro and macro sociological research. Few observers would deny that what is needed in our countries is information to help us answer ontological questions such as: Who are we? Whence do we come? Whither are we going? The answers to these questions are not quantifiable. We must delve into our realities, the texture, taste, and smell of our traditions, the why of our cultural values, the structures of our personalities with all their qualities and defects. There are greater benefits to be derived from a well-constructed case study than from a hundred superficial statistical tables.

Thus, it is better to construct a science of reality, derived from direct personal experience, contact with real people, and fieldwork, to give flesh and bones to cold calculations. In essence, this is what counts; otherwise we would create an esoteric discipline for elite scholars increasingly separated from reality. There should be no more ivory towers. These refuges—or escape tunnels—are built and inhabited whenever science becomes a thing unto itself, with all the dangers thus implied for the alienation of scientists and the warping of their role in society. Thus, if our purpose is to understand society and not only to count and to describe it, we must give weight to values in our scientific output; the qualitative is only a necessary step in our development, allowing us to make intelligent decisions regarding scientific policy and our proposed goals as people and nations.

IMPORTANCE OF INTELLECTUAL INDEPENDENCE

We must develop our own theoretical models to interpret correctly and coherently the problems of our society. This does not imply chauvinism and it does not deny the universality of science. It recognizes that many of the sociological schools and theories that fed our intellectual development were conceived under different concrete historical and cultural conditions. The most obvious case is the transplant of functionalist positivism to our academy. Positivism was a scientific and ideological justification for European liberalism and the recovery of power by the French aristocracy. It also justified the political organization and the constitutions of the newly formed American states during the nineteenth century. Today, it serves to identify transformations within a social continuity, to justify unstable equilibriums (or stable disequilibriums) in capitalist societies. How could this school be applied to societies in intense transition, such as ours in the Third World, where instability, conflict, and open class struggle are an everyday occurrence?

It is even more pertinent to note the reaction of our scientists to the failure of that school, and their rapprochement with Marxism, which today furnishes a common scientific language for most of us. This is relevant because Marxism is also an imported school—with its origins in different historical and social circumstances—and for similar reasons, we should not accept it in a dogmatic fashion. Dogmatic attitudes have in fact brought Marxism to its present crisis, both in Europe and elsewhere. However, Marxist thought has evident advantages for us. It recognizes cultural relativity and provides leeway in verifying and constructing theory through general concepts (e.g., social formation, social relations of production, etc.) that have meaning and make sense only in concrete contexts. The theoretical-political problem of defining modes of production also permits the construction of flexible schemes of explanation within the great conceptual frame of reference of Marxism.

These trends toward becoming free from dogma and the intellectually given, toward casting doubt on premises, toward confronting mature concepts with reality are, of course, the foundation of all scientific advances. In fact, it is

because of our response to these challenges that certain intellectual efforts of Latin Americans are now gaining universal recognition. There are European philosophers who claim that current Latin American contributions to this field are more productive for general theory than the decadent Marxist self-criticism sometimes found in European socialist countries. The respect with which works of Latin American social scientists are received in Europe and elsewhere is evidenced by the increased number of translations of monographs from Spanish and Portuguese into German, English, and French. A few years ago, this was not the case. We should continue building autonomous systems of explanation and action. We count on Marxism for tactical heuristic support, but it should be questioned, when it does not correspond to our reality, amplified, and made more pertinent.

THE NEED TO INTEGRATE DISCIPLINES

In order to achieve the goal of intellectual autonomy, it is necessary to face the artificial division of contemporary social science that sets apart sociology from anthropology, economics, history, psychology, geography, and political science. This is another inheritance from the Old World: the mania of overspecialization. During the last World Congress of Sociology (Uppsala, Sweden, August 1978) there were more than two hundred working groups and committees, and one plenary session was devoted to studying the "limits of specialization." In societies such as ours, with their enormous problems so highly visible, such division is not justified. On the contrary, we need to combine and integrate theories, methods, and techniques of research to advance our real and practical knowledge of the problems that we see and feel.

The difficulties of reaching this goal are exasperating and evident: above all, there are the vested interests of academic departments in universities, all justifying their existence on the basis of traditional divisions. Why could we not conceive our work on the basis of important concrete problems and build interdisciplinary teams focused on them? Exploitation, violence, alienation, ecological control, the implications of technical development—these problems are at the foundation of human survival on our planet, and they cannot be sidestepped. An earnest study of any one of these great problems will show that it goes beyond the reach of any separate discipline.

We must not simply add new disciplines, theories, and special techniques leading to a general scientific superstructure. On the contrary, the sanctity of individual disciplines must be subordinated so that new concepts and theories more adequate for understanding the phenomena will emerge. It is not possible to postulate definitive solutions or answers to social problems, because this field is subjected to a permanent, natural evolutionary process. The sooner we recognize this, the more productive and useful we shall be to our societies as scientists. We need to discard prejudices, abandon personal provincialism, and give up the disciplinary imperialism of our universities. We need to create one "critical social science" similar to that formed during the nineteenth century, before specialization became fashionable. It may be something similar to the "political

economy" of Quesnay, Marx, and many of their contemporaries, but enriched by the research and techniques of the last century, with new possibilities in the practical and theoretical realms.

ACTION AND COMMITMENT AS VALIDATING ELEMENTS FOR RESEARCH

If the above perspectives are accepted as worthy of pursuit, we should begin to work with new scientific strategies. There are signs that an alternative paradigm related to Kuhn's "extraordinary science" is taking shape gradually, and the catalytic agent appears to be "praxis"—understood as the dialectical combination of theory and practice in which practice is the determining factor. We should add to this concept the well-known theses on the personal commitment of scientists. This critical position on praxis is inspired by Marx's Eleventh Thesis on Feuerbach, and it is adopted for the purpose of gaining knowledge to transform society through practice in specific contexts. And not just any transformation: indeed, it must be one that satisfies the interests of those groups that are exploited by capital, the victims of bourgeois development, classes that create wealth which is then expropriated by profit-seeking entrepreneurs and intermediaries. From such an effort to understand this transformation and to participate in it there could arise not only a new society but also a new critical social science—disconnected from the interests of the bourgeoisie that happened to be its first midwife, and linked instead to the interests of the growing proletariat.

A methodological tool well suited to this task is being developed independently in our countries: action research for radical change. It does not appear to be a new paradigm, as some scholars maintain, because the frontiers between this method and others already employed in the sociological tradition are not clear cut; besides, action research has been used by Marxists since the last century. We know that its specificity lies more on the teleological level, and that it takes into account the incidence of values in research more than other sociological methods. Action research requires the scholar to take sides openly on real political issues and to state the why, the how, and the for whom of research objectives. It includes in the research task the powerful ingredient of social purpose and rejects that which is done mainly with academic goals in mind, or as an exercise in pure science, or for simple personal advancement. It respects popular knowledge as an adequate authority in specific fields that require theoretical articulation only to understand their structural dimension and to utilize their practical potential. In these cases, the validity of acquired knowledge is judged not only by reference to the universal store of concepts and theories already proven, but also by the concrete development of practice related to this knowledge.

Radical action research was postulated theoretically (and put into practice) at least by the middle of the nineteenth century, but its acceptance has been rather cyclical. Its current re-entry to the scientific stage, especially in Third World countries, is not pure chance. It is responding to the need of a certain type of scientific knowledge committed to certain social classes, in order to accelerate processes of structural and revolutionary change. European and North Ameri-

can colleagues have lagged behind us in this field, despite the fact that these problems will affect them sooner or later because of the increasing interdependence of our contemporary world.

It is perhaps too much to ask that universities incorporate formally in their academic programs concepts such as praxis and commitment, in spite of the fact that they are so strategic and important for our disciplines. The universities will not be able to advance much in this direction until their social and political milieu is likewise transformed. Nevertheless, students and professors can light the spark to produce such internal change, in turn leading to recognition of these new perspectives and scientific possibilities. So I end with a cordial invitation to rethink our role as social scientists. There is much at stake in terms of personal satisfaction and collective redemption. Of course, sociologists cannot aspire to be philosopher-kings. But we could help justify our existence—our lives—if we contribute to the creation of a critical social science that is solid and serious and appropriate for those resurgent social groups and classes that are in need of it.