

Problems of Sociological Method

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I DON'T intend here to investigate objectively either the influence that American sociology has exercised on French sociology for the past thirty years or the influence that American sociologists have had on me. For my part, I relocate myself constantly vis-à-vis American sociology and its methods, as is obvious in most of my writings, especially *La Propagande*. I would prefer to deal with two related problems, both to be found in American and French sociology: that of the scientific status of sociology, and that of verification.

Sociology as an Exact Science

Is it possible to treat sociology as an exact science, and to use concepts and methods deriving directly from the exact sciences? To be sure, some people have thought to do so by using mathematical methods, and science is characterized by mathematical exactitude. They have also used concepts drawn from physics or chemistry, and attributed to their own units of thought various designations common in physics (for example, force, field, etc.). But this is merely a formal device which hardly makes sociological thought more scientific; it is the outward appearance of science linked to vocabulary, and it fails to account for societal reality or even to interpret its functioning. It is merely the artificial superimposition of a system of concepts taken from physics onto a sociological "object" without the preliminary demonstration of

any identity between this object and the "matter" that physicists study.

The first difficulty arises from the presence of the sociologist himself. The physicist and the astronomer can forget their ideas, conceptions, and family life in conducting their investigations. But for the sociologist, ideas and values are at once a reflection and a part of the very society under scrutiny, and his professional and family life is precisely *what he is studying*. Usually he can't help but have some sort of conjugal relationship and take some sort of political stance. It has often been said that sociology should be independent of sociologists, or even that it should be carried out despite sociologists, in sum, that the very existence of the sociologist is contrary to the premises of sociology. But what is meant here? That sociology constantly critiques its own axioms, wary of any bias introduced by personal preconceptions? So much is obvious. Or is it that the sociologist should be absent from sociology? This, I believe, is both a dream and a mistake: a dream, because whatever the place of computers, data banks, or methodologies, the sociologist remains the only being who can pose problems and offer interpretations with human understanding. This he must do through the filter of his personality. It is also a mistake, because the sociologist is the first witness of the experiences of his society. If he does not examine in and for himself the meaning of such and such a social movement, if he does not take himself as the first object of his observation, he is neglecting a major part of his work. He is not an independent observer; he is not a man watching an avalanche. He is a man observing human phenomena, in which he is involved willy-nilly.

The first wisdom of such an observer is to admit that he is involved. Then he can take the first step toward, if not objectivity, at least scientific honesty. He himself sustains the effects of the forces that he examines; he shares the beliefs that he is trying to elucidate; he belongs to the social relationships that he wants to understand. All this he learns to see in himself—if he is honest. Which obviously doesn't mean that he can generalize

from his individual experience and pass directly from introspection to sociological comprehension! Rather he must take his experience into account within the totality of his data, and avoid justifying himself by claiming that he is independent of the system. The first step toward sociological lucidity is to know that one belongs to a specific political system and that what one advances will *necessarily* be integrated by and into that system. Here is the great distinction between sociology and physics, chemistry, and biology, where such utilization is merely *possible*.

Yet it cannot be otherwise. The ideology of a purely scientific sociology erected against the sociologist is an excuse for avoiding the problematality of sociological axioms. Such an ideology gained currency when sociology was based essentially on the knowledge of "primitive" peoples. In the late nineteenth and early twentieth centuries men thought that an observer of such peoples could be perfectly objective because of his exteriority. He could observe a Tonga group as he might an anthep. The interpreter in Paris or Rome to whom he relayed his data could also remain detached, serene and unprejudiced. But such convictions did not stand up to close investigation. For the European observer was studying phenomena that he knew to be *human*, and bore within himself a whole complex of ontological appreciations. He had a certain conception of man which he couldn't help projecting.

This situation has become aggravated with the advent of contemporary sociology. Now no excuses are acceptable. Indeed, the presuppositions of our sociology must be scrutinized precisely in order to render its attitude explicit. In *La Propagande* I criticized certain American sociologists whose views on propaganda are directly inspired by their own democratic beliefs. It is "impossible," they claim, that propaganda is X, for then democracy itself would be impossible! Of course their concern was never voiced, but it was an implicit epistemological underpinning. The same goes for certain scholars for whom the class struggle, as an irreducible axiom, leads to an entire sociology—without the assumption about class struggle ever being openly stated.

What seems most pernicious to me right now is the behaviorist assumption. One of the obstacles to the transformation of sociology into an exact science is that it concerns human phenomena. One might recall what Marx said about the English laissez-faire political economists: While granting the scientific character of their economic studies, he also showed that they had obtained their results only by deleting the human factor, thus reducing economics to an interplay of forces and tensions, and ignoring the reality of man; or rather, in remodeling man into a *homo oeconomicus* so that he could play exactly the role required of him by English liberal economics. The problem, he said, was to reintegrate man in his totality, with his ambiguities, sufferings, and hopes, without sacrificing the rigor of already established economic thought.

Presently we are watching a repeat of this phenomenon. To attain a scientific sociology expressed in terms of structures, systems, functions, fields, strategies, mechanisms, territories, etc., certain researchers have eliminated the aleatory human factor and the uncertainty which the presence of man introduces into all calculations and predictions. Exactly like the English liberals, they nominally retain him while reducing him to a system and an interplay of forces (stimulus-response—exactly the same model as *homo oeconomicus*). Such is the thrust of behaviorism, as exemplified by, say, Skinner. I daresay that the behaviorists merely respond to the stimulus of the sociologists (though I know that this isn't true!), who want to be provided with an interpretation of man without any uncertain factors and thus utilizable in any rigorously scientific clarification of social movements.

As for the sociologist, he doesn't want to admit to himself that he uses this model of man; he tucks it away in the depths of his mind, he fights off any attempt to render it explicit. Yet he constructs his sociology as if it were obvious that man really is like the model proposed by behaviorism. To be sure, there can be no science without the elimination of the aleatory factor (of course I'm not referring to probability theory or quantum me-

chanics), and especially all that is unpredictable. I'm not arguing that behaviorism is unscientific, merely that certain assumptions inhabit the sociologist's brain (and he always unconsciously chooses those which facilitate his work) and render the most rigorous objectivity essentially subjective.

But there are still other problems with this wish to treat sociology as an exact science. The last thirty years have seen a certain evolution with regard to the subject of sociology. Broadly speaking, we might say that sociology has shifted from the treatment of "objects" to the treatment of "changes" or actions. Social facts were once considered as things (Monnerot was already protesting in 1950 that "social facts are not things"). There was a certain fixation on the social object; it was treated like an object in mechanics or anatomy, its components analyzed, its typology set forth, its infrastructure and superstructure posited. The most complete of these exercises was that of Gurvitch, and it was not without merit. But this methodology seems to have passed. Nowadays, it's no longer fluctuations that are studied, or tensions, or the balance of forces, or social action, or conflict analysis. Nowadays we take exception to the notion of society as a sort of thing and prefer an essentially active and self-generating view of society. We're looking for a methodology of change.

Yet we mustn't deceive ourselves. We're not concerned only with the deepening and improving of our knowledge, and it's not that we've found some new "object" to study. No, what has happened is more profound: We're after another conception of society (that is, the globally societal) and its composition. (We might say that society is not a pile of "strata" but a play of forces—a different preconception.) We're after a new and different way of interpreting phenomena, certain of which were already known (we might say that we want a new set of paradigms), and this entails a change in sociological method itself. There is no similarity between object analysis and functional or strategic analysis. The scientific method for apprehending reality has changed.

But we wonder why this change has occurred. Is it simply that sociologists understand things better now, and so have adopted a subtler, more adequate method? Of course not. It's that the things themselves have changed! Until 1940 it was plausible to conceive of society as a stable and well-organized whole. Naturally there were upheavals, but they were accidental ones; there were changes, but they took place within larger, stable structures. This conception corresponded to a certain reality: The sociologist still inhabited a stable world and interpreted it as such, with a method adequate to its stability. For forty years, though, we've been living in a world of continual flux. The evidence suggests that society no longer has permanence, that it is subject to accelerated mutations, and that hence it must be seen no longer as an object but rather as a continual transformation. This new reality requires a new method. Thus method changes with the apparent object.

One might object that this is perfectly true, but only in the sense that in physics, for example, one must change method when one studies widely divergent orders of magnitude or scale. But really the problem is not at all the same. For the apparent change in the sociological object does not result from a different or closer look at the same object. No, it's history which has rendered the earlier societal object obsolete and which has changed what the sociologist observes. Moreover, this shift of method is in no way the expression of the sociologist's sovereign initiative. It merely results from the social reality in which the sociologist participates and in which his method is eventually inserted. The method is part of the society in question; it is itself a sociological phenomenon. In a social body where change is obvious to all, where it is requisite as an axiom for all thought and interpretation (and where, indeed, it carries a positive connotation), not only the object of study changes but also the method, which becomes the rigorous scientific explanation of the ideology of change.

Incidentally, one might say that all this is rendered problematic by the observation that we are presently witnessing the devel-

opment of two concepts: system and structure. Each offers a different answer. Now, everyone knows that the only finality in the construction of a system is the knowledge of how it *functions*. It's not an object, it's not a description of reality, it's an artificial construction designed to account not for the interaction of the *real social pieces* but for the *real interaction* produced in society. The notion of system returns us to the problem of development. As for the idea of structure, and structuralism itself, the matter is quite different: Structuralism can deal only with traditional societies, that is, those in which change is slow and which may be compared to the historical societies originally studied by sociologists. In other words, this is a scientific method which, retaining object analysis, proves incapable of dealing with current societies of the technological type. Structuralism cannot be correctly used to study our Western society.

What, then, is this new scientific method which continually changes? Is it the sociologist picking a new method every time society alters? No, what happens is that the method has appeared as an outgrowth of the society itself. And this has given rise to another criticism of social "science," namely, that it continues to function as though its subject, society, always preserves its self-identity. Indeed, it is amazing to hear modern sociologists speaking en masse of "society" or "groups" as if there were any such things-in-themselves, as if society were always . . . society. Such is the vexing impression one gets on reading certain highly abstract studies, like those of Talcott Parsons. One can't help asking oneself: Just what is this analysis being applied to? Is it valuable as a whole, and in general, or only for the United States? In other words, is the sociologist, in escaping from personal involvement, not beginning to construct a certain fictive society which he takes as the object of his study (and I'm not talking about the elaboration of a model—the process I'm describing is involuntary) and finding there certain constants which give him the impression that he is being scientific, though he himself has produced his fiction from those very constants? Taking an

object that he deems forever the same, he introduces variations (according to what he notices around him or in history) from which he derives a typology. This is what is so deceptive in Max Weber. For a typology always has the weakness of deriving from a societal whole some given to which value is then attributed by comparing it to an identical given in another societal whole far off in time or space. From this identification is obtained an "ideal type." But this undertaking is always hazardous in that it leads one to remove a phenomenon from its context, though phenomena really derive their meaning from context.

Lastly, we must ask ourselves if sociological "science" is really similar to the other sciences. We have seen how it was obliged to change methods because the sociologist, following the movement of society, had noticed another aspect of the world in which he lived, namely, change. Here precisely lies the problem: How certain is this change? It is obvious that superficially at least we are witnessing plenty of changes. But I've pointed out elsewhere that beneath these changes there has been a fundamental continuity from the eighteenth century to our day, and even a great rigidity: It is the technological system which insures this continuity. And if one puts oneself at the level of this "infrastructure" one sees that the phenomena of change are relatively secondary and that nothing fundamental has been questioned. Put another way, it is perhaps not so legitimate to change method and concepts. The only progress that seems to me to have been achieved here is the notion of system, for it covers at once the fundamental structure and its specific role. To sum up, the person of the sociologist is so dominant in sociology that it is better to recognize the fact and cope with it than to claim to be able to elaborate an objective and independent science.

Global Sociology

Admittedly, there is a method—perhaps statistical or stochastic, certainly mathematical—which might be rigorous and indepen-

dent, and whose application the personality of the sociologist couldn't affect. So much is undeniable. But I must point out that contrary to popular opinion this method can be used only in microsociology. Of course to apply a statistical method one must have large numbers, but "large numbers" in multiple experiments does not necessarily imply large societal units. One may have large numbers concerning small social units. Additionally, we know how important the determination of the unit of study is for true precision. One studies "the family," for example, as a function of several predetermined concepts. However, the mathematical method can be very fruitful. Yet it entails an immediate peril: One becomes attached to this method because it is the only really scientific one, and ends by deciding that anything this method can't handle isn't available to science. In other words, one may ignore fields which are important solely because one can't apply the mathematical method to them. In France there has been a debate over just this in political science. Certain scholars have limited their study to political parties. In this realm one can indeed gather statistics and use rather precise techniques of observation. Others have maintained that the phenomenon of big government and the growth of power in modern society were much more important than party politics. Yet in that realm no rigorous method is available. Hence the first camp has virtually won. Therefore the study of the state has been neglected, and those who make it their business are considered "amateurs" or "essayists," quite unlike true men of science.

Implicitly, then, one may find oneself bowing to the idea that whatever overreaches the mathematical method isn't science. Hence one risks giving preeminence to objects, groups, or movements that have only secondary importance simply because one can study them in a suitable way. Inversely, one leaves fundamental and decisive phenomena unstudied only because they cannot be expressed in mathematics. Thus it comes about that microsociology is favored over "global sociology." Still more recently, scholars have returned to the latter in hopes that the

use of models could objectify these global phenomena. But it is clear that this tendency needs revitalization. If we try to apply this method to vast and complex societal units, we find right away that we can't construct a satisfactory model. We can't integrate all the variables. I'm not alluding to any material difficulty in making the calculations—that can be partially overcome with computers—but to an analytical incapacity to discover what all the components are in the first place. I've often amused myself by noticing how many variables have been forgotten in quite complex models. I've always found plenty. Thus there can never be anything but the most approximate models, varying in accordance with the researcher's own interests and concerns.

All I'm saying is that a global sociology is necessarily conceptual and hypothetical. But this does not mean that it must be inexact. It is merely a different sort of science, "unlike the others." Besides, this research necessarily must forever remain in the stage of research, and it progresses in a rather unusual fashion. More than other disciplines, sociology progresses by debate, by continual discussion of earlier conclusions. One cannot conceive of an acquisition so scientifically founded that it couldn't be questioned, or that one could ever restrict oneself to progressing on the basis of indisputable facts. It is even inexact to say that a series of microsociological studies, however scientifically advanced, could ever result, through accumulation, in an understanding of global phenomena, of social classes for example. There is a sort of qualitative change that comes about that one can never take stock of.

Research consists in perceiving facts that are never exhaustively known or fully culled. Yet they remain necessarily typical. It is as facts that they are studied, and it is as such that they serve as a foundation. It is with them that one may construct an ideal type or model that will later serve to interpret a whole range of phenomena. Yet here again these phenomena cannot be perceived and recognized except through the creation of some sort of interpretive matrix. They have a real existence, but they may be taken

out of their social context only to the degree that the interpretive matrix allows one to grasp them. Hence one can understand the essential role played by the discussion of earlier researchers' conclusions. What's involved is an approach to reality through the quest for a new interpretive matrix, a new model. But at this level one cannot objectively tell what is and what isn't scientific. The sociology of Raymond Aron is certainly exact, but so is that of Castoriadis. And this leads us to the most disquieting question, that concerning the exactitude of sociological research.

Verification by Subsequent Events

I've said that the search for a global sociology is hypothetical. It starts from certain advanced hypotheses and leads to results which are considered equally hypothetical by those who do not share the author's convictions!

Can we escape this predicament? Is any scientific guarantee possible? There's an oft-repeated truism that in sociology and political science we cannot perform experiments to verify hypotheses. Of course certain people have claimed to have carried out sociological experimentation, for example to measure the effects of mass media or propaganda. But I've shown that such experiments were meaningless because they did not reproduce the real social conditions of the phenomena under study. In physics or chemistry we can experiment with a phenomenon by eliminating all fortuitous circumstances to observe it in its purity. But the same isn't true of sociology or political science. Here, the totality is the phenomenon.

We cannot study a social element in its pure state, for there is no such state in concrete reality. For example, we cannot measure the effect of a film by testing a group of spectators. Any film comes to the viewer as one in a series of films and other shows. So we would have to measure the effect of fifty successive films.

One violent movie, for instance, won't induce violent behavior, but when a viewer sees *only* violent movies for one or two years, and when these are themselves situated in a context of information about and discussion of violence, and when he himself witnesses occasional scenes of actual violence, the situation is quite different. This experience cannot be abstracted from life, for the societal process is *continual*; it cannot be arbitrarily halted at a given moment. A societal phenomenon can be understood only in correlation to a whole complex of related phenomena. For instance, it is useless to try to experiment with a certain type of propaganda, such as one of those used by the Nazis. One must *be* in Nazi society to know its real efficiency. The reproduction of a whole societal complex is impossible, and the accumulation of a multitude of microsociological experiments won't help us draw the slightest conclusion about macrostructure. All the sociologist can do is try to be there when the phenomenon occurs, experience it, analyze it, and afterward attempt to understand it. Since no experimentation and reproduction of phenomena are possible, hypotheses and conclusions can't be verified.

Some scientists think that the only guarantee of truth is methodological rigor. If one elaborates a rigorously scientific method, they say, with precise concepts and plentiful, unquestionable data, one must arrive at true conclusions. But my first point here was that such a method is theoretically impossible in sociology. Besides, most sociological concepts are ambiguous and uncertain, while the data, though increasingly plentiful, is open to question even when statistical. Nonetheless, certain sociologists reject these criticisms and continue to rely on methodological rigor to insure the scientific character of their investigations. Their studies are so abstract that—while the approach is indeed convincingly scientific—one never really knows what societal reality they are discussing. With them it is no longer a question of observable societal situations but of the transfiguration of sociology into an ideal world where actions and concepts can be pure and method-

ology absolute. But one remains at a loss to discover what enlightenment they provide for an understanding of social experience.

I myself have come to believe that the only criterion of exactitude for a sociological study comes from subsequent events themselves. This problem is posed directly by opinion polls. Do pre-election polls give a true picture of the voters' intentions? Of course the pollsters make a necessary distinction between pre-election intentions and the actual vote, which may take place a week or so later. They allow, for example, a 6- or 7-percent margin of error, which is quite reasonable. Now, this margin is wide enough to render most elections unpredictable; yet this fact, which is very important for politicians, is really of little account for sociologists. More important is categorization by age, sex, and professional group. Nonetheless, for a public-opinion poll the proof is in the pudding, that is, the election itself. The satisfactory results generally obtained in this field suggest that similar analyses of public opinion in areas where no experimental confirmation is possible are also valid. Here is a method which should be generalized.

Although there can usually be no rapid test, as there is in the case of elections, for a relatively long period (say, twenty-five years), one can state if the explicit predictions in a sociological description or system have been borne out or not. Note: I say "explicit" predictions, and some people would challenge me on this point. It's generally thought that there are two totally separate branches of sociology. The first is a sort of photo of the present, without any future dimension; the second is a prediction, comprising future variables, models for development, etc., and is arrived at with particular methods. But I hold that this division is erroneous in both methodology and interpretation. In effect, the sociologist is never content with a photo of the facts. Obviously, he interprets, he theorizes, and at times he attains a high level of abstraction. In this way he seeks to establish an intellectual

framework, a tool, which, using known facts, encompasses not only the past but also the future.

All sociological research is by nature historical, and cannot help using some historical methods. By this I'm not suggesting that it must return to ancient times, after the manner of Marx or Weber. But there is always a certain historical depth—even if only of twenty years—to any sociological study. This recovery of past time can take place only with the use of an historical method. Now, in just the same way no sociology can avoid inclusion of the future in its analyses. Every social phenomenon is an evolution. There are no "instant phenomena" as opposed to gradual changes. And gradual changes are always changes affecting *something*: groups become more or less coherent, etc. Earlier I mentioned the importance of certain shifts of field: how sociologists may deal with fluctuations, mutations, processes, and so on. All this implies the *future*. Of course this may become quite explicit, as when one tries to predict the most probable shape of the future. But that's not what concerns me here: for a study made without explicit reference to the future contains this dimension anyhow, often without the author's knowing it. In the end, no sociological theory is interesting unless it tries to comprehend events which are happening, and is more than purely retrospective. I use "comprehend" in the etymological sense of "taking with," that is, insuring that one's theory allows one to integrate new developments as they take place; and thus its exactitude may be verified. Possibly events will correspond to what is implicitly given in the theory. Then the theory will correctly explain or integrate them, and may be regarded as an adequate interpretation of reality. On the other hand, the facts might not fit, might require another explanation. In that case, the theory in question not only is wrong but *was* wrong, too. In other words, it did not correctly account for societal reality even at the moment of its construction, and so must be purely and simply rejected.

Of course this is a very pragmatic and rather crude criterion for the very abstract theories which I have alluded to, because

nothing concrete ever bears on them, and indeed one never really knows what societal reality they were referring to in the first place. But any theory that can evade confrontation with actual events must for the moment be held suspect. Unless an author agrees to state explicitly that he as a function of his theory expects X in the tertiary sector or Y in the relationship between academia and society at large, he must be regarded with strong doubt. Yet in fact sociologists rarely agree to make such predictions. In their eyes these are minor concerns, unworthy of a theorist's attention. But this future dimension is involved, I believe, in all sociological research. It is absolutely unavoidable. This is true simply because sociologists deal with human phenomena, and the human always includes the future. It is as a function of the future—whether explicitly or not—that one acts out a desire, or works on a project, or even lives. There is no group life or social life imaginable without this projection toward the future. So to claim to construct a sociological theory without accounting for this dimension is to fail to correspond to reality.

"To account for" means two things: on the one hand, a counting, a simple inventory of elements, as when a group takes stock of its future. No evaluation is implied here. But when that group is being "accounted for" or described by somebody else, when its factors are numbered and evaluated (and how could one help but evaluate them?), then this internal interaction requires that the sociologist present his own appreciation of the group's probable future. He cannot do otherwise, because he is dealing with human phenomena, and for him to restrict himself to a "snapshot" of the group at a given moment would actually render his study incorrect. As in an action photo, the figures would lack real movement, and a sense of movement entails the discussion of futurity. In other words, to be exact any sociological study must include a statement concerning the probable evolution of the phenomenon under consideration. It would be best if this statement were conscious and explicit. Of course this presupposes a study of the augmentation and diminution of certain

factors. And this requires normative, though not moral, appreciation. If one takes a photo one can simply say, for example, "Here you can see X small businesses with less than ten employees and Y corporations with over one thousand." But what sort of sociological research has been done if, having ascertained these facts, one does not try to find (1) the factor which best denotes the society under study (for example, does it have 10,000 businesses with two employees each, or one business with 50,000 employees?) and (2) what the future of that factor is according to the interpretive theory which one has established.

At this point, of course, the sociologist sticks his neck out: He may be contradicted by the facts. Thus we are confronted by a dilemma: Either we can avoid this dimension of sociological theory, in which case we do not account for reality, or we can accept it, and with it the test of fire. This is why I said that, though experimentation is impossible in sociology, verification certainly is! Yet this view is hardly to be found among sociologists and political scientists. One of the most remarkable proofs of this contempt for verification by examination of subsequent events is the following: I have noticed that when events flatly disprove a theory this in no way diminishes the capital of confidence and scientific esteem enjoyed by those who were wrong! There is in France an esteemed and very well known political theorist, a man highly prized for his brilliant analyses, who has been wrong *every single time* that he made a short-term prediction on the basis of his theory. All the specialists simply forget his errors, and he is still regarded as an excellent political theorist.

Again, in France in 1968 innumerable sociologists affirmed that "the revolutionary process has begun." Among them were conservative sociologists who deplored this "revolution" as well as progressive sociologists who were excited by it. After June, many sociologists announced that an active second push of the "revolution" would begin after October. All of them were wrong. Yet things go on as if they'd never said anything, and their sociological studies are as highly prized as before. On the other hand, that

tiny minority who claimed that the crisis had no political depth and was important only as an index of the gap between an important segment of French youth and the rest of society (and that it had to be analyzed as a test of this phenomenon) and that later on this crisis would leave ideological traces and introduce a certain rather predictable lag; or again that even tinier minority who had actually announced, as early as January that year, that an explosion would take place—none of these men are considered good sociologists! What a strange and fascinating phenomenon!

This leads us to ask what sense there is in sociology. For if it is a discipline satisfied with its methods and refusing all comparison with reality, a discipline which hones its methodology in private, as a thing-in-itself, we may well wonder what legitimacy it really has. If, on the other hand, it agrees to confront the facts, it is then obliged to account for a reality so complex that no method can grasp it and all positions taken toward it seem rather unscientific. When I speak of taking positions, I obviously don't mean a siding with some current political orientation, or what Touraine envisions when he speaks of "the function of sociology" (which, in his eyes, is to unite cultural and social conflicts, to "reintegrate the game with its stakes," as he puts it, the "game" being the social conflict and the "stakes" being the culture, which has somehow become dissociated from it). Nor do I think to prevent the confusion of a society's modes of action with an existent authority. Such notions presuppose a militant role for the sociologist, and I think that one might well take exception to the supposedly scientific character of such research. Yet one must be conscious that all sociological research involves either a confirmation of the being of society or a challenge to it. No rigor, no exactitude, no scientific method can eliminate this fact! One must be aware of it, and consequently be aware of the risk one takes in producing any sociological theory.

But I wasn't thinking of any of these things when I spoke of "taking positions." What I meant was the taking of decisions not *outside* one's own sociological work—so that it may be ori-

ented toward some ideology—but *within* this work, within what is properly scientific. I'm speaking, then, of the choice of a determining factor and of the understanding of the process of contradiction (a word I use to avoid "dialectic," that much-abused term which has led to so many confusions). I believe that the option to pick one or more determinants is unavoidable. This implies the attribution of a sort of coefficient to any given datum. This cannot be done abstractly or purely scientifically, in the classic sense of the word. And this coefficient depends on the globally explanatory character of the factor or on its potential as a determinant for the evolution of the system as a whole. Hence it must be selected in accordance with two criteria: (1) Which factor accounts for the greatest possible number of data? (Here I'm not referring to a sociological system but to something in the society itself.) (2) What seems to orient the process of development or the decline of a group or society? Hence one cannot avoid dealing with structured groupings. But structure has no value in itself: it's not the articulation of the structure of a group that's most important but—once that structure has been discerned—the discovery of whatever has instigated the structure and what can cause it to change. There is no permanence in an evaluation of this type. No complete generalization is possible in sociology: no explanatory theory works for all eras and all types of regime. An ideological, class, economic, or religious factor may be the major determinant for a group at a given moment, but one can't generalize on the basis of this fact. That was one of Marx's cardinal errors. He claimed, on the basis of an exact study of nineteenth-century Europe, to have found the key to all social evolution.

But as soon as we speak of determinants (and none is ever unique, exclusive, or permanent) we are obliged to introduce the dimension of conflict and contradiction. For this factor is never dominant in the continuation of the status quo. There is always at least one factor which tends to produce change. Such factors are of two types: Either there are radical upheavals through the

intervention of some unexpected disturbance factor (and here one cannot properly speak of a determinant) or there is change through one of the society's own components. In our global societies certain factors seem to follow a developmental process which both expresses the nature of the society and influences it totally. Such factors are a sort of privileged "place" where all sorts of forces converge and combine. A polarization of all social forces takes place around this node and follows its development. Such an evolution is both linear and conflictual: Apparently the factor in question develops its own logic, but at the same time this logic cannot unfold unless it concentrates within itself other tendencies; inversely, as it subsumes the principal orientations of the group the factor obliges the group to change. Thus conflict is necessary. But all conflict is not necessarily "social" or "political" conflict. Everything depends on the phenomenon as it rapidly evolves. Let me repeat that there is no certain objective method for ascertaining what the major determinant is. Here the sociologist's initiative, freedom, and personality enter the picture.

Yet I won't say that my view is antiscientific. I've tried to show that the sociologist's personality cannot be eliminated—no matter what method he uses; and that if one does not pick a determinant factor, attribute coefficients, and discern conflicts, one constructs an empty sociology, that is, one which can never conform to reality and which is thus unscientific. Verification through examination of subsequent events will bear out the value of such aleatory personal choices. In the end, these choices will always be more important than the fact that such and such an accident did, or did not, confirm the researcher's predictions.

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Preliminary Remarks on the Interaction Between American and European Social Science

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THE term "social science" is here taken in a somewhat broad connotation to include the ancient and well established social sciences, that is, history, law, and economics, as well as the more recent and less venerable ones, such as sociology, political science, psychology, cultural anthropology, and so on. Needless to say, the latter ones will command the bulk of our attention. Moreover, a caveat should be kept in mind as far as the phenomenon of interaction is concerned. It is doubtful whether any kind of interaction is likely to take place among sciences of a different substance and orientation without implicating a direct reference to the economic, political, and generally social background of the specific historical context to which those sciences belong and in which their more or less pronounced development unfolds itself.

Without falling into the verbal trap of the fashionable Marxian phraseology which tends to relate mechanically *Unterbau* and *Ueberbau*, it seems safe to maintain that an interaction between American and European social science would not make much sense or become understandable at all without taking into careful consideration the political and economic circumstances and the general historical features of the American and European setting. In this connection we will not be in a position to go into detail as regards the two settings, given the nature of the cursory