

## WAS THERE A SCIENTIFIC '68? ITS REPERCUSSION ON ACTION RESEARCH AND MIXING METHODS

## ¿HUBO UN 68 CIENTÍFICO? SU REPERCUSIÓN EN ACTION RESEARCH Y MIXING METHODS

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**ABSTRACT:** The author asks whether there was a "scientific '68", and focuses on aspects of two specific methodological proposals defined in the 1940s and 50s by the terms "action research" and "mixing methods", applied particularly to social sciences. In the first, the climate surrounding the events of 1968 contributed to heightening the participative element to be found –by definition– in "action research"; that is: the importance of making the research subjects themselves participants in the design, execution and application of the study of which they are the focus. This approach captured the democratic and anti-authoritarian spirit at the heart of the proposal, which was part of the prevailing climate in those days. The repercussions of 1968 on "mixing methods" focused on studying what had actually occurred, especially between the youth and workers, and therefore, particularly from the point of view of sociology and social psychology, using a "mixed methods" approach. The author explores the proposal of Norman Denzin; but traces the recent origins of both "mixing methods" and "action research" back to the proposals of mainly Kurt Lewin and the Chicago School.

**RESUMEN:** El autor se plantea si hubo un "68 científico" y se centra en dos aspectos que corresponden a dos propuestas metodológicas definidas en los años cuarenta y cincuenta del siglo XX con las expresiones "action research" y "mixing methods", aplicadas especialmente en las ciencias sociales. En la primera, el clima creado en torno a los sucesos de 1968 contribuyó a acentuar el sentido participativo que tenía, por definición, la "action research"; es decir: la importancia de que las personas investigadas participen en la elaboración, ejecución y aplicación de esa misma investigación que se hace sobre ellas. Se apelaba ante todo al fondo democrático y antiautoritario que latía en esa propuesta y que se había convertido en parte del clima general dominante en aquellos días. La repercusión del propio 68 en "mixing methods" se centró, por su parte, en el estudio de lo ocurrido (el estudio del 68 por tanto), sobre todo desde el punto de vista sociológico y desde el de la psicología social planteada sobre la base de "mixing methods". El autor se detiene en la propuesta de Norman Denzin; pero, tanto en el caso del "mixing methods" como en el de la "action research", se remonta a sus respectivos orígenes inmediatos y, por tanto, a las propuestas que partieron principalmente de Kurt Lewin y la escuela de Chicago.

**KEYWORDS:** Mixed methods; action research; May 1968; Kurt Lewin; Norman Denzin.

**PALABRAS CLAVE:** *Mixed methods; action research; mayo del 68; Kurt Lewin; Norman Denzin.*

The 1960s was a decade marked by the threat of war between the Soviets and the Americans, but also by “our deepening involvement in the disaster of Vietnam, the hippies’ summer of love, and the assassinations” of Martin Luther King and Robert Kennedy in 1968— as Brewster Smith notes in his memoirs (1986, pp. 31-32). On January 1968, Vietnamese liberation forces launched an offensive on 140 towns in South Vietnam. The streets of America and Europe erupted in rebellion, especially from May on. The movement was uneven. It was considered by many to be carnivalesque and puzzling. “*La réforme, oui; la chienlit, non*”, as the French president General de Gaulle said at the time. In French, *chienlit* refers to a carnival mask (Ross, 2002, p. 65). In fact, the results were confused, heterogeneous and discouraging for many of the actors. Nevertheless, this atmosphere of confusion pointed to a clear desire for a different style of politics (Ali and Watkins, 1998; Miller, 1994). The bibliography on this topic is impenetrable and uneven. I will mention only the very recent contributions of Michael-Matsas (2016) regarding the contemporary attitudes of Deleuze and Guattari; David Porter (also in 2016) on the subject of French anarchism in the same period; Barker (2015) on the evolution of Althusser at the time; and Robcis (2014) on the ethical turn of French thought in 1968. I am unaware of any substantive approach to the possible influence of May ‘68 on science. My aim is merely to point to this bibliographic contrast. In these pages, I seek specifically to highlight two facts: first, 1968 was an optimum time to apply “action research” —already an important concern in sociology; and second, some scientists saw an immediate need to extend the scope of their research in order to understand the events of 1968, and thus to mix data and methods, particularly in relation to the psychosocial demonstrations of these months.

This research therefore focuses on the concepts that clearly express both topics: “action research” and “mixing methods”. The relationship between action research and the events of 1968 is described in Eikeland’s work (2007, 2011; before, Rapoport, 1970). As far as I know, there is no connection to 1968 in the bibliography on the history of mixing methods. The first —action research— refers to the relations between anti-authoritarianism and research in 1968; and the second —mixing methods— concerns the methods applied to study such a complex movement of revolt.

## ACTION RESEARCH

The concept of “action research” emerged in the works of Collier (1945) to study ethnic relations in the US Indian administration, and in Kurt Lewin (1946) as a method of sociological research on minorities. They conceived this “action” as a type of study directly linked to a social group or community in such a way as to enable its members to participate actively in the research alongside the scientists. They believed the best way of doing this was for the subjects to collaborate throughout the whole investigative process: from the beginning (the research design) to the presentation of the results and the ensuing discussions about their applicability to improve the community (Bargal, Gold and Lewin, 1992, pp. 8-9; Karlsen, 1991, p. 147; Rosenwein and Campbell, 1992, pp. 134-7).

This was a manifestation of the so-called “turning to practice” in sociological research especially after the War. The long post-war period was in fact a very fertile time for new —or reinvigorated— methodological concepts (Shadish, Cook and Campbell, 2002). “Action research” (in the 1940s), “survey feedback” and “quasi-experimentation” (in the 1950s), and terms like “community work”, “encounter groups”, “organisation development” (in the 1960s) became common conceptual expressions in participative research in the general scientific methodology.

Several of these concepts had been in use earlier —“community work”, for example, was applied to the activities of the American Young Men’s Christian Association by Frank Ritchie in 1915—, but all these concepts shared the “turning to practice” that characterised the post-war period. “Multi-methods” and “mixing methods” would perhaps be the last main product of this “turning to practice”.

Anti-authoritarianism challenged almost every shibboleth of Western society in 1968, as Fraser recalled years later (1988, p. 354), and practitioners of “action research” became more radical. They criticised the scientific establishment as a whole, and its “positivist” aim of unifying science; they advocated bottom-up knowledge, from “the people”, and encouraged the possibility of an alternative research founded on indigenous methods of knowledge. As Villena observed in Spain in 1975, this did not mean rejecting science, but giving priority to experience understood as life itself. Some researchers tried to derive knowledge from life as an experience.

This was also true in the case of workers’ participation and the general strike of May ‘68. Of course, the

main issue here was not “action research”, but “action committees” (*comités d’action* in French), invented at the time to negotiate improvements in working conditions. Besides, the pretended disappearance of any distinction between intellectual and manual labour had a similar effect, as did the general dream of bringing about a Cultural Revolution based on the example of Mao’s China (see Ross, 2002, p. 78, p. 96).

## MIXING METHODS

This was also the time to highlight the importance of mixing methods and to rebuild –if necessary– its epistemological bases.

Throughout history, people have tended to confer significance on certain figures of speech that thus acquire an extra charge of symbolic meaning. This is the case of the term “mixing methods”, which existed and was used in the early 20th century, but did not become a key concept until later. Practitioners of mixing methods often mention Campbell and Fiske’s use of the expression “multi-method” in 1959 to study the validation of psychological tests. From 1968, however, this composite word broadened its conceptual scope to include such different disciplines as international relations (thus in Coplin and Kegley, 1971). This amplification was partially the consequence of a debate raging in the 1960s, particularly among sociologists, social psychologists, educationalists, pedagogues, experts in evaluation and other social scientists (Bryman, 2004; Yeasmin and Rahman, 2012). The work by Campbell and Fiske (1959), Campbell and Stanley’s book *Experimental and quasi-experimental designs for research* (1963) and other works had sparked a lively debate as to the real feasibility of achieving an objective quantitative validation of calculations that were partially or totally founded on qualitative elements. This imparted special relevance to the so-called “triangulation” method, whose use in social research Campbell and Fiske also explicitly advocated in 1959.

However, triangulation was no longer merely a technique for validating results. Since at least 1957 (in several works published the same year), Julian Stanley had verified that some actions often comprise two or more experimental variables, and surmised that these interactions were worth studying.

This represented the definitive triumph of triangulation in mixing methods, as advocated by Campbell and Fiske in 1959, and Campbell and Stanley in 1963. Campbell and Stanley saw that all these findings could

have a possible effect on the acquisition of more experimental knowledge about educational practice, and might also inform the orientation of this practice. It was one thing to mix qualitative and quantitative data in research, and quite another to adopt measures –based or not on this research– to improve teachers’ daily work. Routine educational practice tends to contain a balance of both qualitative and quantitative aspects, and innovations such as incorporating students with problems (quantitatively a greater efficiency) may lead to a qualitative decline if the teachers are required to lower the standard of the knowledge they impart, as observed by Campbell and Stanley in 1963.

## STATISTICS AS A NEXUS BETWEEN MIXING METHODS AND THE NEW PHYSICS BEFORE 1968

These debates on improvements in education coincided in time with the efforts of some mathematical statisticians (Roy and Gnanadesikan, 1959) to advance in another important issue: how to come up with models that combine different types of prior multivariate designs. The statisticians called them “mixed models”, an expression that Campbell and Stanley made their own in 1963. In statistical terms, mixing models went beyond educational research, and entailed mixing true scientific “methods” when the “mixing” concerned quantum mechanics.

In fact, the expression “mixing methods” had already appeared in Sakamoto’s *Statistical theory* (1965) on “the systematic sampling and mixing methods of bulk material”. This system had been developed in several fields of manufacturing in relation to particle physics and the problem of mathematically explaining its irregularities. Bulk material showed a “mixing mechanism” from the microscopic point of view. Particles revealed different-sized pieces and contents that were distributed in a heterogeneous material flow, and this led Sakamoto to apply the mathematical method of arbitrary functions to set up a mathematical model. He hoped that by following a suitable plan of “multi-stage grinding, mixing and bedding operations”, he could bring initial chaotic states of bulk material to a near homogeneous state. He thus related the concept of “mixing methods” to mixing different mathematical approaches in order to solve the important problem of “non-determination” in quantum physics in a specific application.

This view of Sakamoto’s works should not be overrated. Some experts believe that similarities between the terms used in current mixed methods research and other disciplines offer very little evidence for any

actual connections between them. Without such evidence, the relevance of the work of Sakamoto, for example, would appear to be extremely dubious; Sakamoto was referring to mixing materials, not methods, according to this criticism. As these experts saw it, the actual use of mixed methods in many of the natural sciences apparently had no impact on the self-conscious development of mixed methods research.

I do not agree. First, there are indeed actual connections. Sakamoto referred to Shewhart's (1924) pioneering explanation of the causal relation between the physical aspects of a given state of control and the quantitative aspects of the data obtainable under this state, and applied the fundamental independence theorem developed by Hopf (1931). Sakamoto then combined Hopf's method with Shewhart's solutions to Rutherford and Geiger's observations (1910) on the frequency of different numbers of alpha particles striking a screen subtending a fixed solid angle within a given interval. It was perhaps Hopf who suggested the concept of "mixture". He worked on the mathematical problems of radiative equilibrium, and wrote about mathematical combinations in which the concept of "mixture" could be considered to be explicitly accepted ("theory of mixture", "mixing mechanism", "mixture and statistic regularity" and "mixing flow"), in his own terms (Hopf, 1931, p. 53, p. 71).

This could be considered a further example of a mere coincidence between the terms used in current mixed methods research and other disciplines. Current mixing methods nevertheless has its own "protohistory", in which a key aspect is the adoption of modern physics as model of scientific research, and its influence on statistics. Models of probabilistic causation were developed as probability calculation became an important tool for solving problems related to the new physics, including statistics applied to research, as shown by the works of Thorndike, McCall and Chapman (1916).

I mention these as they were the basis for McCall's (1923) and Fisher's (1925, 1935) own proposals for research into education, particularly from an experimental point of view (Oakley, 1998). Edward Thorndike considered human actions to be little more than responses to stimuli, which could therefore be measured and quantified (Teddle and Johnson, 2009, p. 64). The work of measuring the relations between the traits of a man's constitution, events in his career and circumstances of his environment—he argued in 1913 (p. 141)—, both as they occur in nature and as modified by ingenious experiment, can increase our knowledge of his nature and our grasp of the facts.

Similar statements by McCall are also worth noting (for example, 1922). Fisher, however, believed that this involved statistics as a method, and conceived probability as the expression of the true structure of a person's nature, as suggested by evolutionary biology or quantum mechanics. Fisher himself devoted his post-graduate research in Cambridge to this: the study of quantum mechanics with the leading physicist James Jeans and the theory of errors with Frederick Stratton.

All these works were among the precursors of the conceptualization of "mixing methods" in the 1950s, despite their divergence, as explained by Campbell and Stanley in 1963. Oakley concluded in 1983 that Campbell and Stanley were to social research what Fisher was to medical research.

#### FROM HEISENBERG-SCHRÖDINGER'S GENERAL THEORY TO LEWIN'S GENERAL THEORY OF (SOCIAL) SCIENCE

The new physics also influenced the new social psychology through other routes. Fisher had accepted Heisenberg's principle of uncertainty in the 1930s (Louçã, 2008, p. 11) at the same time as Kurt Lewin was searching for a general theory of science.

Heisenberg's quantum mechanical theory (1925) and its mathematical development by Schrödinger (1926) not only had an impact on quantum physics. Heisenberg began with the assumption that the formal rules used in quantum theory for calculating observable quantities contained relationships that were apparently unobservable in principle, and with such basic elements as the position and period of revolution of the electron. Schrödinger's equation did not solve the problem. It worked very well, but did not seem to make sense. The quantum theory developed by both Heisenberg and Schrödinger and other physicists could describe with incredible precision the counter-intuitive behaviour of electrons, atoms and the tiniest particle in the submicroscopic world, and this mathematical precision also served to demonstrate their non-determinist behaviour. An atom could be on the left and right side of a box at the same time, meaning that this atom simultaneously had two mutually exclusive properties. This did not appear to be "logical", but was mathematically proven. However, this so-called mathematical "superposition" of the atom disappeared as soon as an observer opened the box to see where the atom was. In this observation the atom arbitrarily seemed to choose between the right and the left side, so another equally illogical observation was added to one that already seemed illogical (Seife, 2005).

The mathematical explanation of quantum mechanics lacked any evident physical foundation. A dynamic system like this did not obey the commutative law of multiplication, but only satisfied certain quantum conditions if there was a set of standardizing – uniformising– variables for this dynamic system. The problem was that there was no such set for a system containing more than one electron.

The conclusion was clear for Einstein, Podolsky and Rosen. As they argued in 1935, in quantum mechanics, specifically in the case of two physical quantities described by non-commuting operators, the knowledge of one precluded the knowledge of the other. Thus, the description of reality as given by a wave function was incomplete. Quantum physics was therefore an incomplete theory, and there had to be “hidden variables” which would be identified in the future.

Other physicists did not support this conclusion. As Brush noted in 1980, the outcome of the proposals and debates of Heisenberg, Schrödinger and others was that from 1927 to 1930, leading physicists accepted that atomic theory could provide only statistical predictions of experimental results. Schrödinger’s proposal showed that an atomic system could be represented by a wave in the co-ordinate space, and he thus obtained a fitted differential equation. This did not solve all the problems of quantum mathematics, but it offered a basis for a general theory that could be used as a framework to study systems with a very large number of degrees of freedom (Fradkin, 2011).

As far back as 1925, Kurt Lewin expressed his intention of seeking a general theory of science (Back, 1992, p. 54), and physicists now offered their own general theory. The time had come to test its analogical application to social sciences. This approach could be seen as a return to an earlier reductionism rather than as a “mixing of methods”. However, the impact of the critical world situation in 1929-1945 (the financial crisis and the Second World War) created the optimum conditions for Lewin’s proposal to serve as the catalyst that culminated in the conceptualisation of “action research” and “mixing methods”.

Lewin and his collaborators were driven by the social fallout from the crash of ‘29 and their unshakable commitment to social justice (Lee, 1986). They were all profoundly affected by the subsequent social experience of the Second World War, and particularly by such shocking events as the Hiroshima and Nagasaki atom bombs. For Lewin this was not only a personal or political reaction, but also the percep-

tion that the war had greatly accelerated the need for a change in social sciences towards a higher level of development (Smith, 1986).

Psychologists were the first to subscribe to these new attitudes. They saw the need to integrate the social sciences and move from merely describing social groups to tackling the dynamic problems of their shared lives, developing new techniques and instruments to help them in this ambition. Lewin’s most important contribution to social psychology was perhaps his approach of combining all the ideas mentioned above and the disciplines required for their study: a new “mixing methods”, which he preferred to express in different terms as a “combination” or similar (Lewin, 1946, p. 36). He understood that cultural anthropology, psychology, sociology, and economics had to be mixed in order to gain a deeper insight into social problems, and was convinced that this fusion of disciplines could be expressed in theorems similar to those of the physics of his day. Topology was only the first step in his proposal. Before the atom bomb, he argued, physical scientists considered social phenomena to be less “real” than physical objects, but Hiroshima and Nagasaki had changed the way people thought. The concept of “leadership” had taken on a new meaning, and social psychologists and economists were aware of this. Leadership was now more clearly dependent on group attitudes. Relationships had become more important than the units in the group (Lewin, 1947, pp. 5-10; also Maccoby, 1992, pp. 172-174). It was thus impossible to understand – let alone predict– human behaviour without first learning how humans perceive and conceptualise their world –their “life space”, in Lewin’s expression. “Life space” did not refer to the world in which human beings live, but the space of interaction between people and their environment (Bargal *et al.*, 1992, pp. 4-5; White, 1992, p. 49), in the line of Mead (1934, 1938) and Dewey (1934/2005).

Of course, Lewin was aware that all individual life spaces contain individuals and groups who can prescribe and impose appropriate patterns of behaviour (Maruyama, 1992, p. 158), but he believed there was also a world outside the life space that cannot be included in theoretical explanations of experience and action. This was important, as the boundaries between the external world and the life space are precisely the place where a set of behaviours and attitudes may be changed or completely reversed. In this interface a small change may have enormous

consequences (Back, 1992, p. 56, p. 60). The effectiveness of social practice in altering people's experience and behaviour is measured by the extent to which the boundary of their life space is permeated (Gold, 1992, pp. 72-73).

Lewin related this to the concept of "social field", as the totality of coexisting social entities. As in physics, the concept of "field" was essential. Social events depended on the social field as a whole. He believed the dynamics of social events were no different to any other scientific field, and sociometric and interview techniques, group observations and other tools offered reliable data to study groups from a quantitative point of view and in their entirety. Mathematical representations of social problems, topological geometric systems and vectorial psychology were seen as ways to research social problems, as Lewin explained in his *Principles of Topological Psychology* (1936).

He believed a social field must be studied following a "general field theory" (Lewin, 1947, pp. 5-10, pp. 13-14), an expression he also borrowed from Heisenberg and Schrödinger.

Some experts are not convinced that the concept of mixed methods research in social science partially arose from quantum theory, or even from Lewin's topological psychology and the practice of action research. However, Lewin's contribution to mixing methods was not limited to the practice of action research and his topological psychology, but was far more ambitious. His aim was to arrive at a global theory of science—thus, a global mixture of sciences and methods—although he only went as far as mixing cultural anthropology, psychology, sociology, economics (and topology).

## DENZIN'S '68

The key role of Norman Denzin in the possible social-psychological response to the events of '68 also gives rise to serious disagreements, as summarised below:

1. Mixing qualitative and quantitative data, the essence of mixed methods research.
2. Denzin did not invent modern mixed methods research.
3. The modern founders of mixing methods are Jick, 1979; Rossman and Wilson, 1985; Fielding and Fielding, 1986; Greene, 1989; Brewer and Hunter, 1989; Morse, 1991; Brannen, 1992; Creswell, 1994; Tashakkori and Teddlie, 1998, and others.

4. Denzin spoke of using methods for triangulation (i.e., convergence/validity) which is quantitative in orientation (which may be why he abandoned triangulation as he became more constructivist and post-modern). He probably did not even use the term "mixed methods" in his original work *The Research Act*, but "methods or methodological triangulation". Most importantly, in virtually all of his work after *The Research Act*, he ceased to support the idea of mixing qualitative and quantitative data (which is the core of MMR). Denzin went on to advocate in favour of qualitative research; he had no use for quantitative data and construed qualitative research as the opposite of quantitative research and its "positivism."

I only can endorse the second point, and the global vision of the fourth: Denzin did not invent modern mixed methods research, although the expression "modern" mixed methods requires a definition. As for the other two items, the readers have enough information in the previous pages to form their own personal opinion. My task is to clarify Denzin's role in the orientation of social psychology in the aftermath of '68, and for this purpose the first thing I must say is that his first important works appeared in 1970, when he was twenty nine years old. His 1970 works were namely *Sociological Methods* and *The Research Act*, and, as editor, *The Values of Social Science*. Denzin's last work on mixing methods was published in 2012.

It is clear that he was exercised by contemporary issues in the days after 1968. The editors of his *The Values of Social Science* (1970b) were very explicit; they noted how commonplace the word "crisis" had become among housewives, Wall Street lawyers, black militants and even professional politicians. As they also argued, social scientists had to apply a multidisciplinary approach—"from many points of view and from the vantage points of many disciplines"—to the study of US overseas policy and its contradictory attitudes to dictatorships, the uses and abuses of law enforcement agencies, the experience of blacks in American ghettos, realignments between nations, urban expansions and population explosions, massive shifts in norms of social conduct, dissident minorities and so on.

Denzin's proposal for addressing these problems stemmed from the thesis that the basic unit of naturalistic analysis had never been clarified, and this was the necessary starting point. In his reasoning, "naturalistic behaviourism", "symbolic interaction", and "theory"—understood as a cluster of hypotheses that involve interpretations—are *essential elements in research*.

“Naturalistic behaviourism” is a research strategy in which the researcher actively enters the social setting of the people being studied and renders this world understandable according to a theory based on the behaviours of the participants (Denzin, 1971, p. 66). He considered it a positive step for native people to take part in their own research, according to the conceptualisation of “action research”, but emphasised that it was important for researchers to work in such a way that they entered people’s minds in order to make their world comprehensible according to a theory grounded in the behaviour, languages, definitions, attitudes and feelings of the subjects. In Kurt Lewin’s terms, it could be said that the researchers should not only identify and study the “life space” of the people in their research, but become embedded and form part of this space.

Like Lewin, Denzin also believed that researchers must start from a personal theory, before entering people’s minds to compare this theory with their empirical findings. Theory, therefore, was also essential to Denzin as for many other scientists: he believed all the steps in the sampling process in sociological research must be theoretically guided.

In his thought, theory is more than a mere hypothesis. According to positivists, a theory must have three essential components: (a) concepts or constructs, (b) propositions or statements linking these concepts together, and (c) rules for connecting the concepts with the empirical world (Brannick and Coghlan, 2007, p. 63). Denzin can be said to place special emphasis on “interpretation”: researchers must observe people directly in their natural setting, and consider these people’s lived experience from the point of view of the men and women living it. Only this way can the researcher understand and interpret how people create and maintain their social world (Pedersen, 2007, p. 108). He may have been seeking a second orientation of “action research”: we must involve the people who are the object of this research, but we must also ourselves become engaged in the life of these people.

In sum, (i) we start with several specific theoretical propositions involving our interpretations; (ii) we determine the empirical relationships between our theoretical propositions and our interpretations of the data; (iii) we compare our propositions and these interpretations; (iv) we retain only the previous interpretations that withstand the comparison; (v) we select the proposals from the comparison; and finally (vi) we reformulate our theories according to these results.

All this implies that each act of research involves “symbolic interaction”, as human beings formulate their actions according to how they perceive the actions of others in a social setting. They do not respond directly to each others’ actions, but to the meaning they attach to such actions –in other words they tend to respond to presumed intentions rather than to actions, although these intentions may be erroneously attributed (Soloski and Daley, 1978, p. 39). This is the meaning of “symbolic interaction” (SI).

This last expression reveals Denzin’s commitment to the American theoretical tradition in social sciences. “Symbolic interaction” entailed linking his proposal to Blumer’s “symbolic interactionism” (1969), and Blumer was considered the heir to the Chicago school of psychology. This was where the relationship had been forged between the thought of Dewey, Mead, Blumer, and Lewin, and where the influence of the cultural anthropologists Frank Boas and Bronisław Malinowski held sway, especially on Dewey.

Some experts also doubted the connection between cultural anthropology and the development of self-identified mixed methods research. The work of Goldman (2012) provides a clarification.

For Denzin, more explicitly than for his predecessors, interaction requires mixing methods, as the most rigorous statistical procedures or the most loosely conceived theoretical strategy are not sufficient to resolve the problem of sampling in social matters. In “Place of Methods”, one of the first chapters of *The Research Act* (1970/1989, p. 13), he reasoned that if every method leads to different aspects of empirical reality, no single method can capture all the relevant features of this reality, and sociologists must therefore use “multiple methods” in their analysis of each empirical event.

I have been unable to consult a copy of the first edition. In the third edition he concludes: “This is termed *triangulation*”. Thus, in his thought, triangulation is more than a mere technique and even more than a mixture of methods. He converts triangulation into a true method, whereas other approaches imply it to be an essential technique for combining several data sources or data-collection strategies (see Newman and Benz, 1998, p. 82). For Denzin, all sociological reality is such that there is no single method, no single theory or single observer that can capture everything that is important in it. He does not see triangulation as simply a means of mixing qualitative and quantitative data, but as a way of

combining multiple data sources, research methods, and theoretical approaches. It is therefore necessary to practice “multiple triangulation”: data triangulation, researcher triangulation, theoretical triangulation, and methodological triangulation.

He explicitly links action research to mixing methods, and calls attention to the need to combine the interactionist observer with the interactionist analyst, and even to transform the interactionist observer into the interactionist analyst. Each research method tends to contain restrictions and biases that make it advisable to combine as many methods as possible (Denzin, 1970b, p. 5; also Denzin, Lincoln and Smith, 2008).

These aspects of a “scientific ‘68” cannot be denied, and nor can the role of Blumer’s 1969 work. In 1992, a new Denzin editor –Charles Lemert– launched the series *Twentieth-century Social Theory* with a book by Denzin, and wrote that Blumer’s 1969 *Symbolic Interactionism* had become “the locus classicus of SI”, and was notable for its “surprising suppression” of art and politics in American pragmatist social theory, “in fa-

vour of a symbolic interactionism seeking the status of a reputable sociological method.” Lemert argued that 1969 was “exactly the wrong time to invent a sociology as method”; and that “what Blumer was unable to do in the 1960s, Denzin is doing now” in his own *Symbolic Interactionism* (in Denzin, 1992, pp. ix-x).

Denzin had certainly not steered clear of politics in 1970. He distinguished several different roles in social scientists of this time: that of critic, activist, mere observer and salesman. If a scientist opted for activism, criticism had to be carried over into alternative and meaningful social action programmes. These activities could range “from involvement in protest movements, to strikes, political proposals, petitions and fund-raising”. The point is to achieve the proper mix of roles. “Above all the sociologist should never abdicate these standards for money, prestige or political pressure” (1970b, pp. 10-11). In the following pages, Ross Crumrine studied ethnic identity, Irving Horowitz wrote about black sociology... but Herbert Gans had to ask “Where have sociologists failed?”

But had they failed?

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