

Biography and Social Research

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Abstract

This essay tries to show the scientific validity of the biographical approach in social research. In a sense, it is the realization that nobody would talk about his or her intimate existential experiences, or *Erlebnisse*, to a tape-reading machine. The object of sociology is not an object. It is a person, a human being living in society, and as such it must be dealt with.

Keywords: biography; social research; existential experience; sociology; social commitment.

The Ambiguity of Sociology

As detached fact-finding and unavoidable social commitment, sociology is essentially ambiguous. Its object is not an object. It is a person, a human being, living in that special «togetherness» that is traditionally called «society». The question looms large, however: is there a crisis in sociology?

I am well aware that the question, while in itself legitimate, sounds largely superfluous. Sociology was borne out of a major historical crisis. Crisis have accompanied and stimulated its development. Far from being inhibited, sociology thrives on them. Crisis is nothing new to sociology. Its official founder, Auguste Comte, could not conceive of any other relevant function for sociology except the reorganization of society based on a scientific clarification of ideas to be used as cornerstones in the building up of social consensus. Rather than the «science of society», sociology should perhaps more appropriately be called the «science of social crisis».

What seems peculiar to the present day intellectual scene, however, is that the crisis does not concern only a given society to be analyzed and reorganized according to rational criteria with the decisive help of sociology. At the present time, the crisis seems to have entered into the very substance of sociology itself, both as a specific field of learning and as an instrument for social experimenting. There again, one should not forget that sociology has gone through other critical and difficult phases in its two-century old evolution.

Already by the second half of the nineteenth century, it became apparent that the high ambitions of the official founder, who would see sociology as the *Scientia scientiarum* as well as the operational tool for the rational transformation of society, not to mention

the aims and theories of Marx, Proudhon and others, were by and large misplaced ambitions, at least insofar as these thinkers believed it possible to set definite goals and outcomes of historical development through the discovery of social laws quite similar to a natural law. It is true that these early sociologists, whom I would describe as systematic because they appear to be concerned with society as a global, rational, and therefore, fully intelligible structure, or tendentially close system, never look as if they had doubts about the future of human society. They talk to the humanity in the name of humanity. For them, there is no division of scientific labour yet. They criticize existing conditions but they never seem to have lost their front-door key, as it were. They write about alienation, but they seem to know exactly where we come from, where we are now, and where we are bound to go. Great outsiders, their style of work is that of the classical craftsman: independent, autonomous, individualistic.

The systematic season did not last very long. Sociology soon became one academic discipline among others. Its object of study ceased to be mankind or history as such. Despite the extraordinary insight by the young Marx (in the *Manuscripts of 1844*) that «there will be only one science», including natural and social sciences, sociology tended to be more analytically defined and specialized. Before the century was over, Durkheim had severely criticized his great predecessors, Comte and Spencer, on account of their generalities. But was the academic community, in its structures and personnel, ready to accept sociology in the full sense: that is to say a science which is also a consciousness; a detached scientific analysis, which is, at the same time, necessarily a social intervention; an objective research in which, however, observer and observed tend, in the last instance, to coincide, making personal involvement inescapable?

The fact can be distasteful to many professional sociologists, understandably eager to become or to remain respectable members of the scientific community, but it should nevertheless be recognized and flatly stated there is a basic ambiguity in the sociological enterprise, which comes from far back. It is connected with its very beginning and it carries to this day a considerable weight in the present crisis. It should always be kept in mind as it cannot be dispelled by any trick. Too many descriptions of the crisis in sociology take the crisis for granted as a uniquely sociological phenomenon and are more psychological than intellectual. They reveal a disappointment, which probably comes from excessive expectation. No matter how verbally daring and iconoclastic these descriptions are, in fact, if carefully analyzed, they show a fairly awkward rationalization of individual troubles or personal positions. They usually boil down to outright self-justification. They are perhaps linked also with an uncritical idealization of science in general and should be regarded as a kind of intellectual hangover, the importance of which, as a symptom, is not to be minimized. Whoever was, until recently, ready to rely completely, or I should perhaps say religiously, on quantitative methods or on mathematical models applied to social phenomena, to the point of

risking the quantification of the qualitative, is now likely to repent so sweepingly as to swing to the extreme opposite position of renouncing any theoretical and logically grounded approach in favour of an immediate and total immersion in praxis. It is *prima facie* evident that the same idealizing or para-mystical attitude is here at work. Substituting Carnap or Lazarsfeld with Mao is hardly a way out of present difficulties.

This state of things should not justify any self-complacent irony on the part of self-styled «*esprits forts*». If there is a crisis in sociology at the present time, it is only natural that its first dimension should be the one which concerns directly the person of the sociologist, his self-image as a professional and his basic commitment as a citizen. After all, it is through individual sociologists that sociology lives and expresses itself in the world.

The danger lies in the reductionist tendency to view such a crisis simply as the outcome of some sort of personal maladjustment. There is some truth in this, to be sure, but it would be grossly misleading to confine the sociological malaise to its personal or psychological components. It should, in the first place, be noticed that this uncomfortable feeling of inadequacy, of not being quite up to the challenge of the times, and actually of being taken by surprise, if not altogether mastered, by events, of not being free from economic and social constraints, is not confined to sociology but concerns science and scientific activity in its wider meaning, especially as regards its implications. The crisis in sociology is not, therefore, an exclusively sociological matter. The idea of science as a smooth, self-correcting flow of cumulative knowledge, providing new theories as soon as new facts emerge which would not be explicable in terms of the old established theories, has been proven to be untenable.

There is a growing awareness that science can no longer be regarded as a secular religion, as a viable substitute for old-time dogmatic certainties. It is only a human enterprise, exposed to the risk of regression, whose history is full of economic pressure, power struggles, and mental and physical persecution. The second dimension of the crisis in sociology recalls, it seems, a more global crisis, a science crisis, concerning its place in the history of mankind, its final meaning and its social function. One used to think in terms of pure science and applied science. One used to question the technical viability and eventually the ethical acceptability of the application of scientific results to the production process. But the distinction between pure science and applied science is now clearly obsolete. Science is no longer connected with production through a technical mediation. It has been incorporated into the production process; it has become a direct productive force.

Under present day circumstances, the problem is rather a problem of control: science for whom and for what, and, first of all, science in whose interest? We had forgotten about economic interests as far as sciences is concerned. The scientific revolution

appeared as a neutral enterprise, far above any trivial, petty economic worry, a superior task. Scientists themselves were regarded as the new apostles for a new humanity: rational, unselfish, basically just, naturally democratic. These obvious corollaries of a technocratic mentality incapable of grasping the human meaning of science have found their inspired expression in the famous booklet by C. P. SNOW (*The Two Cultures and the Scientific Revolution*).

Sociology in crisis is then part and parcel of science in crisis, the sociologist is in quest of a new self-image, as much as is the nuclear physicist, or the historian or the economist. The terms of such quest are obviously different. But this difference has little to do with anguished introspectiveness or internal dispositions; it depends essentially on the needs of the society as they are expressed by the various publics corresponding to the various and contrasting social strata and classes of which society is composed. Now, to what public do sociologists address themselves today? No clear-cut answer seems, for the time being, satisfactory. But this is certainly the third major dimension of the crisis. To whom are sociologists talking today? We have seen that systematic sociologists used to talk and write for humanity as a whole. At present, however, sociologists prefer to talk and write for specialized publics – any general sociological contribution would rather fall in the domain of social philosophy and would not seem to be able to meet the essential scientific requirements for serious professional recognition. The fact is that the bulk of practicing sociologists today address themselves to academic colleagues. Even though later and to a lesser degree than other more ancient disciplines, sociology has become, more or less everywhere, institutionalized. Institutionalization is both a protection and a barrier. It tends to separate the sociologist from his specific society and from the peculiar needs of his immediate social environment. He then tends to view these needs as contingent problems. At worst, he might simply forget that sociology is not the object of sociology.

The third dimension of the sociology crisis seems, therefore, to be twofold. On one hand, there are the problems posed by a process of institutionalization within the framework of traditional academic structures which were historically formed and developed long before sociology emerged as a relatively autonomous field of scholarship. On the other hand, one must take into account the demands made by society upon sociology in terms of social intervention and social engineering, almost as if sociology were a magic art. Moreover, one should not overlook the frustrating effect that many of these requests have upon sociologists whenever a purely instrumental character, attributed to research and its findings, is transparent.

In this connection, the terms of a fourth dimension of the crisis become fairly clear. This dimension is no longer psychological nor organizational or purely institutional. It is substantive and strikes at the very heart of the discipline. It has to do with the construction of the conceptual frameworks whereby the gathering of empirical

data is coherently and significantly guided. Especially under the important influence of Talcott Parsons, contemporary sociology has made an extreme effort towards abstracting a concept of society as a system at such a high level that the historical bases and characteristics of social phenomena have been practically obliterated. This has permitted a certain degree of formal reasoning but at the price of being unable to describe and eventually explain relevant issues in a specific historical situation. The historical, dynamic nature of sociological concepts has been overlooked in the hope of building a universal body, timeless and spaceless, of sociological theory. That such effort bring about mechanical transfers of concepts from one historical and economic context to another end that, even more seriously, as David Lockwood and others have soon pointed out, would lead social research to a static meta-historical Utopia are nothing to be startled by. They are only the necessary consequences once the historical dialectical nature of sociological concepts has been denied or misunderstood. In this sense, the importance of Talcott Parsons' work can hardly be overrated, and does not seem to depend only on the fact that for many years Professor Parsons was the pre-eminent sociologist at Harvard. It also depends strictly on intellectual factors stemming from the centrality, or crucial quality, of his intention, which has been quite clear since *The Structure of Social Action* (1937). It consists in the elaboration of the concept of society as a system at a level of abstraction sufficiently high to permit subsuming under it, as subsystems, at least the concept of culture and the concept of personality. The framework made up by social structure, culture, and personality is the threefold grid, which, according to Parsons, would enable sociology to cope with any relevant social phenomenon irrespective of historical background or economic way of production or ideological orientation. In this way, Parsons' sociology present itself as the final and highest stage of sociological thinking, while representing, both domestically and from the point of view of international relations, a refined rationalization of the strategic needs of the American system (unifying coherence internally and instrumental hegemonic activism abroad); and, in this respect the two volumes of *Theories of Society* are quite revealing. This kind of sociology, perhaps the most sophisticated version of official, or academically accepted sociology, is certainly in crisis today, confronted as we are with largescale events in the United States and in the world which seem difficult or impossible to accommodate within that framework and which, on the basis of that conceptual scheme, there was apparently no way of predicting.

These considerations could be helpful not so much in deciding whether there is or is not a crisis in sociology, but rather in establishing what kind of crisis and what is to be done about it. Far too many explanations of the crisis have a purely *internal* character; they tend to explain and limit the crisis of sociology in terms of a technical breakdown, or of a failure to realize the political relevance of social research, or of a lack of financial autonomy. But they don't seem to recognize, in its full import, the

fact that the problems of sociology as a discipline are closely connected with the practical (objective) issues confronting the larger society, the mechanisms and the institutions which regulate its functioning. In this perspective, no catastrophic or romantic meaning should be attached to the word crisis. Crisis is distressing and painful but also revealing, epiphanic. As far as sociology is concerned, it is a crisis of growth, not of exhaustion. And the present state of crisis, if seen in its proper light, that is, not as an exclusively personal trouble, becomes important in order to rediscover and to reassert the nature of the sociological enterprise, its necessarily unfinished character, its problematic disposition, its ambiguity. It could also open our eyes to what «being a sociologist» means, that is to say to the importance of «being a man among men» and to the advisability, in order to recapture its original meaning beyond any illusion of ethical neutrality, of passing from «sociology as separation» to «sociology as participation».

Basic questions remain unanswered. In the first place, if it is true that definite value orientations and political presuppositions are implicit in any social research, should then the sociologist be purely and simply equated with the day-to-day politician or policy maker? Most radical sociologists would answer in the affirmative and would, therefore, be trapped by the contradiction between political immediatism and the scientific mediation of political relevance. They would be finished as sociologists without being sure of their rebirth as politicians.

In the second place, if it is true that sociology is connected with its specific historical and economic background, then there will be a plurality of sociologies or schools. Are these various sociological schools (corresponding to different value orientations and to different objective backgrounds) bound to:

- a) ignore or another;
- b) dogmatically fight one another;
- c) or is it possible, and desirable, to have an intellectual exchange among them?

In the end, having rediscovered sociology as a *science in permanent tension*, the phrase «sociology in crisis» and its ready-made prescriptions lose much of their appeal.

In particular, their limitations become apparent in the perennial fluctuation from an internal critique of the Parsonian evolutionary universals and the necessity of a clear denunciation of their ideological nature. A most urgent task, in this connection, seems to me the rediscovery of the importance of Marxism for sociology and the recapturing, as it were, of its sociological soul. I know, of course, that there are many Christian religions. What seems important is not to confine ourselves to the official Soviet version of Marxism, no matter how tempting the symmetrical parallel with the Parsonian systematic sociology in the end might be. What should be called into question is the way in which social theory is being built. It would be, in my opinion,

a sad mistake to think that only formalistic theories developed within academic institutions are worth considering as scientifically relevant. Marxism after Marx offers us quite an example of a theory of great scientific and political relevance that has been developed outside, if not against, academia. Let's face it, if Weber and the Weberians, Parsons and the functionalists, old and new, have constantly reacted to Marxism, this has happened not on the basis of scientific premises or of technical developments of the theoretical framework, but rather under the pressure of real, practical conflicts, both economic and political, which would radically question the institutional fabric underlying the legitimation of their theory. The construction of social theory should not be confused with an abstract model building, essentially meta-historical and apolitical and in the end, at best, tautological. We should learn from Marx and practice the «rule of historically determined abstraction». It implies a constant back and forth between the theoretical level, field work and political, practical involvement in such a way as to give rise to a process of mutual fertilization and enrichment between theory and practice.

Some fifteen years ago, I published, in Italian, a book called *Sociology as Participation*. The book is now out of print; it was not quite understood, and I mean this literally. Nobody can understand Italian nowadays. Not even opera singers. The book fell flat between the autobiographical lamentations of a dissatisfied, part-time politician, and the do-goodness of a half-baked Marxist social worker. *Habent sua fata libelli*. What I wanted to convey, however, was that we need a radical reorientation: from science as separation to science as participation, practical involvement, political commitment; a return to science as a human, all too human enterprise. I don't think involvement is a moral or, worse, a philanthropic exigency; it is a cognitive, scientific requirement. Nor do I think that passing from Parsonian functionalism to ethnomethodology would be a way out. Undoubtedly, sociological analysis is inevitably dependent on the common-sense understanding must not be used as an unexamined resource as in conventional sociology but must be made a topic of enquiry. In this way, they try to mark out a domain, which is distinct from the concerns of the actors themselves; for instance, which would treat talk as a topic rather than a resource - a domain taken for granted by actors, or members. Thus, the ethno-methodologists seek to re-establish the scientificity of sociology and avoid the crisis of not being able to distinguish logically between sociologists' discourse and members' discourse, as they see it. My reaction to the claims of ethno-methodologists would run, by and large, along the following lines: first, if common-sense understandings are inevitably used by conventional sociologists, they are equally used by ethno-methodologists; and second, simply changing the focus of analysis to what members take for granted cannot overcome this, although it can give an illusion of working without presuppositions. Third, the idea that sociology can regain its scientificity at the level of analysis of interpersonal relations is mythical, and not, of course, without political implications.

Where the ethno-methodologists seem right is in insisting on the inevitability of using common-sense understandings. This means that sociology cannot claim its old-time, scientific and early positivistic objectivity. But this is, to my mind, all too the good. Sociologists have shown a tendency to be either too grandiose or too petty, and in the end they have often settled for navel-gazing if not for the ambiguous role of classroom Machiavelli.

I know: it is always later than we think. And yet, we should never be in a hurry to conclude, to close the discussion, to arrive at a final synthesis. We live in an age, it seems, in which the lived experience is richer than conceptual frameworks. We must perhaps give up the nostalgia for a complete *globus intellectualis*; that is, for an all-comprehensive system, without renouncing the chance to build it when the time will come.

The Rejection of the Sociological Approach

In the broader cultural landscape, one cannot help noticing the persisting “separateness” of sociology. The intellectual situation – not only in Italy even though certain phenomena take on extraordinary importance there – presents a somewhat paradoxical appearance. Today we are in a kind of sociological *lógos spermatikós*. One is aware of a curious transmigration of concepts and theoretical apparatuses, a penetration and utilization – mostly surreptitious of concepts and categories borrowed from sociology, in history and also in geography, economics and in Italian studies themselves. At the same time, Italian culture appears more than ever closed to sociology, which is systematically rejected and condemned to a ghetto existence as “sociologism”.

A contradictory situation is therefore emerging. While the conceptual apparatuses and technical equipment of sociological research are increasingly employed, in a more or less spurious manner, we have come to a real crisis in the rejection of sociology, culturally isolated in its particular “separateness”. As a recent example, not among the most significant, one thinks of the welcome proffered in Italy to Jean-Louis Flandrin’s book (1979) on the “family” in pre-industrial society. The theme of the family is typically sociological, but in Italy not even the famous *Storia d’Italia*, published by Einaudi, which believes itself inclined to the social and thus highly innovative, takes it into consideration.

The polemical reaction of – as it were – “official” historians was instantaneous and very tough, but also revealed an unquestionable discomfort (cf. Corsini, 1980). Some isolated voices were raised in defense of the “new history”, which was then new only relatively speaking, observing that the “official” historians, the accredited representatives of the “old history”, viewed “micro-history” with a certain contempt. This was because they held, as Diaz argued, that “the beliefs, myths, or obsessions of Menocchio” (the sixteenth-century

millers who were the protagonists of Carlo Ginzburg's *The Cheese and the Worms*) are of very little account when they "are not inserted into the major movements of society and the economy where political will is always the great motive force of historical development". Then they reprove the "new historians" for having invented nothing, and for being at most affected by "strange curiosities" (cf. Diaz, 1980; Aymard, 1978),

This kind of situation concerns not only Italian culture but also external factors, such as political terrorism, which cast shadows on the social sciences; it leads to a defense of sociology that guarantees rigor in concepts and the correct use of specific research techniques. However, this legitimate defense runs into internal difficulties and weaknesses in sociology, especially in the tendency to conceive of it in a reductive manner and from a restricted perspective. And it serves only to make it the equivalent of any technical practice: ideally indifferent, morally neutral and mechanically interchangeable, ready to sell itself on the market to the highest bidder. This is due in part to the confusion between professionalism, as a calling to the practice of a set of skills manifested in personal ability, and profession in the full bureaucratic sense – a service requested by the client and over which the client himself has instrumentally no possibility of control.

The Biographical Method

Life history as the basic method of sociological analysis drastically eliminates these and similar reductivisms with ease. However, the conscious critical adoption of life history as method is not simple, nor can it hastily be converted into a convenient short-cut, as it has sometimes been interpreted by some students of "community" who are as socially generous as they are culturally ingenuous. Only by way of a long series of intellectual experiences and practical tests in the field have I arrived at the point of confronting the problem of the *autonomy of the biographical method* and its decisive character in the future of research in the social sciences. Interested in the human consequences of technical and economic-industrial development, in the 1950s, I had begun to collect life histories and autobiographical documents in some Italian communities, which seemed to be attacked forcefully and in notable proportions by the process of industrialization.¹

At first, this research concerned the effects on an agricultural community in Northern Italy of a "mass production" plant, the rapidly expanding Olivetti factories. Later I sponsored and carried out similar research in the South.² It was my intention or, rather,

¹ I have written extensively on the process of industrialization as a global social process. These writings are collected in *Sindacato, industria, società* (Ferrarotti, 1970b). For the more strictly autobiographical aspects, see the prefaces to some of my books, especially Ferrarotti, 1982; 1976. A very summary first outline of my development in this respect can be found in my remarks in Ferrarotti, 1978b: 130-2.

² See Ferrarotti, 1950a. Part of this research involved the commune of Castellamonte, Canavese: cf. *Quaderni di sociologia*, 1 (Summer 1951). I later published the biographies (Ferrarotti, 1959).

I nurtured the hope that by means of this research I might find a positive solution to the deficiencies of sociological studies carried out only on the basis of rigidly structured questionnaires. For some time I had had the impression that research of that kind, although very rigorous from the formal methodological point of view, generally ended up by regarding as resolved problems which in reality had not even been touched upon. From the time of my visits to Chicago, and long, impassioned discussions with Leo Strauss, the immortal author of *Natural Rights and History* and *Thoughts on Machiavelli*, it seemed clear to me that the world of values not only should not escape the attention of the domain of social sciences in the name of a raw, fragmentary “factuality” of a paleo-positivist kind, but that shared and jointly undergone experiences and values were their living thread and privileged object, and should be a central focus of our work.

However, I did not have the necessary conceptual clarity to see, then, that this implied a reversal of the prevailing methodological positions, which unconsciously quantified the qualitative. I also failed to see how this reversal would not take place, would be destined to remain an ambiguous prologue in the clouds if a break in the method of conceptual expression and the practice of the social sciences did not come about. I could understand that life history was an often valuable contribution to directing research, but I did not realize that the ancillary function to which it was relegated was more than a matter of simply conforming to the canons of an objectivity still conceived of in grossly naturalistic terms. This way of regarding life histories as the music and scenery that add color to the main production of research corresponded precisely to the wish of the researchers not to take up a position, and their logical refusal to situate themselves politically and socially, so as not to destroy their chances in the market. I must therefore acknowledge that the way in which life history presented as a method of “field work” in my *Trattato di sociologia* (Ferrarotti, 1968) was, if not erroneous, incomplete and insufficient.

Although I was especially struck by the *synthetic character* of the autobiographical account as a *practice of life*, I felt the danger of *literariness*. That is, I was held back and tormented by the incontestable fact that the individual biography was, after all, an account of a unique and irreducible destiny. I could not see the nomothetic elements present in the descriptive. This had a reductive effect on my manner of viewing life history. At most, I recognized in it an integrative function, which was however, in the strict sense, unverifiable. I regarded it as useful as an instrument of *background research* – a usefulness to which I continue to attribute a fundamental importance today. But I did not grasp the basic elements of what I now call the *dialectic of the social*, which lies essentially in the complex non-a-priory-determinable relation between *givenness* and the *lived*. Certainly, the structural frame, givenness, was primary in my concerns, but I could not understand that by itself givenness understood as *reified factualness*,

a fact closed in on itself and distanced from the living, is nothing. It cannot even be analyzed by the social sciences as their real object without lapsing into the fetishism of elementary empirical data held to be theoretically autonomous and self-explanatory, as if truly *the facts spoke for themselves*. Like a clumsy pathologist, by error going into the operating theatre, I conscientiously dismembered the living with the same meticulous care with which I would have set about the autopsy of a corpse.

The Transition from Rural to Industrial Society

In this early work I attempted very carefully to connect the individual biography to the global, structural characteristics of the given, lived, historical situation. My problem was always that of the human and social consequences of the process of industrialization.³ It is the same problem that I was later to identify as the problematic relation between technico-formal rationality and substantive rationality. The latter is what Weber called “material rationality”, from the depths of his age-old pessimism (as befitted an “orphan of Bismarck”, an elitist *malgré lui*) about the practical possibility of managing to formulate (in a politically valid, i.e. intersubjectively binding manner) common, properly collective ends.

In this perspective, biographies had the goal – heuristically speaking – of illuminating the transition between the peasant world and a technically oriented industrial society. I held that through biographies “transition” was no longer a mere abstract category, a pure analytical term. It was personified and so to speak fleshed out by specific types, whose biographical elements provided the particular nature of sociological material. However, the material provided by biographies was always only material I considered to be illustrative. In the *Trattato di sociologia*, I did not go beyond the notion of life history as endowed with a primarily integrating function as regards quantitative data. The examples I quoted then did not depart from the traditional type of monograph, moving between the rigorous inventory of anthropological authority, and what is called today in the United States “investigative journalism”. I was well aware of the ecological studies of the Chicago School of the 1930s, from Clifford R. Shaw’s *The Jack-Roller, a Delinquent Boy’s own Story* (1930), to Paul Cressy’s *The Taxi-Dance Hall* (Chicago 1932), but I did not omit the basic research of Frédéric Le Play on family budgets, and I also spent a little time on the Italian contributions, though these were still very undecided between an ingenuous populist protest, social documentation and evocative history.⁴

³ As seems clear to me from a backward glance, right from my earliest books, such as Ferrarotti, 1950b; 1954b; 1955b.

⁴ See in particular Ferrarotti, 1968: 387-91, 419-23. As regards Le Play, I brought out his originality, in that he had discovered the method of participant observation independently, fifty years before Malinowski introduced it into anthropology, and seventy years before it passed into modern sociology (cf. 1968: 421). Today, besides the names then mentioned, from F. Cagnetta to D. Dolci and R. Scotellaro, see the biographies collected by Revelli (1977) (whose extraordinary character is not excessively damaged by the fact that the biographies are not sufficiently placed in context).

Despite the limitations which appear today in certain formulations in the *Trattato* – limitations connected primarily with the schematic, dichotomous opposition between the descriptive moment and nomothetic standardization – I think that from then on the importance of life history for me was indisputable. As I argued, this was because “it allows us to move to analysis in depth”, although just there, in that holistic scope, appeared to me to be its characteristic and chief difficulty; “it demands and immediate contact, one of reciprocal confidence between the object of research and the researcher” (Ferrarotti, 1968: 387). The discovery of the interactionist relation implied by this “confidence” as basic in and specific to life history, must consequently be seen to have been extraordinarily productive in shaping my critical sociology. In fact, during this research in the 1950s, little by little the idea of a sociology as participation was taking shape, as distinct from the generic “participant observation” of mass observation, presenting itself as the first nucleus of an alternative, meta-mechanistic, methodological foundation (cf. Ferrarotti, 1961: 1972).

Methodology as a Technique of Listening

This is not the place to retrace in detail an intellectual journey which is not without tangled knots still not wholly unraveled, but in which, notwithstanding, three phases which correspond fairly exactly to the three periods of my scientific work can be distinguished and briefly described here:

1. The study of Thomas and Znaniecki (1918-20)⁵, in which the function of biographical materials is purely illustrative with regard to knowledge already acquired by other means. In this sense and for this aspect Herbert Blumer’s criticism (1939) seems to me to be conclusive.

2. The phase of *Vite di baraccati* (Ferrarotti, 1974b), in which, especially in the first chapter, now in *Cahiers internationaux de sociologie* (Ferrarotti, 1980d), I criticized the bases of Oscar Lewis’s research on the culture of poverty. My criticism basically concerns Lewis’s notion of the family as the “natural unit” of enquiry.⁶ However, the whole book should be seen as the sequel to a previous work on Rome (Ferrarotti, 1970a), in which traditional quantitative methods, as also later in Ferrarotti et al (1980-2), had yielded all they could. For example, they had permitted the establishment of a connection between poverty, social marginality and “special classes”, which accounted for the fact that these classes, set up to help children who normally were not able to keep up with the others as regards scholastic achievement, ended up in practice by branding

⁵ For a panoramic review of Polish sociological studies based on life histories, see Markiewicz-Lagneau, 1976: 591-611.

⁶ In my view it seems essentially misguided that the various members of the “Sanchez family”, for example, should recount their history independently of each other as individual-monads, thus losing for the family itself its character as “primary group”, and giving rise instead to a series of “parallel lives”. In the life histories I have long been collecting, by contrast, each person’s account is continually interrupted and corrected, as it were, by the comments of other members of the group, precisely because the group itself lives its own specific life and interacts in a rich, interrelating dialectic.

the children in them with a mark of mental inferiority that confirmed their state of social inferiority. However, in order to understand the scope and human meaning of the phenomenon, biographies and intensive specific case studies, showed themselves to be essential and not purely integrative of quantitative data.

3. The current phase, in which I am suggesting the study of the primary group and “basic associations” through the biographical method, centering on some fundamental variables such as (a) experiences of work (cf. Terkel, 1974); (b) class structure as global, structural position and at the same time as specific existential content; and (c) the context, both given and lived, of the framework of a determinate historical situation (the notion of “historical horizon”).

Today I begin to see, perhaps with a clarity and immediacy formerly denied to me, how difficult it is to acquire the full knowledge of the cultural and political implications – in the proper sense, not the party political one – of the research work one is engaged in. It seems quite clear that many researchers who customarily use the biographical method nonetheless are unaware of its possibilities; they use life histories as a not strictly necessary element of a qualitative kind, to integrate work based on survey methods or opinion polls carried out on representative samples of a given, statistically identified universe (naturally, based on official statistics). In these cases, life history is presented as a photogenic overlay, worth a thousand (sold, statistical) words.

This use of the biographical method is not in itself illegitimate, but it does demonstrate a radical inability to place the problem of such research at the critical level. Besides the interactionist relation, which revolutionizes the traditional foundations of research in that it presupposes a situation of basic equality between the researchers and the human groups being analyzed, the unique thing about the biographical method is that it allows one to reach social strata and structures of attitude which, because of their marginal nature and their state of social exclusion, inevitably escape formally gathered and analyzed data and the official images society has of itself. The insurmountable limitation of surveys concerns not only the asymmetrical structure of the research, the pre-codification of the questions against the apposite responses before knowing what the important problems may be, or the unjustified second-level analysis of data now completely abstract and hence dehumanized. This limitation is also a *technical starting-point datum* and derives from the fact that sampling operations inevitably require data already gathered officially, for and by statistics, which cannot be contested. The research thus designed and organized starts off from the condition of a “hostage of officialdom”, or, that is, is already in the very hand of that formal institutional structure when it should rather have set itself the task of critically testing, describing, interpreting and *demystifying* it to get to the “raw data” of its research problem.

If, then, one is aware that in only moderately developed societies the most important social problems, whether as structure or behavior, do not emerge, do not stand out with the desired clarity in the framework of official figures – precisely because of their refractory nature in the face of the tendency to encapsulate bureaucratically that which is lived – a double phenomenon becomes manifest. On the one hand, there is the tendency for social science research to adopt already constituted and rigid methodological categories which exclude from the scope of the research what is basically extraneous to it, even when it may be socially and politically of great potential importance. On the other, there is the need to adhere to a *methodology as a technique of listening*, in which, between the human researchers and the human group being investigated, there is a communication on equal footing, which is not only methodologically correct but humanly meaningful. This meaning is not a moralizing or optional optimal value for research, but an integral part, a guarantee of methodological correctness.

Towards Biographical Research

In this sense, I maintain that the discussion of the biographical method and its autonomy should be reopened. The use of biography in the social sciences, despite the particular favor shown to it for some time, has not exhausted all its possibilities and indeed has even retarded the advance of the conceptual and practical, awareness that life history as a method in the full sense necessarily implies a break with current methods and that in addition this break rests on and is manifested in a systematic uncertainty in its present use. This involves a rigorous analysis of:

1. The *Erlebnis*, especially in Wilhelm Dilthey's theorization, as the possibility of "re-living" existential and historical experience in terms of "interior interconnection", not assured by reference to a psychologizing subjectivism and voluntarist idealism;
2. *Verstehende Soziologie*, Weber's "understanding sociology", whose attempts, so widely misunderstood by interpreters of a more or less strict historicist persuasion, to construct a synchronic or "ideal-typical" model – only with diachronic historical materials – can be appreciated. This however does not escape the well-known deficiencies of formalism and methodological individualism;⁷

⁷ This point has been well taken and expressed by P. Ammassari: "Weber, who was firmly nominalist, rejected any 'substantialist' (*substanzielle*) conception of collective formations, repeatedly insisting that the sole unit of analysis of sociology is the action of the individual being (*Einzelindividuum*) and that any other concept (state, class, stratum, family, community, association, etc.) simply denoted a particular category of social relations, which could be considered as independent subjects only for investigative purposes. However, from this nominalism which preserves only the individual, and puts forward the connection of subjectively intended meaning in action as the correct object of sociology, there does not follow simply and primarily a psycho-sociological study of such meaning and its 'typically homogeneous' forms: that is, of the uniformities of meanings, intentions, and motives which make up the phenomenology of social action". (Introduction to Gerth and Mills, 1969: XLIX).

3. *Intuitionist currents*, formerly harshly criticized by Max Weber (“whoever wants a sermon should go to a monastery; whoever wants visions should go to the cinema”);
4. The minimalist use, ancillary and illustrative, pioneered by Tomas and Znaniecki in their classic work *The Polish Peasant in Europe and America* (1918-20) confined by them however in the framework of a function broadly illustrative of socio-anthropological considerations *separately* gathered and analyzed. Currently, on the other hand, we see the tendency to use biographies as illustrative material not strictly necessary in the context of the studies, surveys, polls, all based on quantitative techniques, almost as though to “sweeten” the rigors of exact quantitative measurement, thereby dusting off again the unjustified dualism between tough-minded and tender-minded – or sentimental – investigators. The latter are more drawn to a cloying philanthropic and social-populist participation than to the tough discipline of scientific research in the real sense. See, for example, John Goldthorpe’s research on social classes and on the affluent worker, where, when it is admitted, life history is conceived of as repetition, an appendix for the traditional quantitative surveys: as a qualitative sounding, but scientifically not serious because it is not quantifiable (!), and hence subject to the dreaded deficiencies of the “science of the vague”, as one might in this context define human sciences;
5. The *dépassement* suggested by Daniel Bertaux and others, which falls under the attack of the remarks on *Insiders and Outsiders* by Robert K. Merton as “empathic participationism”, essentially acritical.⁸

Beyond the Secondary Biographical Materials

Traditional biographical method prefers secondary materials (“more *objective*”) to primary ones, or those collected directly by the researcher in contact with the subjects of the study, systematically bringing the secondary to the primary. Despite its epistemological abdication, this method retains the value of breaking with traditional methodologies: it is a new approach, and fragments of society long crushed by a kind of formal, sociological encirclement and suffocation, break the barrier and force themselves on our knowledge. However, the biographical method does not realize and indeed betrays the greater part of, its heuristic potential when it accepts being a marginal methodology of social history and a sociology in search of a “concrete shell”. The elementary condition for a renewal of biographical method runs by way of a reversal of this tendency. *We must abandon the privilege accorded to secondary biographical materials. We must bring back to the very heart of the biographical method primary materials and their explosive subjectivity. We are not only interested in the objective richness of primary biographical material; we are chiefly concerned*

⁸ Cf. Bertaux, 1977, but especially the report presented at the 1978 World Sociological Congress at Uppsala (Cf. Bertaux, 1981).

with its *subjective fullness of meaning* in the context of complex interpersonal communication – reciprocal between the narrator and the observer.

Here we approach the central problem: *how can the subjectivity inherent in autobiography become scientific knowledge?* If the biographical method decides no longer to dodge and renounce the subjectivity and the absolute historicity of its material, in what manner can it grounds its heuristic value?

Let us be satisfied with tracing the general, hypothetical lines of a reply.

Every autobiographical narrative recount, according to a horizontal or vertical section, a human practice. Now is, as Marx put it in the sixth thesis on Feuerbach, “the human essence... in its reality is the ensemble of the social relations”, any individual human practice is a synthetic activity, an active totalization of a whole social context. *Life is a practice which appropriates social relations (social structures), internalizes and re-transforms them into psychological structures for its de- and restructuring activity.* Every human life reveals itself through its less generalizable aspects as a vertical synthesis of a social history. All behavior, every individual act appears in its most individual forms as a horizontal synthesis of a social structure. How many biographies are needed to arrive at a sociological “truth” and what biographical material will be most representative and give us first some general truths? Perhaps these questions have no meaning, because – and in full consciousness we emphasize this point – our social system is completely within each of our actions, our dreams, fantasies, accomplishments and behavior, and the history of this system is completely within the history of our individual life.

In the strict meaning of the term, we “imply” the social through a synthetic interpolation which de- and restructures it, endowing it at the same time with psychological forms. However, as it is the product of a synthetic practice, the relation, which links an action to a social structure, is not linear, and the close relationship, which runs from a social history and a life, is certainly not a mechanical determinism. We must abandon the determinist model, which directed the attempts to interpret the individual through sociological frameworks, borrowed from bad textbooks of a naturalistic science, which the most alert scientists themselves had already abandoned. The individual is not an epiphenomenon of society. In relation to the structures and history of a society, he is an active pole and he impresses himself on it as a synthetic practice. Far from reflecting the social, the individual appropriates it, mediates it, filters and retranslates it by projecting it in another dimension, which then becomes that of his subjectivity. He cannot break away from it, but he does not suffer it passively, and indeed he reinvents it every second.

Here, Sartre’s formulation seems to me the only possible one, at least in the sense of a suggestive literary example. Man – and I would add, man invented by the bourgeois

evolution – is the singular universe. Through his synthetic practice, he singularizes in his actions the universality of a social structure. By means of his de- and re-totalizing activity, he individualizes collective social history. Here we are, therefore, at the heart of the epistemological paradox with which the biographical method presents us. We can no longer compare what an action or the history of a life have in common with the actions and the histories of other individuals – in a general perspective which alone could be scientific knowledge – with everything this action or this history contains, which is absolutely specific. Uniqueness, that is, which will never be science, but an unexplained, pre-scientific residuum, chance. A social anthropology, which considers each person as the individualized and active synthesis of a society, eliminates the distinction between the general and the particular in an individual. If we are, if every individual represents, a *singular* reappropriation of the social and historical universal which surrounds him, we *can know* the social by starting from the irreducible specificity of individual practice.

From the restoration of subjectivity to science: what makes an action unique or a history individual presents itself as a means of access – often the only possible one – to the scientific knowledge of a social system. This is not a linear path and is often cryptic, requiring new, original approaches in order for it to be pursued.

Bibliography

1. Aymard, M. (1978) 'The impact of the "Annales" school in the Mediterranean countries', *Review* 3-4 (Winter-Apring).
2. Bovone, L. (ed.) (1986) *Storie di vita composita. Una ricerca sulle scelte esistenziali della generazione di mezzo*. Milan: Angeli.
3. Cavallaro, R. (1981b) *Storie senza storia. Indagine sull'emigrazione in Gran Bretagna*. Rome: Centro studi Emigrazione.
4. Cavalli, L. (1981) *Il capo carismatico*. Bologna: Il Mulino.
5. Ferrarotti, F. (1950) 'Techniques and Community Life' in W. Scott (ed.) *The Community Factor in Modern Sociology*. Paris: UNESCO.
6. Ferrarotti, F. (1957) 'Sociology in Italy: problems and perspectives', in H. Becker and A. Boskoff (eds) *Modern Sociological Theory in Continuity and Change*. New York: Dryden.
7. Ferrarotti, F. (1980) 'Les biographies comme instrument analytique et interprétatif', *Chaiers internationaux de sociologie* 49: 19-27.
8. Ferrarotti, F., G. DE Lutiis, M.I. Maciotti and L. Catucci (1978) *Forme del sacro in un'epoca di crisi*. Naples: Liguori.

9. Ferrarotti, F., (1983) *Histoire et histoires de vie. La méthode biographique dans les sciences sociales*. Paris: Méridiens Klincksieck.
10. Ferrarotti, F. (2007) *Social Theory for Old and New Modernities. Essays on Society and Culture, 1976-2005*. New York: Lexington Books.
11. Ferrarotti, F. (2014). On the strained relationship between philosophy and sociology. *Academicus International Scientific Journal*, (10), 14-19.
12. Ferrarotti, F. (2021). Introductory remarks on ideology and sociology. *Academicus International Scientific Journal*, 12(24), 16-38.
13. Gerth, H. and C.W. Mills (1969) *Carattere e struttura sociale*. Turin: UTET.
14. Maciotti, I.M. (1986) *La disgregazione di una comunità urbana. Il caso di Valle Aurelia a Roma*. Rome: Siases.
15. Sartre, J-P. (1960), *Critique de la raison dialectique*. Paris: Nouvelle Tevue Française.
16. Stoetzel, J. (1984) *I valori del tempo presente. Un'inchiesta europea*. Turin: Sei.
17. Thomas, W. I. and F. Znaniecki (1918-20) *The Polish Peasant in Europe and America*. Chicago: Chicago University Press.
18. Touraine, A. (1984) *Le retour de l'acteur*. Paris: Fayard.
19. Vittorini, E. (1962) *Le due tensioni*. Milan: Il Saggiatore.