

Interview to Alberto Marradi

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Some time ago, when Alberto Marradi slowed down his activity in the methodology summer schools he founded, I matured the intention of collecting some essays as a tribute to this Maestro of sociology, but above all of Italian methodology and social research. What follows is the result of this idea, which opens with an interview with Alberto, where he traces his university career and the main turning points of his scientific thought. Methods such as deflation or techniques such as stories or the use of self-anchored scales, or the close criticism of instruments for surveying opinions and attitudes such as the universally and badly used Likert scales, should become a constant part of every social researcher's toolbox.

Philosophy and history.

In the high school I was doing well in literary subjects, while my math teacher was a bigot with no certification, and therefore I was left with the much that a demanding Sicilian teacher had taught us in junior high school. That was not enough for me to take on a science-type faculty. In the advanced level examination I got 9/10 in almost all subjects, but I still suffer from a double 7 in history and philosophy that a certain Fortunato Brancatisano, a Calabrian Marxist, sentenced to me. I would have accepted an even poorer mark in philosophy, which like all discursive subjects had remained foggy to me, but in history I felt I deserved an A. I always felt history as a part of me, I feel it inside. On my computer desktop I constantly update a dozen historical files scanned by periods (up to 1000, from 1000 to 1500, and so on). Whenever an event with a date is mentioned on television I take note on a piece of paper, and as soon as at the computer I insert that enrichment in the relevant file.

I am wary of theoretical reflections that border on chatter: I need concrete things, events and dates. I do not agree with those who disqualify dates as forms of "notionism"; for me dates are all important because they anchor events, that

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otherwise would float in the air. I find contemptible the habit, of American origin, of citing works which have been written in ancient times with the publication date of their most recent coming out: e.g. Thucydides (2013). I have introduced the criterion, adopted by some colleagues, of always putting in brackets first the date on which a text was written, then the date of the translation/version from which we are quoting. With a little patience, by searching in internet, it is not difficult to discover the date in which any manuscript was written.

How did you arrive at sociology and methodology?

From high school I could have gone towards the literary faculties but I was not attracted by the natural outlet of secondary school teaching, while for the scientific faculties I had the shortcoming of the bad teaching in mathematics I told you about; therefore Political Sciences was an almost inevitable outlet.

I attended the courses with profit — I got the prize for the best graduate of the year 1965. In Florence there was the famous Cesare Alfieri, created by a nobleman from Piedmont in the 1860s when Florence was Italy's capital, and it was a somewhat snobbish faculty. Some of my classmates did not say: 'I attend Political Sciences'; they said: 'I attend the Cesare Alfieri'.

In '71 I won the position of assistant professor with Sartori, who was then participating in a major international empirical research and needed someone to get his hands dirty: therefore he decided to invest in me. With funds from an American foundation he enrolled me in a series of summer schools in data analysis: Essex in '71, Cologne in '72, Syrrathclyde (Glasgow) in '73 and Ann Arbor in '74.

In the early 1970s, Franco Leonardi had created a new Political science faculty in Catania; taking advantage of Formez funds, he organized major international conferences. My thesis advisor, Alberto Spreafico, who had taught in many places and won tenure in Catania, recruited a generation of 35-year-olds, attracted by the condensed time schedules and the fact that they could have assistants paid by the Formez. Thus, political scientists such as Caciagli and Cazzola, sociologists such as Reyneri and Moscati (for a brief period even Alberoni), and economists such as Fodor, Gilibert and Marina Storaci flocked to Catania. The teaching of methodology was held by Stefano Draghi, a student of Pagani's, who, however, was disgruntled and wished to return to his Milan. For this reason in 1977 Spreafico called me to replace him, entrusting me with the teaching of Statistics for the social sciences in addition to that of Methodology.

In 1981 I won a full professor position and was called to Bologna where I was very well off; however, soon I had to return to Florence because my father was ill and my elderly mother was suffering to assist him alone. Fortunately Alberto Spreafico, supervisor of my thesis in 1965, took care of the transfer. Since in Florence Methodology of Social Sciences was taught by Alessandro Bruschi (who had graduated with Sartori, who was then dean) the subject I taught was called Methodology of Political Sciences.

What important experiences did you have in Italy during your career as a methodologist?

In Catania, sociologists and political scientists almost always gave empirical theses, which in fact entailed that it was up to me to follow each of them. The most important experience was on the occasion of a nationwide research directed by Luciano Gallino, full professor in Turin, in which Emilio Reyneri, then assistant professor in Catania, participated.

In those days, interview responses, once coded, were punched on 80-column cards. There were no personal computers, and the data were analyzed thanks to one of three computer centres that served all of Italy: the CINECA in Bologna, the CNUCE in Pisa and the CSATA in Bari. Boxes of punched cards had to be brought to one of these centres where they were transferred to disks. The departments of Catania were connected with CSATA. I remember Reyneri and I bringing two of these boxes to Bari in order to transfer his data to one disk. Back in Catania, I would send the analysis programs to Bari, and after hours of waiting the time would come when the CSATA technicians would load our disk, and the analysis results would arrive on the department printer. The trouble was that any trivial mistake in typing an instruction would cause the program to stop, and we would have to wait in line again.

Tell me about your career.

In 1971, when the competition for full assistant professor was held in the Faculty of Political Science in Florence, the holder of the chair, Giovanni Sartori, was in America, and the committee was chaired by Stefano Passigli, a young professor who esteemed me; my paper was also appreciated by the other two commissioners. For years I worked assiduously in the Institute of Political Science, putting together a classification by topic of the books present in the department library that was much appreciated by Sartori and all his colleagues who visited him.

In 1977 Alberto Spreafico, who in the meantime had won tenure in Catania, got me a position there in Social science methodology. The dean was Franco Leonardi, an excellent organizer: at his behest the faculty had an open-shelves library — a rarity in Italy. In 1980-81 competitions were announced for as many as 9 chairs in political science. There was indeed room for many candidates; I won and was called to Bologna by Giuseppe Di Federico, who had appreciated an extensive review of the political science research on the judicial system that I had published years earlier in the *Rivista Italiana di Scienza Politica*.

How did the instrument you call “the stories” come about?

In 1970 Taranto had been chosen as the site of an IRI's steelworks in Italy, which was then christened Italsider (now Ilva). In the early years the people of Taranto had welcomed the novelty; but that attitude changed soon. Then a manager asked professor Luciano Gallino to point out a colleague who had both methodological and anthropological-cultural sensitivity and was therefore able to discover the reasons for that change. Gallino knew me because I was publishing in his journal; he suggested my name. At the time I was teaching in Catania and living in Aci Trezza. I did travel between Catania and Taranto by a night train.

When setting up research in an unfamiliar environment, it is a good idea to start by interviewing so-called “privileged witnesses” — people who are assumed to have a good knowledge of that environment. I asked each witness to describe episodes and situations that in her/his opinion were noteworthy. By choosing the most interesting episodes and generalizing them, I constructed a number of “stories” each attributable to a socio-anthropological (particularism/universalism) or psychological dimension (emphasis on reason/emphasis on feeling; tendency to take responsibility/ tendency to avoid it; passivity/ initiative; and so on).

As for the research carried out on behalf of Italsider, the managers thought that the hostility of the population was due to deaths at work, especially since this topic was much emphasized in the local press. Thanks to my research, it turned out that the problem was instead the citizens' relationship with the sea: draining water from the sea to cool the plants killed many fish and other sea creatures, and just as many were killed by pouring into the sea the water that had been heated by passing through the plants. This generated muted hostility among the people of Taranto, since the city had started as a village on a coastal islet — like many Greek colonies in southern Italy. It was something the managers (none of whom came from Taranto) had never thought about.

Going back to the stories: initially each story went on its own, created on the basis of an episode I had been told and which I later de-contextualized. In 1981 I reached a full professor status in Bologna and happened to meet, in the courtyard of the Faculty of Political Sciences, Gian Paolo Prandstraller, who proposed that we should use the stories in a piece of research he planned to do in order to control empirically his belief that those who practiced older professions, such as doctors or lawyers, were more conservative, while those who practiced newer professions were more innovative.

I drafted a questionnaire which included several “stories”, some of them imagined on purpose. We involved as interviewers some of our students in the faculty, to whom Prandstraller gave fine lectures on the socio-anthropological dimensions; listening to his lectures I was able to attribute each of the stories formulated to one of these dimensions. I later adopted the particular skill needed in order to interview with stories as my subject in the program of a master’s course I was heading in Florence in those years.

Some students then adopted some of the stories for their own research, and/or begged me to write others on an ad hoc basis. In 2005 I wrote a book in which I presented the dimensions, each of them with a few relevant stories; for reasons of space I did not add the results of interviews already done by me or by students. Too bad because I had collected a lot of material and — being engrossed by other commitments and interests — never found the time needed to make it public.

A glossary of methodological terms, of which you appear to be among those responsible, is circulating on the net.

We must jump back: in Turin in ’68 there was a meeting of sociologists in which, given the climate of the moment, several young professors shelved the Association of Italian Sociologists, which was revived in 1971 in Rome (as AIS). One evening, during the general meeting of sociologists when our association was revived as AIS, Piergiorgio Corbetta, Antonio De Lillo, Franco Rositi, Luca Ricolfi and I gathered in a small room in the hotel and decided to set up a methodology section, which was the first section of the AIS. Rositi proposed the idea of launching a series in imitation of Sage’s and entrusted me with its direction.

In the first years De Lillo, Ricolfi, Rositi and me, as a steering committee, would often meet at the Franco Angeli publishing house in Milan, decide which volume proposals to accept, and above all we would discuss around a glossary of epistemological and methodological terms.

Usually the discussion was between Ricolfi, who owned a more technical view, and myself, with De Lillo mediating and Rositi abstaining. For each accepted volume we would choose one or two tutors who were competent on the topic; I was responsible for making sure, often by discussing with the author, that the language respected the glossary. As long as I was teaching and participating in conferences and competition committees, the influx of works was constant and the series grew: 20 volumes came out from 1993 to 2012. When I retired in 2012, the influx dwindled and in 12 years only 9 more volumes came out, the last one in 2020.

Are there people who have particularly marked your scientific career?

As I said, I entered the university as a full assistant to Sartori, whose conceptual and terminological precision I certainly inherited. But the teaching of social science methodology in the Faculty of Political Science in Florence was held by Alessandro Bruschi, who had graduated with Sartori. I was able to teach methodology in the faculty of political science in Catania, where I was called by Alberto Spreafico, who had been the supervisor of my thesis in Florence and then had won tenure in Catania. The dean of the faculty was Franco Leonardi, an excellent organizer, with whom I had lively epistemological discussions.

In 1981 I won a full professor position and was called to Bologna by Giuseppe Di Federico, who had appreciated my writings on a topic that political scientists were little concerned with, the judicial system.

Some research that particularly marked you as a scientific career.

In the 1970s Luciano Gallino, full professor of Sociology in Turin, led a nationwide research on people having two jobs; Emilio Reyneri was in charge of the Sicilian branch of this research and I was in charge of the analysis of those data. Personal computers were yet to come, and in Italy all data analysis was done by relying on one of three computing centres: the CINECA in Bologna, the CNUCE in Pisa and the CSATA in Bari. In Catania we referred to the latter. In those days, the data to be analyzed were entered into the computer by punched cards of 80 columns; in the computer centre these data were transferred to disks. Reyneri and I flew from Catania to Bari via Rome carrying long boxes each containing up to 2,000 cards. The CSATA operators transferred the data to disk. In order to analyze the data I used the remote job link of the physics faculty, located in the hills above Catania.

In the mid-1970s, the University of Michigan promoted a large international survey as an academic reaction to the events in 1968; the title (*Dissatisfaction, Protest and Change*) disclosed the reason for the research. I participated on behalf of Sartori, personally analyzing the 2,500 questionnaires gathered in Italy. I was particularly interested in two or three “thermometers,” an instrument that at the time was a novelty. I noticed that some respondents were giving systematically high or systematically low scores, despite the fact that the cognitive objects listed in each “thermometer” in order to be assessed were quite different from one another. To remedy this biased use of the instrument I transformed the raw scores into deviations from the mean of all the scores given in the battery, and called this procedure “deflation.”

The gimmick appealed to my colleague Ricolfi, who dedicated a conference of methodologists in Turin to it. In that occasion, I was careful to point out that before operating “deflation” one should consider whether the data situation recommended it: it was not a procedure to be applied all the time.

Then at some point you began the experience of summer schools.

Actually all I did was imitate the experience of summer schools that I had followed in previous years. I talked about it in Rome with Giovanni Di Franco, a graduate technician in the department headed by Statera. Di Franco told me that he knew of a convent in Viterbo that had just been renovated with Jubilee funds and had enough cells to accommodate a hundred people; so we our first summer school started there. It was a very rewarding experience that involved several generations of researchers; unfortunately, when I passed the age of eighty, I no longer felt sufficient energies to continue this experience, and with regret I had to give it up, passing it on to my pupils and students who are continuing it. In Italy, at the time, there was an American-style quantophrenic trend that I was trying to counteract with summer schools.

And did the South American experience bring anything interesting?

The South American experience was born from my office neighbour in Bologna, Giorgio Alberti, who had convinced the rector to set up a branch office in Argentina. As soon as I heard about it, I applied to teach a course on comparison. Alberti accepted, and I moved my course to Bologna in the second semester so that I could spend the colder months in Buenos Aires, whose climate was similar to Rome's. I must say that the level of the Argentine students was slightly higher than the students I had in Italy. I only regretted that very few

took the thesis with me: they thought that by staying in Buenos Aires only three or four months each year I would not follow their work.

What can you tell me about your pupils? Do they follow your directions enough?

I was able to get the best ones to do research theses, but few were planning a university career, given the paucity of career options. For many years the subject Methodology was in the social sciences curricula only in Milan, Florence and Rome.

And finally?

The activity I have tried to pursue in the sociological milieu has been a *tertium generis* between the preference for rather misty theories and the American-inspired quantophrenic orientation. I have criticized both without restraint, as well as the blind adoption of Anglo-Saxon terms in place of perfectly equivalent Italian terms. This has basically the contribution that I developed not only in teaching but also in my setting of the summer schools of methodology.

Thank you Alberto, there is one thing missing and I will add it. Social science research can be divided into two: before and after you, although your just but challenging precepts should be known and applied much more.