Thomas Osborne and Nikolas Rose

Do the social sciences create phenomena?: the example of public opinion research

ABSTRACT

This paper is an investigation into the philosophy and the history of the social sciences. Some philosophers of the social sciences have suggested that a key feature of the natural sciences is their capacity to create phenomena, and that the social sciences do not meet this criterion. We suggest, to the contrary, that the social sciences can and do create phenomena, in the sense of new ways of describing and acting that have been used to produce all sorts of effects. Like the natural sciences, the social sciences create their phenomena through the procedures that are established to discover them. But the creation of phenomena is a complex, technically difficult and contested process and its success rare. Historically, this argument is developed through a case-study of the development and evolution of public opinion research in the USA and Britain. We argue that by the 1950s public opinion produced a version of the world that had entered 'into the true'. Special attention is given to technical considerations in the development of public opinion research, especially the genealogy of a particular research technology, that of the representative sample. Whilst we are not concerned with demarcation criteria, we argue that there are some important differences between the social and the natural sciences; that the former have a less concentrated 'spatial mix' and a slower 'tempo of creativity'. None the less, in this particular case, the social sciences have played a key role in the creation of opinioned persons and an opinionated society.

KEYWORDS: Philosophy of social science; irrealism; public opinion; research technology; sampling

There is a school of thought in the philosophy of science that holds that the 'scientificity' of a discipline can be measured by the extent to which that discipline is creative of new phenomena (Hacking 1983; Bachelard 1949).[1] On such a view, the specificity of a science is not just reducible to a question of exactitude, epistemology or methodology but also relates to the material and technical factors that lead to such a creation of phenomena. It is not surprising that the paradigm of scientificity for such a tradition of thinking about the sciences has been the laboratory sciences. In the laboratory, technical apparatuses can be assembled which produce phenomena

British Journal of Sociology Vol. 50 No. 3 (September 1999) pp. 367–396
ISSN 0007–1315

that were hitherto unseen, or which – on some accounts – do not even pre-exist the activity of creation itself. The laboratory sciences are paradigmatic of what Randall Collins, in a stimulating paper, has called 'high-consensus rapid-discovery sciences' (Collins 1994). The proponents of these positions usually suggest that the so-called social sciences do not meet these criteria: they do not create phenomena. We disagree: the social sciences, we argue, have played a very significant role in making up our world, and the kinds of persons, phenomena and entities that inhabit it. This, at any rate, is the view we develop in this paper, taking as our example the phenomenon of public opinion.

NATURAL SCIENCE AND SOCIAL SCIENCE

What is specific to phenomena-creating sciences, for Collins, is that they (1) are characterized by a high level of consensus on what counts as secure knowledge, and (2) generate rapid discovery of new results. On the basis of what Collins calls, following the work of Bruno Latour, 'science-already-made' (Collins 1994: 158; cf. Latour 1987) they produce a fast-moving research front – the zone Latour calls 'science-in-the making' – where controversies are concentrated. For Collins, these two features are inter-linked: what makes the existence of rapid-discovery science possible is high consensus itself. It is because there is so much basic agreement on the results and discoveries of 'science-already-made' that scientists can be left to concentrate, at the level of 'science-in-the-making', on agreed areas of dissent at the research front. In fact, this process actually reinforces consensus and the sense of objectivity of what has been discovered. The consensus is enhanced by another characteristic of the natural sciences: their research techniques and instruments have a history which embodies the past of the science and the skills of previous generations of scientists, at the same time as they develop and extend them. Collins thus says that (3) research technologies have genealogies. Scientists build instruments – research hardware – which integrate past theories and research results and act as a platform for further research and the discovery of new phenomena. Galileo's lenses, for instance, represent, for Collins, a classic example of a tinkerable research technology in the natural sciences.

> Assembled in one way they produced a telescope, in another way, a microscope; combined with mirrors and prisms, they give rise to the analysis of light spectra. This, in turn, combined with techniques of the chemical laboratory for heating and separating materials, results in spectral analysis of the elements. Recombined with the telescope, the result is the study of stellar spectra, and so on, apparently endlessly. (Collins 1994: 171)

A further characteristic of the hardware of these high consensus rapid discovery sciences, for Collins is (4) their applicability. He suggests that this

typically takes the form of the export of laboratory equipment or its products into the wider, lay world. Hence the rather 'one-sided' relation between scientific intellectuals and the lay community; in return for various kinds of support to the sciences, non-scientific laypersons can count on receiving the material benefits of science (ibid.: 170).

Collins, as we have seen, uses the term 'discovery'. But he does not think of discovery in any epistemological sense. Discovery is a term which can be applied to the creation of phenomena once they have acquired the aura of objectivity. Thus Galileo and his followers hit upon the recognition that manipulating their research technologies – lenses and so forth

> would result in previously unobserved phenomena . . . [T]he research technologies gave a strong sense of the objectivity of the phenomena, since they were physically demonstrable. . . The theory of the phenomenon, and the research technology that produces it, became socially objectified simultaneously, when enough practical manipulation had been built into the machinery so that its effects were routinized. (Collins 1994: 163)

The contrast with the social and humanistic disciplines is, of course, meant to be striking. In these endeavours, intellectual distinction is more likely to be gained not so much from innovation at an agreed research front but in adherence to one or other basic lineage or school. As a multitude of commentators have pointed out, such sciences, if they can be called that, do not have the form of a research front of rapid discovery leaving a trail of cognitive consensus behind. On the contrary, their problems and solutions seem to follow a cyclical and backward-directed pattern. For example, they are characterized by recurrent commentaries on and rediscoveries of the classics in a way that would be unthinkable for rapid-discovery enterprises, where the research front moves constantly forward. The social sciences thus seem to follow the pattern of intellectual life in Europe over many centuries, including that of the natural sciences before the rapid-discovery revolution: stasis or circularity rather than progress is the norm and disagreement appears to be endemic at all levels. They thus do not appear to be rapid discovery enterprises, do not enjoy substantial consensus, and do not produce research technologies with the kinds of genealogies that characterize the electron microscope, say, or the particle accelerator. Further, as far as Collins is concerned, their applicability is poor. In sum, they have not succeeded in creating new phenomena (cf. Hacking 1983: 249). Mathematical and statistical methodologies and other innovations with regard to the extension of techniques and the attainment of exactitude and calculability have done nothing to mitigate this situation. Collins is pessimistic. Perhaps, he says, only some innovations in the area of the micro-sociology of interaction or of the development of Artificial Intelligence can save the day for sociology.

In what follows, we argue that, in spite of the many merits of Collins' account with regard to the natural sciences, such a pessimistic outlook with regard to the social sciences may be misplaced. Whilst not wishing to

downplay many of the distinctions between the natural and the social sciences that are implied in Collins' argument, we wish to focus on the central question of discovery, or, as we prefer to put it, the creation of phenomena. Our aim is not really to contribute to the longstanding debate about the demarcation of science and non-science, to argue that social science meets 'scientific' criteria in this demarcationist sense, or to claim that there is no difference between the natural and the social sciences on various of Collins' criteria. We merely want to argue that, in the sense in which Collins and others of a similar turn of mind use the term 'discovery', the social sciences have discovered a great deal. That is to say, the social sciences have brought, and can bring, many new phenomena into existence.

The social sciences can and do create phenomena. And, whatever their other differences, in this matter of the creation of phenomena, the social sciences work in ways rather similar to those paradigm natural sciences explored by Collins and Hacking. Of course, not all that is embraced within the actual existing social sciences and their history has been creative in this sense, and this distinction between creativity and non-creativity is, we think, of particular interest to those concerned with the current plight of these disciplines. Here we organize our discussion around but one example: a brief case-study of the problematic of public opinion research in twentieth-century North America and Britain.

CREATING PUBLIC OPINION

There can be little doubt that public opinion, today, is a reality – in at least some senses of the term. McDonald and King have a nice title for the book that they wrote to celebrate the 50th anniversary of the Market Research Society in Britain: *Sampling the Universe* (McDonald and King 1996). They estimate the world wide market for opinion research at around $10 million dollars each year. It is really since the end of the Second World War that the opinion industry has taken off. In the USA, the American Association for Public Opinion Research was founded in 1947 after a conference attended by about 70 researchers organized by Harry H. Field, director of the National Opinion Research Center then at the University of Denver: it now has over 1500 members. The Market Research Association was founded in 1954 and has around 2,400 members, there is a National Network of State Polls, a National Council on Public Polls and a Council of American Survey Research Organizations. The European Society for Opinion and Marketing Research lists over 1,100 research organizations in its directory and, in the UK alone, opinion research has become a £600 million industry: the British Market Research Society, founded as a luncheon club in November 1946 with 23 individuals, had 7,000 members by its 50th anniversary in 1996. There are specialized journals, conferences, training courses, professional associations with codes of practice and disciplinary committees and much more. The activity of politics, of advertising, of marketing, indeed public

debate itself seems unimaginable without reference to public opinion. And the term 'public opinion' conjures up, as its necessary technical aspect, the public opinion poll.

This is a cursory survey, but it is enough to show that public opinion exists, at least in the sense that it has a reality and an efficacy in the world in which we live. But in what ways can one regard it as a created phenomenon? And if it is created, is it an example of the creation of phenomena by the social sciences analogous to the phenomena created by sub-atomic physics or whatever? Of course, some would say that, despite all the activity around public opinion and justified in its name, the phenomenon is somehow an illusion, an artefact, ideological, pre-scientific. This would be the implication of the view expressed by Jürgen Habermas in his essay on 'The scientization of politics and public opinion' (Habermas 1970). Habermas is interested in this issue of 'the public' because, at least at this early point in his biography, he favours a democratic pragmatist model of scientized politics. He would like public opinion to be the mediator in a critical interaction between political will, as embodied in the political programmes and policies of politicians, and technical evaluation provided by scientific experts. Such public opinion is one in which citizens come to rational conclusions about available options based on rational discussion. But, he thinks, 'the empirical conditions for the applications of the pragmatist model are lacking. The depoliticization of the mass of the population and the decline of the public realm as a political institution are components of a system of domination that tends to exclude practical questions from public discussion. The bureaucratized exercise of power has its counterpart in a public realm confined to spectacle and acclamation. This takes care of the approval of a mediatized population' (ibid.: 75–6). If public opinion has been produced or created, then, it is very unlike the real thing: the genuine opinion that would arise from rational discussion in the public sphere. What we call public opinion today is the product of domination, manipulation and all those other processes denounced by the dismal prophets of the Frankfurt School.

Pierre Bourdieu comes to a similar conclusion from a different perspective. For him, public opinion polls represent, at best, a kind of 'spontaneous sociology' which is driven by the demands of the market and constrained by the institutional structure of the polling agencies, produce answers to problems only posed by those who are the customers of the polling agencies rather than those which their subjects pose to themselves, ignore the only interesting questions which are those of the economic and cultural determinants of opinions, and are scientifically useless because they neglect the kinds of controls on empirical research which are exercised by professional sociologists: polls usually measure nothing but the effects exercised by the polling instruments themselves (Bourdieu 1990: 168–74). For both Habermas and Bourdieu, then, public opinion *is* a created phenomenon, but only in the sense that it is one fabricated by shoddy operators and merchants of illusion to satisfy particular commercial, political or

professional demands: from the perspective of a scientific sociology, let alone that of a rational society, it should not exist.

These elevated criticisms from continental social theorists are matched by those developed at a more mundane level by workers in cognate fields (e.g. Marsh 1979; cf. Roiser 1987). Now we would not wish to deny many of these criticisms or defend the epistemological status of public opinion, or indeed of the other phenomena created by the social sciences – the juvenile delinquent, the class structure. . . But to argue that something shouldn't exist, or shouldn't exist in this form, is not to say that it doesn't exist. This is to conflate description and critique. The views which question the 'reality' of public opinion, rather mistake the kinds of realities that are brought into existence by knowledge practices. We tend to follow Ian Hacking here (Hacking 1983). In *Representing and Intervening* Hacking argued that the debates about the reality of the entities proposed by science could not be settled if one thought of sciences in terms of representation, that is to say, in terms of theories. But it could be settled when one considered the experimental or interventionary moment in science. When one entity postulated by science (say the electron) is used to investigate another phenomenon (say that of the weak neutral current) we can be realist about the entity, even if we are sceptical or anti-realist about the theory that claims to elucidate or account for it. More recently, Hacking has developed his position through a discussion of perhaps the classic 'constructionist' text in the sociology of science: *Laboratory Life* by Bruno Latour and Steve Woolgar (Hacking 1988, Latour and Woolgar 1979; reissued 1986). It is not a question, for Latour and Woolgar, of whether facts or reality exist: the word is full of facts, but those facts are inescapably the outcome of the micro-sociological processes that put them into circulation. The birth of facts is often preceded by controversy, but one cannot explain how the controversy settles by reference to the real existence of facts. It is because a controversy settles that a reality effect is produced and that objectivity is conferred on certain facts. And this involves a kind of splitting, which produces, simultaneously, a statement and the entity to which the statement is linked. Neither the truthfulness of a theory nor the body of phenomena to which the theory refers determines whether this reality effect is produced: this is the outcome of battles over truthfulness within thought, and through the deployment of a whole range of resources – laboratories, laboratory instruments, technical skills at the creation and mobilization of traces and inscriptions, and much more. At a certain point the controversy is settled. The phenomenon exists: now it is time for it to be investigated, explained, analysed, refined, purified, categorized, classified, utilized. It becomes a usable kind of thing, usable in science, usable elsewhere.

Now it would be mischievous to give the impression that Hacking goes along with the full 'constructionist' thesis of Latour and Woolgar – which he terms, in deference to Nelson Goodman, their 'irrealism' (Goodman 1978; McCormick 1996). In fact he suggests that the virtue of their account does not lie in its irrealism. Its virtue arises from the ways in which it

demonstrates that, of all the possible candidate facts that make up a phenomena, some are discovered rather than others. It is this historically contingent outcome that sets our research off in one direction rather than another. Hacking thinks that there are many potential descriptions that are true of the world and that correspond to different of its aspects. But the sociological, political, and technical events that establish the truth of one version close off other equally true versions. Research marches off in one direction, not in all the others that were possible. This contingent historicity of truth does not disqualify the truth status of the versions of the world we arrive at. But it does account for why some things become true rather than others.

This disagreement between Hacking and Latour is important. But for present purposes, we can mix them together without risking combustion, so long as we do not attempt to use this volatile combination to answer the impossible question of demarcation. If our concern is with what facts have come to be true in our world, how, and with what consequences, we can think along the following lines: that this is a matter of the emergence of certain descriptions rather than others; that some descriptions triumph over others through rather technical and practical matters arising from experimentation and intervention; that once they emerge, descriptions will survive if it is possible to do things with them and use them to produce effects. If these conditions are met, then we can say that a knowledge practice has created a phenomenon. And if we are content to think in this way, we can understand the ways in which some of the knowledge practices of the social sciences have indeed produced phenomena.

PUBLIC OPINION AS A RESEARCH FRONT

In January 1937, the *Public Opinion Quarterly* was established in the USA. This marks the date at which public opinion research became respectable – if hardly without its critics – from an academic point of view. The Foreword to the first issue of the journal stressed the importance and the novelty of research into the subjective propensities of collectivities of citizens (Carlson 1975: ix)

> A new situation has arisen throughout the world, created by the spread of literacy among the people and the miraculous improvement of the means of communication. Always the opinions of relatively small publics have been a prime force in political life, but now, for the first time in history, we are confronted nearly everywhere by mass opinion as the final determinant of political and economic action. Today public opinion operates in quite new dimensions and with new intensities; its surging impact upon events becomes characteristic of the current age – and its ruin or salvation?

We think that, loosely-speaking, public opinion research had, by 1937,

become something like a research front in Collins' sense. That is, public opinion had become a conceptual phenomenon around which rapid discovery was organized through the utilization of a research technology. And this technology had a genealogy that embodied the results of previous discoveries and their associated techniques and skills. Of course, the social sciences had long been concerned with something like the subjective propensities of collectivities. But hitherto, on the one hand, this concern had tended to be directed not so much at public opinion as – for instance, in the early years of the twentieth century – at the more general problem of the crowd or the mass, and on the other, this question of collectivities was seen in more or less universally 'negative' terms.

Thus, for psychologists of the crowd at the end of the nineteenth century, the mass was an intrinsically dangerous or 'negative' kind of collectivity. In the crowd, thought Le Bon, in 1895, we could observe

> the disappearance of the conscious personality, the predominance of the unconscious personality, the turning by means of suggestion and contagion of feelings and ideas in an identical direction . . . [T]he individual forming part of a crowd . . . is no longer himself, but has become an automaton who has ceased to be guided by his will. . . Isolated he may be a cultivated individual; in a crowd he is a barbarian' (Le Bon [1895] 1966: 32).

As for public opinion, in so far as the concept was used, this tended to be restricted to the middle and upper classes and in deliberate contrast to what was seen as the more irrational habits of the mass. That, in any case, was how American political thinkers of the nineteenth century regarded the issue. William Mackinnon, to take an early and prominent example, wrote in 1828 that: 'Public opinion may be said to be that sentiment on any given subject which is entertained by the best informed, most intelligent, and most moral persons in the community, which is gradually spread and adopted by nearly all persons of any education or proper feeling in a civilized state' (quoted in Qualter 1985: 8; cf. Speier 1952; Lee 1947). Such a concept was therefore political rather than sociological; one could not 'measure' the aggregate of public opinion in Mackinnon's sense because that notion was essentially evaluative – public opinion was the attribute only of those qualified to have opinions.

By the 1920s, however, the notion of public opinion had become democratic. That is, political and sociological commentators began to argue that one could speak of the public in terms of 'the people as a whole'. It appeared to the participants in these debates that the USA was in transition from an elite society to a 'mass society', and that a new kind of democratic government had to be invented to cope with the challenges posed by the relative decline of the small town and the family farm as the dominant mode of American life, the progress of science and technology, immigration, the migration to the towns, the inexorable rise of large industry and powerful industrialists, the internationalization of economic relations

and so forth. The traditional practices of local American democracy could not measure up to these new demands. Herbert Croly and Walter Lippmann were only two amongst many writers in the first three decades of the twentieth century who argued that a more conscious ordering of American life was required to cope with these demands (Croly 1909; Lippmann 1922; cf. Karl 1983). But what did democracy mean in this new context? What role could and should 'the mass' or 'the people' play in the direction of their own society? It is here that the question of public opinion became relevant. The issue was controversial. For some, public opinion was a significant resource for politics, even if it was somewhat lacking in abstract rationality (see the discussion in Ross 1991: 293–300). Sociologists and political scientists of a Progressive persuasion – Abbott Lawrence Lowell, for example, or Charles H. Cooley – thought that such public opinion was certainly sufficient for setting the basic moral directions of policy, and for choosing those who would govern. For others, however, the public opinion produced by 'mass society' was given a more 'negative' evaluation. That is to say, such public opinion could be dangerous; not a correlate of the good government of a democratic polity, but a threat to it. Walter Lippmann's *Public Opinion*, published in 1922, addressed this question directly. Lippmann's problem centred on the limits to good – that is, secure – democratic government. Lippmann argued that democracy is only governable on the basis of a knowledge of public opinion; one has to know what it is that people are thinking before one can govern them, hence 'personal representation must be supplemented by representation of the unseen facts' (Lippmann 1922: 31). But Lippmann was not exactly concerned with public opinion as a matter of democratic principle. That is to say, he did not think that those who governed should seek to discover public opinion in order to follow its lead. On the contrary, public opinion had to be made an object of knowledge because it was so potentially dangerous. Above all, the aim of a knowledge of public opinion, Lippmann thought, was to ward off unwelcome surprises and *coups d'authorité*

> Insecurity involves surprises. . . Every democrat feels in his bones that dangerous crises are incompatible with democracy, because he knows that the inertia of the masses is such that to act quickly a very few must decide and the rest follow rather blindly. (ibid.: 271–2)

In 1922, he thought that the question for those who governed was to form public opinion: controversial questions should be thrashed out before government commissions in order that 'a public opinion in the eulogistic sense of the term might exist' (ibid.: 405). By 1925, in *The Phantom Public* he took an even more negative view: 'I set no great store on what can be done by public opinion and the action of the masses' (Lippmann 1925: 199). Harold Lasswell summed up his argument in his review: Lippmann now thought that 'the public is incompetent to decide with wisdom and justice upon the substance of a policy because it is doomed to act from outside upon those who have inside knowledge and responsibility' (Lasswell

1926: 534). The public should be trained to perform the only reasonable task that can be expected of citizens, who are far from omnicompetent: not to set the substance of policy, but to evaluate whether or not those who govern them have followed reasonable procedures in making, enforcing or amending rules.

The upsurge of popular irrationality that was discerned in the First World War did little to encourage those who placed their faith in the opportunities presented by public opinion for good government. The experience of warfare tended to re-assert the association between the idea of the collectivity and the phenomena of irrationality. For example, in 1916, Gustave Le Bon was drawing attention to the affective, irrational consequences of warfare;

> war touches not only the material life but also the thinking of nations . . . and here we meet again the basic notion that it is not the rational which manages the world but forces of affective, mystical or collective origin which guide men. The seductive promptings of these mystical formulas are all the more powerful in that they remain rather ill-defined . . . immaterial forces are the true steerers of combat. (quoted in Virilio 1989; cf. Rose 1990: 27)

But despite the popularity of this kind of reflection, optimistic assessments of the linkage between democracy and opinion predominated in the USA in the 1920s and 1930s. At one level, they resulted from a new way of thinking about the phenomenon: opinion was not a kind of supra-individual conscience or feature of a mass: it was only the aggregate of the opinions of individuals. None the less it showed a certain relatively durable patterning and exhibited a durability over time such that it could be spoken of as a phenomenon in its own right (see the discussion in Rose 1996: 116–49). Opinion, here, was linked up with one other key concept: attitudes. Floyd Allport played perhaps the key role here, in disciplining the notion of attitudes and claiming attitudes for science: his article 'Towards a science of public opinion' was the opening piece in the first issue of *Public Opinion Quarterly* in 1937. Public opinion, said Allport, should not be personified as a supra-organic being, generalized in phrases such as 'the public wants so-and-so', seen as superior to individual views or confused with the journalistic presentation of opinion. It was the arithmetic sum of individual expressions in favour of or against certain definite conditions, person or proposals which had a probability of affecting action directly or indirectly towards the object concerned. (Allport 1937; many of the arguments here had been set out in his earlier textbook of social psychology: Allport 1924; see Roiser 1987). Not, of course, that everyone shared Allport's behaviourism; the effects of his scientific manifesto were at a more diffuse level, linking the possibility of a science of public opinion with the concepts and methods of attitude research.

However, the emergence of a more 'positive' public discourse on opinion cannot really be put down to these theoretical advances alone. The

arguments were practical, even technical. They focussed on the invention and development of a particular kind of technique for measuring the opinions of collectivities – the *opinion poll*. If we wanted a date to mark a threshold here, we could choose 1935. This was the year when George Gallup established the American Institute of Public Opinion, with a dozen staff, 200 part-time interviewers, and regular 'America Speaks' columns in the *Washington Post*, the *Los Angeles Times*, the *Cincinnati Enquirer* and around sixty other newspapers (Robinson 1996: 85). This date recommends itself because, in Collins' terms, this was the date at which one could identify the organization of researchers around their research technologies. Here we can see the routinization of a technology that embodies within itself an array of practical manipulations such that it can reproduce the phenomenon in question at will and relatively consistently, a technology that produce a fast moving research front, generates new findings, simultaneous reinforces the sense of the objectivity of the phenomena described and of the theory and technique which produce them. In these respects, the opinion poll by the late 1930s can be seen as quite a good equivalent to Collins' notion of a research technology in the natural sciences.

This is not to say that Gallup was the first; that he was akin to a sort Newton or Galileo of public opinion. The research front was much more anonymous than that. Gallup's own involvement dated back to 1922 when he interviewed households for the *St. Louis Post-Dispatch* about their newspaper reading habits (Robinson 1996: 83). By 1932, following a spell as a consultant for Lever Brothers, Gallup was Vice-President and Director of Research of the advertising firm of Young and Rubicam conducting studies of consumer preferences and purchasing habits. The American Institute of Public Opinion was set up in his lunch hours. Gallup's first political opinion poll was conducted in 1933, but of course there had been many polls in the USA prior to this. For the most part, such interest was of a political order, focusing above on presidential elections. Straw polling in presidential elections dated back at least to 1824 when the *Harrisburg Pennsylvanian* undertook a 'straw vote' to measure presidential sentiment in Wilmington, Delaware (Teer and Spence 1972; Abrams 1951). It was only in the early twentieth century, however, that research began in earnest. In 1904 a poll was carried out by the *New York Herald* before the presidential election and repeated this for subsequent elections up to 1916. But, oddly, it was the farming community in America that took a particular interest in polling. The US Department of Agriculture was the first government department to conduct surveys, and in 1912, the *Farm Journal* started a poll on the US election. By the presidential election of 1928, over 80 polling organizations were operating, though only six of these were nation-wide (Robinson 1934). By 1935 there were political polls in the *Women's Home Companion* and, from 1936, in the magazine, *Fortune* (Parten 1950: 24).

Gallup's great coup, as is well known, was his prediction, in 1936, of the presidential vote; or rather, his prediction that the poll conducted by the *Literary Digest*, previously regarded as the most reliable, was going to miss

the mark because of its sampling methods. The *Literary Digest* had begun its polling in 1916: it sampled its respondents on the basis of telephone subscribers, automobile owners and registered voters. Gallup argued that the method of polling conducted by the *Digest* over-emphasized the views of the upper economic levels of the electorate. The basis of the opinion poll, for Gallup, was the representative sample

> If a sample is accurately selected, it represents a near replica of the entire population. It is a miniature electorate with the same proportion of farmers, doctors, lawyers, Catholics, Protestants, old people, young people, businessmen, laborers and so on, as is to be found in the entire population. (Gallup 1948: 22–3)

We shall return to the issue of the representative sample presently: it is a crucial element of the genealogy of the research technology of the opinion poll. On the basis of such a representative sample of a mere 300,000 persons, Gallup claimed in 1936, one could predict figures accurate to within 3 points for all doubtful states and for all other states with a sizeable population. In 1936, the *Digest* distributed some 10 million ballots using its usual methods, and on the basis of 2 million returns it predicted a win for Landon of 56 per cent. Gallup's Institute of Public Opinion dismissed this poll, arguing that the influence of those in lower income groups would lead to a win for Roosevelt, and forecast a win for the New Deal coalition. When Franklin D. Roosevelt was elected, Gallup's claims for the methods of representative sampling were vindicated. Following on this success, the British Institute of Public Opinion was set up by Dr. Henry Durant in 1937, having its first success in predicting the winner of a by-election in 1938, and this was to be a part of an international organization, with Gallup Institutes set up in France in 1937 and in Australia in 1938. In the following years, using the techniques of representative sampling, Gallup's interviewers would conduct face-to-face interviews on questions selected by Gallup in locations across America, chosen to reflect appropriate proportions of urban and rural, town and city, interviewing pre-determined quotas of individuals made up with regard to such factors as age, sex, socio-economic status and political affiliation. The sampling methods actually used proved on later examination to be less than representative in relation to women, black Americans and the lower social classes. None the less, probabilistic sampling was the basis of a widespread programme of investigation of public opinion in the next few years. Americans were asked their views on such diverse issues as 'Roosevelt's popularity, the sterilization of habitual criminals and the insane, the merits of women jurors, jail sentences for drunk drivers, sit-down strikes, New Deal legislation, the humaneness of modern warfare, and birth control' (Robinson 1996: 88).

Gallup's polls represented not just a technique of social research but a particular and optimistic vision of the function of public opinion itself. Opinion, here as with Lippmann, still related to the question of the most adequate forms of governance in the specific context of democracies. But

Gallup took the view that democracy actually needs public opinion to function; not that the idiosyncracies of public opinion might undermine democracy but that the monitoring of its profile is actually constitutive of what democratic governance might be. 'The value of polls to democracy', said Gallup, 'is written into the record of this era' (Gallup 1948: ix). In simultaneously creating and monitoring public opinion, the opinion poll could play its part in defining a new type of democratic politics. No longer, said Gallup, was it possible to think of the people simply as an ignorant 'boobocracy'. Rather, public opinion research showed that the people 'have displayed such good sense, and have made such a good record, that the faith of many persons in the basic premises of democracy has been rekindled' (ibid.: x). Moreover, the existence of public opinion research, Gallup thought, served to promote an interest in public issues amongst the populace themselves in a kind of virtuous circle

> By injecting the element of controversy, by showing the division of opinion, in fact by helping to simplify major issues by expressing them in language understandable to the great mass of people, polls have helped to increase public interest in many national issues. (ibid.: x)

By the time Gallup and Rae had published *The Pulse of Democracy*, in 1940, it already appeared that the public opinion poll was vital to government with the consent of the governed, and, as a consequence 'the common man, the farmer, the industrial worker, the stenographer, the clerk, the factory hand must become as politically articulate as the professional man, the businessman, and the banker' (Gallup and Rae 1940: 125, quoted in Robinson 1996: 89). Public opinion was democratizing, but for it to be democratizing, the public had to acquire and express themselves in the form of opinions.

WHO NEEDS PUBLIC OPINION?

It would not be until the 1940s that politicians in the USA and in Britain would come to feel that they really needed public opinion in order to govern. Practical public opinion research gained its impetus in the first half of the twentieth century for reasons that were less directly to do with matters of politics. Such impetus came, broadly speaking, from three kinds of concern: they were bound up with the rise of mass markets, the expansion of mass communication and the experience of 'total war'. First, chronologically and perhaps also in importance, were the demands of commercial and consumer research. Firms and companies wanted to sell goods. To sell on mass markets, amidst rising competition and fluctuating economic conditions, it was a help to know what consumers actually wanted. The first major British textbook of market research appeared in 1931 (Platt 1991). That year also saw the publication in America of Percival White's *Marketing Research Technique*. But those in the commercial world scarcely need textbooks either to get them started or to keep them going, and practical

techniques of consumer research clearly predate this time. House-to-house interviewing of prospective consumers had begun as early as 1916. The psychological profession inaugurated its consumer research activities in 1921 with the foundation by Henry Link of the Psychological Corporation, an association of psychologists farming out their services to industry (Abrams 1951: 65–6). In 1926, the General Foods Corporation selected 'a panel of leading homemakers across the country' to test recipes for jam and jelly (Parten 1950: 37; Converse 1987: Ch. 3). Also, for anyone wishing to connect these developments to the procedures of Fordist mass production, one might add the year 1933, and the establishment of the Consumer Research Staff at General Motors, conducting questionnaires into the attitudes of car owners and prospective car buyers (Weaver 1941).

Second, technological innovations in mass broadcasting also acted as a spur to research into the feelings and concerns of publics. Once again, the key dates were in the 1930s. In the British context, in 1936, the BBC established a unit for listener research; they wanted to know who listened to what and why. This panel consisted of a continuous survey of listeners, listening panels gauging the impact of particular programmes, and local correspondents acting as 'sounding boards of public opinion' (Silvey 1944). In the USA, radio research is associated in particular with the name of Paul Lazarsfeld. In 1933 when Lazarsfeld first arrived in the USA, he brought with him something like a decade's worth of experience in radio research in Vienna in the 1920s. First of all, Lazarsfeld set up the Office of Radio Research at Princeton, then later in 1940 he moved to Columbia to the Social Research Laboratory. One of Lazarsfeld's employees at Princeton was the daunting Hegelian thinker, Theodor Adorno, who wrote scathingly about the effect that radio had on the performance of symphonic music – perhaps not quite the sort of research that was expected by those who financed the research (Wiggershaus 1995).

Third, the experience of warfare was instrumental in the consolidation of the belief that those who governed needed to know the opinions and attitudes of those who they governed. By the time of the Second World War, the morale of citizens was considered as a key component in the military fortunes of a nation. Indeed this was perhaps the first 'total war': both the Allies and the Axis commanders targeted their assaults as much upon the morale of the civilian populations of the enemy nations – their fortitude, their support for their leaders, their willingness to suffer privations in furtherance of their cause, their beliefs as to their prospects of victory or defeat – as on their military forces (Rose 1990: Ch. 2). If morale was under attack by one's enemies, morale had to be fostered by government itself. And to foster morale one needed to be able to assess it and understand it, in order to develop, adjust and evaluate policies towards it. This gave political polling a new, and crucial, governmental role. Actually, it was not entirely new: twenty years before the development of sophisticated public opinion polling for the war effort in North America, the British government had embarked upon a project of almost continuous, if surreptitious, opinion

testing and propaganda – evidence that the aspiration for government through opinion pre-dated long pre-dated its technical realization (Middlemas 1979: 337–8). The Second World War only intensified these kinds of investigation; notably, in Britain, with the establishment in Britain of the Ministry of Information under Duff Cooper (McLaine 1979). It started out as a bit of a joke, with sarcastic comments about 'Cooper's snoopers' and complaints that ancient English liberties were being violated by shady characters asking survey questions. But by the time of air raids over Britain's cities, the state of civilian morale became a key factor in the war effort.

By 1941, the very idea of citizenship took on an intrinsically subjective form (cf. Rose 1990: 32). Of key significance in Britain was the Wartime Social Survey, an organization initially composed of some fifty-five specially trained fieldworkers, plus regional investigators, inquiring into all manner of policy issues as they affected the public: 'It is concerned with social problems, in the investigation of which it aims at establishing facts and the attitudes of the public towards these facts' (Box and Thomas 1944: 151; Gray and Corlett 1950; cf. Abrams 1951: 124–5). In the USA the idea of the morale survey, based on the techniques of attitude scaling developed by Rensis Likert and the techniques used in investigating group relations in industry and elsewhere, became the basis of a major programme of public opinion research to study of the effects of the war on civilian morale. This involved the National Opinion Research Center, founded in 1941, the Office of Public Opinion Research, together with researchers like Floyd and Gordon Allport and commercial organizations such as Gallup and Fortune. There was a Committee for National Morale, the 1942 yearbook of the Society for the Psychological Study of Social Issues was devoted to morale and much more. The Survey Division of the Office of War Information played a key role in linking up the opinions of the population to the machinery of government: there were programmes for the maintenance of morale in industry, amongst the civilian population and in the military machine. (Rose 1996: 129–30; cf. Bradburn and Sudman 1988: 25). And, in the war machine itself, Samuel Stouffer and his team in the Research Branch of the Information and Education Division of the US War Department engaged in a valiant attempt to use the techniques of polling and surveys of attitude and morale in military administration: a mode of government not of whole populations but of organization life in the light of the subjective opinions and preferences of the individuals who made it up (subsequently published in four much discussed volumes under the general title *The American Soldier*: Stouffer et al. 1949a, 1949b, 1950a, 1950b).

In short, in the couple of decades surrounding Gallup's great poll coup of 1936 – roughly between 1925 and 1945 – and in several distinct ways, the exploration of popular opinions and the prediction of popular desires became a field of concern and research, indeed a research front of sorts. Obviously this research front did not have the relatively clear-cut profile that one might expect to be a feature of the natural sciences. For one

thing, opinion research was by nature of its objectives clearly not particularly amenable to the intense, concentrated investigations – for instance in the laboratory and the University – which can produce the rapid transformations that often characterize research fronts in the natural sciences. For another thing, closely-related, the invention of public opinion research was not initially the province exclusively of people – like those later involved in *Public Opinion Quarterly* – who specifically called themselves sociologists or social scientists. As one of its most notable professional exponents, Paul Lazarsfeld stressed in 1957, psychologists and sociologists did not participate significantly in public opinion research until the mid–1930s; and until that time it was, for the most part, the territory of market researchers, industrial engineers, agricultural administrators, and newspaper and magazine editors (Lazarsfeld 1957). And, in fact, despite our earlier dates, the couple of years between 1946 and 1948 might be taken as the moment of *routinization* of polling. It was then that the professional associations were formed in the USA and in the UK. But perhaps more significant, by 1948 the polls had been sufficiently accepted for their universal failure to predict the winner of the presidential election to be a scientific problem: the object of an inquiry undertaken by the US Social Science Research Council.

THE CREATION OF PHENOMENA

We thus think we are justified in claiming that from this point on public opinion *existed*: the version of the world that could be produced under this description had become true. Public opinion consisted of the views of the individuals who collectively made up the people, now understood as the thoughtful and rational citizens of a democracy. Despite later attempts by sociological critics of mass society and technocratic social science, the controversy has been settled and doubts about the very existence of this phenomenon are eccentric and marginal. On the basis that public opinion exists, it can now be charted in more detail, techniques refined, explanations proposed, classifications developed.

But we need to retrace our steps a little if we are really to understand what is involved in the creation of phenomena. Like the phenomena discussed by Collins, Latour and Woolgar and Hacking, public opinion is created by the procedures that are established to 'discover' it. The phenomenon of opinion is an artefact of the technical procedures that are designed to capture it. It is determined by *technical* considerations: for instance the assumptions that go into the construction of a representative sample (we have already mentioned the 'biased' composition of Gallup's samples: c.f. Robinson 1996: Ch. 2). This, it should be said, is not to denigrate the achievements of public opinion research. On the contrary, the artifactual aspect of public opinion brings it closer to the model of the successful natural sciences. As Lazarsfeld put it

> . . . what is overlooked [by the critics of polling] is something that has happened often in intellectual history; a new technique has permitted the sorting out of various aspects of a diffused concern and has prepared the way for a more rational approach to the different elements. (Lazarsfeld 1957)

Just one particular innovation – that we have already mentioned – was of absolutely fundamental significance here. None of the research aspirations with regard to public opinion could have been attempted, let alone realized, without the representative sample. The representative sample is probably as important to the social sciences in the twentieth century as the telescope was in the sixteenth.

It is easy to *aspire* to know the wills, desires and expectations of everybody in the nation. Indeed, one can find many examples of such aspirations throughout history. Lazarsfeld cites as a precursor of public opinion research, the efforts of Napoleon III under the Second Empire, collecting reports on the citizenry; 'These were not the usual reports of the Secret Police, denouncing individuals, but rather were detached impressions of how various social groups responded to the policy of Napoleon III' (Lazarsfeld 1957). This strategy no doubt used a model of information-gathering centred upon particular 'gatekeepers' of knowledge; prefects of police, those in authority. Indeed, most survey research in the nineteenth and early twentieth centuries was carried out in this way. Charles Booth, for instance, in his classic *Life and Labour of the People of London* relied for his testimony not upon the direct accounts of those in poverty but upon trustworthy informants; the ones he used were those who deployed a practical authority over the circumstances of poverty: School Board Visitors (Booth 1892–7). Of course, all survey research requires mediation by expertise of some sort, but the sample – by reducing the scope of the investigation and enhancing what might be called its intensity – brings investigation, as it were, closer to the material. Indeed, a 'social' conception of opinion is only possible on the basis of survey investigation that uses sampling methods; that is, which attempts to operationalize a cross-section of opinion as representative of a general social whole (Seng 1951; Kruskal & Mosteller 1990). Let us consider this matter of the sample in a bit more detail.

Our argument is that the history of the representative sample serves quite well as an instance of what Collins calls a 'genealogy of research technology' in the social sciences. Today the sample has precisely the quality that Latour calls 'science-already-made'. From the perspective of the present, it seems more or less completely obvious and un-problematic. Controversies may abound as to the technical operationalization of the representative sample, but the sample itself is a fully accredited instrument of research. Yet, during the period when the research technology was being assembled – the time, so to speak, of 'science-in-the-making' – things were by no means so clear; a host of researchers of different kinds clustered around the question of the applicability and scientificity of the representative sample.

The proponents of sampling techniques did not treat it as a more or less obvious and neutral surveying device but as an imaginative discovery of some considerable importance. To some, it even seemed as if sampling technology would bring the social sciences up to date with the natural sciences

> Engineers work out the strength of certain materials by testing certain samples of them in the laboratory; the quality of the contents of a grain elevator is determined by a small sample; doctors have found the effectiveness of treatments by investigation of relatively few patients; a doctor determines the blood count of a single patient from a small blood sample. There is hardly a field of human activity in which we do not find sampling methods in practical use. (Cantril 1944: 129)

The notion of a genealogy, of course, implies that there is no simple or single lineage of a research technology: like a family tree, although it is easy to start from the point that interests one, the lines of descent and lineage soon diverge and diversify in multiple dimensions. Or, to put it another way, there is no single point of origin for a research technology. None the less, one important event in this genealogy occurred in 1906. This was the year when A. L. Bowley of the London School of Economics gave his presidential address in Leeds, to the Royal Statistical Society (Bowley 1906; c.f. Bowley 1913a and b; and 1915). His 1906 lecture took the form of a plea for the merits of the method of representative sampling. Bowley, it needs to be said, was not specifically interested in the question of public opinion; his main research interests related to employment and poverty. Nor was he even the first to call for, or even to practice, methods of representative sampling (Converse 1987: 41; cf. Desrosières 1991: 237). But, if only for its clarity and the importance of the forum in which he made it, his plea was something of a landmark moment: 'It is frequently impossible', Bowley said, to cover a whole area, as the census does, or as Mr. Rowntree here [in York] and Mr. Booth in London successfully accomplished, but it is not necessary. We can obtain as good results as we please by sampling, and very often quite small samples are enough; the only difficulty is to ensure that every person or thing has the same chance of inclusion in the investigation' (Bowley 1906: 553). Bowley emphasized that samples could be reliable tools, even that they could be more reliable than more extensive methods; because the accuracy of a sample was dependent not upon the size of the sample relative to the size of the whole, but only on the absolute magnitude of the sample itself (Bowley 1913b: 673; cf. Edgworth 1913: 175; Hilton 1924).

The representative sample obviously had a number of advantages over other kinds of survey. Such samples were regarded as being good on depth as well as scope; they allowed for an intricacy of questioning that was not possible in more extensive surveys. Because of their scale, samples allowed for the possibility of a degree of detail and exactitude that was not previously imaginable in social scientific research; they made for measurement rather than mere surveillance. With a small representative sample, the

researcher could introduce controls, and could even measure intensities. Moreover, the possibility was introduced of something like an experimental ethos with the introduction of a sampling approach; with the sample, the social world could become knowable at a new kind of, as it were, virtual depth. Yet, conversely – aside from this narrowing of the social gaze – samples also presupposed a wider surveillance. That is, in order to take a sample of the whole, one had to know the general features of the whole of which the sample was just a sub-set. Bowley, for instance was concerned precisely with this issue in Reading: 'Before one can get sound information from samples we must have a method of numbering our classification by persons or by district. If we had a definite system of registration and identification, as in Germany, it would be easy to choose, say, 1 in 100 or 1 in 1,000 at random from among all the persons whose record satisfied certain conditions, and then to investigate more carefully the history and circumstances of those chosen' (Bowley 1913b: 555).

Of course, we could trace this research genealogy back even further, to the mathematical and statistical techniques that made it possible to think of problems, or samples, in terms of 'representativeness' at all. The International Statistics Institute was founded in 1885 and in the 1890s controversies raged over the very possibility of using samples to get knowledge about a population (Gigerenzer et al. 1989: 116: we draw the following paragraph from this source). Most thought 'partial investigations' inescapably imprecise and unscientific. In 1898 Anders Kiaer, director of the Central Bureau of Statistics of Norway pressed for the use of what he called 'the representative method', not based on random sampling, which was only understood in the early twentieth century, but on a sample that agreed in important characteristics with the population at large as described by a census: he was unable to convince the International Statistics Institute (Gigerenzer et al. 1989: 250).

Indeed, while Bowley developed the theory of sampling in economics and the social field, it seems that the techniques developed in a practical context by the market research of the 1920s were also influential on Gallup and his public opinion pollers (c.f. Roper 1940). None the less it is worth looking at such events as Bowley's work on the representative sample, because it is through a genealogy which lashes in work of this sort to all sorts of other practical and pragmatic knowledges and practices, that the sample became the subject – to refer again to one of Collins' features of high discovery research sciences – of a certain *consensus* amongst the variegated community of researchers involved. After a while (probably not until as recently as the 1940s though) it began to attain something like the status of 'science-already-made' – at least in the textbooks and training courses for the mathematically unsophisticated social scientists and psychologists.

The technical innovation of the representative sample thus made it possible to hone opinion into something like a new 'social fact'. 'Public opinion' attained an autonomous status that separated it quite rigorously from neighbouring, related concepts. Sociologists and psychologists had for a

long time, for instance, debated the difference between opinions and attitudes (e.g. Wiebe 1953). Allport provided a well known definition. An opinion is 'action or readiness for action with regard to a given issue on the part of members of a public who are reacting in the expectation that others in the public are similarly oriented toward the same issue' (Allport 1937). On this kind of view, an attitude – although the matter was up for some quite vigorous debate – might be something that someone holds almost as a reflex, a personal view (cf. Thurstone 1928). An opinion, on the other hand, is something public by definition, collective by design. Opinion was public in at least two senses. On the one hand, someone can only hold an opinion, by definition, *in* public – in that opinions are things that people, so to speak, project into the public arena, in the context of a whole 'field' of public opinion to which they are relating their own views, whatever their personal 'attitudes' might be. On the other hand, opinions always themselves relate *to* specifically public matters – one has an opinion on only public things such as elections, governments, the desirability of consumer goods, the quality of radio programmes and so forth. One cannot, then, just hold an opinion on *anything* (Riesman and Glazer 1948). Rather one can hold opinions only on 'matters of opinion'; a new sphere of reality was born, a new kind of phenomenon effectively produced.

A CONSENSUS ON PUBLIC OPINION

By the end of the Second World War, then, it appears that there was something of a consensus on public opinion: that it exists, that it can be measured, that it is related to action, that it is important for all kinds of political and non-political decision making, that it can be ascertained by certain research technologies and so forth. But what are the implications of this consensus for the *status* of opinion. As we have said, we are not interested in the philosophical question of demarcation. Our concern is with the more mundane issue of how truths, and the phenomena about which they are truthful, are, in fact, fabricated in this world. In this sense we need to be 'emergentist' – perhaps even 'instrumentalist' – about the phenomenon of opinion (cf. Hacking 1980). To see what we mean here, compare the current conception briefly to the mediaeval notion of opinion. Currently, as we have seen, public opinion is what a research technology produces out of the collectivity of individual opinions, yet which stands over and above such a collectivity: this forms the object of the contemporary concept of 'public opinion'. For Thomist epistemology, opinion meant something quite different; it was a kind of non-universal form of knowledge. Opinion here, historians tell us, did not refer to subjective attitudes, propensities and preferences but was seen as a form or procedure of knowledge in its own right; a form of knowledge properly related not to abstract kinds of knowledge but specifically to concrete knowledge of bits and pieces of the world. Hence opinion can be either true or false (Hacking 1976: 22).

The twentieth-century science of opinion, on the other hand, related less to a procedure of knowledge than to a kind of conceptual object, a social fact, called opinion. Opinion here hardly referred to anything beyond itself; it became, so to speak, something that was thing-like in itself, something that existed in its own right and, with the right technical resources and procedural methods, could be known and measured. In other words, opinion was something that simply emerged as a fact in its own right from the collectivity of people's individual opinions. And as an indicator of this emergent existence, note in this context the comparative rarity – because rather anomalous – of attempts to measure opinions by some more objective standard of truth that would be an 'improvement' on opinion itself. Cantril's attempt at a case-study on 'the measurement of civilian morale' which tried, rather ambitiously to compare public opinion about wartime successes with actual gains and losses in battle is rather unusual (Cantril 1944: Ch.17).

All this means that – unless we are to take an 'occultist' view of the concept of opinion, holding it to be a mysterious force with ineffable laws of its own – we also have to be instrumentalist about opinion. That is, we have to acknowledge that the notion of opinion is the product of the particular procedures by which opinion is elicited. This is so in a number of ways. It is so in an obvious way. For clearly *without* surveys and forms of measurement we would not know of public opinion at all; we would have no knowledge of what there is to measure without procedures of measurement. But this kind of instrumentalism is trivial. More interestingly, we can observe that public opinion is something that is demanded by the very activity of asking questions in surveys. That is, the existence of questionnaires and surveys *themselves* promote the idea that there is a public opinion 'out there' to be had and measured. They invite respondents to measure their own responses in relation to the existence of such an objective field of opinion. And this idea is actually of a rather self-fulfilling sort; we are asked a question, we respond, partly in the light of what we project to be the responses of others, and, in doing so, we actually contribute to the establishment of the objective field called public opinion. This feature of opinion research raises the issue of what can only a little recklessly be described as the proper *discipline* necessary for public opinion to exist. For when asked questions by pollsters and others, people have to know what to do; they need a sort of political education in the expression of opinions; people need to know how to create that phenomenon called opinion.

Let us leave to one side the question as to whether public opinion research is really ethical or appropriate for a democracy. It is clearly not enough to say that public opinion research is a charade, that public opinion does not exist. On the contrary, public opinion does exist in so far as there are technologies – and respondents attuned to the technologies – to ensure that it does so; and as such, it is, so to speak – like the 'ground' that determines the very possibility of all properly scientific questions – actually *beyond* true and false. There is, that is to say, what Collins

might term 'a trail of cognitive consensus' around the matter of public opinion.

For some sociologists, though, this trail of cognitive consensus is precisely the problem, or rather, cognitive consensus is produced here, as in other areas in the social sciences, by procedures that are obstacles to further rapid discoveries, in particular to those that might refashion and reconstitute the consensus itself. Of course, there is a long and honourable tradition that distinguishes between the kinds of formal, abstract, general and universal laws articulated in genuine sciences and the empirical generalizations about habitual associations in observed phenomena, that pass for laws in the social sciences (e.g. Willer and Willer 1973). For the Willers, the statistical techniques that we have suggested are *constitutive* of the phenomenon of public opinion are actually *obstacles* to the social sciences producing genuine discoveries, rapid or not. Statistical measures of association rely on the selection of particular correlations amongst observables and the neglect of others, and this selection usually occurs either on the basis of existing beliefs as to what is true, or on the basis of common sense beliefs and implicit notions about human behaviour. Whilst true scientific discoveries break away from common sense, in the social sciences, the role of statistical measures of association is to support, not to challenge such existing theories, common sense beliefs and implicit notions. Stephen Turner has made a similar point (Turner 1987). Innumerable different theories are capable of accounting for a given set of observed facts, and most theories can maintain themselves in the face of apparently contrary evidence. In the social sciences, statistical measures of correlation do not allow one theory to triumph over another, because new data can easily be absorbed into existing statistical models without decisively defeating the concepts that hold them together. Hence, it appears, the apparent regularities revealed by the statisticalization of the social sciences are actually a bogus substitute for scientific laws and an obstacle to discovery.

Such arguments are useful, for they help us clarify our own position. There are two aspects here. First, as we have said repeatedly, we are not searching for an epistemological demarcation criterion. Epistemological demarcation criteria regard science as a matter of theories, of representations, whilst for us, it is the moment of experimentation, of intervention, of creation of phenomena that is important. Second, though, we doubt that one can link epistemology and the creation of phenomena in the way that might be implied by the Willers or Turner. Is there something about the method of phenomenon creation and consensus generation in the social sciences that differs fundamentally from that in the natural sciences and prevents them becoming truly creative? Here we find ourselves convinced by the ethnographers of science that the actual existence of most (if not all) of what we call science does not look so different from the activities which we have briefly described in our case study. In fact, perhaps we should make our difference from Collins clearer at this point. For Collins implies that the dynamic of scientific change is generated internally to the natural

sciences once they have reached a certain threshold. Yet in our case study we have not distinguished between so-called 'internal' factors – Bowley's concepts of sampling, for example – and so-called 'external' factors – such as the demands for a measure of opinion for political commercial and military purposes. And, in fact, we would argue in the same way if we were studying the development of measures of accuracy in nuclear missile guidance – to take the topic of Donald Mackenzie's excellent study which does just that (Mackenzie 1990). Or, indeed, if we were concerned with the emergence of a 'molecular vision of life' which defined the locus of life at the submicroscopic region between 10^{-6} and 10^{-7} cm. as Lily Kay has put it in her recent study of the history of molecular biology (Kay 1993). That is to say, we do not think that the consensus constructed through 'systematic empiricism' is an obstacle to rapid change in the social sciences so long as they contain an interventionary moment. As long as this is present, it is clear that they – like the other endeavours that we have come to term science – will be mobilized and historicized by the dynamics of the networks and assemblages into which they are inserted. Indeed, perhaps it is not statisticalized systematic empiricism that prevents much contemporary social science creating phenomena, but precisely the opposite: the illusion that the essence of scientificity lies in the formal properties of their representational systems, their theories.

APPLICABILITY

By this point, a number of other objections to our argument will doubtless have suggested themselves. If it is granted that public opinion is a phenomenon created by the social science disciplines, then surely we still need to insist upon certain fundamental differences between the creation of phenomena in the natural and the social sciences. However, we think that these are better understood as matters of degree rather than in terms of a rigid line of demarcation. We have already seen one such difference in the context of our particular example, in the fact that the research front in public opinion research was a far more diffuse affair than the kind of thing envisaged by Collins with reference to the laboratory sciences. Instead of a closely demarcated corpus of researchers working upon a coherent cluster of problems, we had something more like a fairly lengthy cascade of investigations and innovations, built up incrementally and piecemeal, undertaken by a wide variety of persons and with regard to a good range of practical and theoretical problems. Rather than say that such a diffusion disqualified the example of public opinion from being defined as a research front, we would rather say that there is a lesson here for the historiography of the social sciences. The lesson is that it may be a mistake to construct such histories in institutional or professional terms, that is, tracking the endeavours of people who are avowedly social scientists. The scientists of society have to find their laboratories where they can; and that may be an election

campaign, a car plant or a jelly factory but not necessarily a research laboratory in a university department. It is a fact about much of the work of the social sciences that, unlike much work in the natural sciences, their activities take the form of a fairly well-diffused kind of spatiality; let us say that they often have a wide 'spatial mix'. In other words, whereas an experimental problem in the natural sciences is likely to be addressed most effectively in a concentrated space, with the manipulation of as few a number of variables as possible, the social sciences have to work, by very virtue of their concerns, in something like an open field of operations. In procedural terms that may make their tasks rather difficult, but for the most part it is a condition of their efficacy, and of their relevance to their material and to their constituency of human beings.

But if we grant that this difference might be considered as one of degree, are we not on less stable ground when it comes to consider the one feature of Collins' account of high discovery natural sciences that we have so far quite specifically neglected? This is the question of *applicability*. Collins notes that the social sciences are, in a sense, cut off from their subject-matter.

> When the social sciences offer practical application, it is in the form of information or advice: we give policymakers descriptions of a social problem, and sometimes . . . projection of what should happen if a given policy is followed. Beyond this, applied social science consists to a large extent of exhortation. . . (Collins 1994: 170)

Actually we find this formulation rather misleading. In fact, one of the key findings from histories of the social sciences is that the vectors of discovery do not lead from the laboratory to the outside world in the form of application: if anything, they flow the other way round. That is to say, that inventions and discoveries emerge first in relation to specific practical problems – be they those of intelligence in the schools or shell shock in the army, in the case of the psychological sciences, or those of the morale of the population in total war or the elimination of education disadvantage in the sociological sciences. One then observes a rather complex work of alliance building and mutual accommodation between academic researchers and professionals and practitioners, in which the practitioners draw upon the intellectual credit accorded by the academics to boost their credentials, and the academics draw upon the pragmatic credit of their appeal to a professional audience to increase their likelihood of research funding and the like. Whilst we would not, ourselves, wholeheartedly adopt the approach to such processes proposed by enthusiasts for 'actor network theory', it is clear that the metaphor of application, implying knowledge developed in academia and merely deployed in the 'real world' is rather too banal and superficial to characterize this dynamic.

It would be misleading to say that public opinion research was 'applicable', then, as this would imply a wholly erroneous relation of externality

between knowledge and application. But nor would it be helpful to say that public opinion research failed the test of applicability. A problematic of research such as that of public opinion does not fit the simple model of applicability, but it certainly fits the more complex picture of negotiations and interchanges between scientists and practitioners, or more generally between knowledge and government. In fact, it would perhaps be better to say that a phenomenon such as public opinion has a kind of performative as opposed – or in addition – to a denotative applicability. It is not 'applicable' merely in the sense that it solves a problem which can subsequently be 'applied' in the real world; rather merely to *act* in the domain of public opinion research can mean to apply some of the desired effects of that research. That is especially so with regard to opinion research carried out on behalf of governments.

Obviously, such research will be all the more in demand during times of governmental activism; when, that is, government most demands kinds of feedback from the populace as to the merits and efficacy of its activities and when, perhaps more importantly, the government wishes to show the populace that it is doing something. To take a relatively trivial example; during the New Deal, in 1933, the Psychological Corporation asked a sample of citizens the question, 'From what you have seen of the National Recovery Act in your neighborhood, do you believe it is working well?' (Abrams 1951: 66). Here, the research itself, in its very performance, was achieving a kind of 'application'; drawing attention not least to the fact that the government was at least trying to do *something*, whatever the content of the answer to the question itself. The answer to the question might denote something about public opinion; but the question itself performed something like a speech act, not least in relation to the very constitution of that opinion.

Of course, some might think that the close links between the creative and the 'applicable' aspects of the social sciences and the rationalities and techniques of government itself disqualifies their claims to scientificity. This is an understandable reading of some of Foucault's analyses, say of the relation between the disciplinary rationale of the prisons and the birth of the sciences of the individual (Foucault 1977). But if 'corruption' by governmental concerns was to disqualify knowledge practices from scientificity, the cupboard of science would be pretty bare. Taking only the two studies to which we have already referred, that by MacKenzie would force one to discard much contemporary science of control guidance and navigation, and that by Kay would force us to jettison most modern molecular biology (MacKenzie 1990, Kay 1993). What both these studies attempt, and what interests us here, is put rather well by Foucault himself, in the preface to his study of the birth of clinical medicine (Foucault 1973). For our own concerns, we can paraphrase it thus: it is research which is both historical and critical, which is concerned – without prescriptive intent – to determine the conditions of possibility of 'social' experience in modern times. It is only enterprises that have a bad conscience who think that the writing of such a

critical history is the compilation of evidence for some future determination of guilt or innocence.

AN OPINIONATED SOCIETY

Of course, we acknowledge that this case-study is particularly amenable to our argument that the social sciences can indeed create phenomena. This example seems all the more apposite in that it is taken from the 'subjective' end of social research – concerned with subjective opinions, beliefs, attitudes and so on – rather than from the more objective end of things. This appropriateness may actually stem from the fact that the social sciences have a fundamentally different relation to 'science-already-made' than do the natural sciences. Natural sciences, as Collins argues, exhibit a kind of one-way relation with the lay community. But this is not so – or not necessarily so – with the social sciences. In our example, people *learn* to have opinions; they become 'opinioned' – or perhaps, even, 'opinionated' – persons. In that sense, opinion polls 'make up' people; people come to 'fit' the demands of the research; they become, so to speak, persons that are by nature 'researchable' from that perspective. It seems to be the case, then, that with at least the successful social sciences, genealogies of research technology can be paralleled with a genealogy of persons: the phenomena created by the knowledge practice are, so to speak, actually internalized within persons. Our argument here is not particularly original. For example, Ian Hacking has recently proposed something similar in his analysis of 'multiple personality' and 'recovered memory' (Hacking 1995). And this mutuality in the history of knowledge and the history of its object has been a key feature of the approach to the history of psychology developed by Kenneth Gergen (e.g. Gergen and Davis 1985). Paul Atkinson and David Silverman have suggested something similar, pointing to the congruence between the sociological valorization of the 'confessional' interview as a means of access to 'authentic knowledge' in the form of narratives of experience, and the rise of what they call an 'interview society . . . relying pervasively on face-to-face interviews to reveal the personal, the private self of the subject' (Atkinson and Silverman 1997). For us, as for these authors, one key aspect of the creativity of phenomena in the social sciences thus pertains to the subjective attributes of persons themselves: the kinds of persons they take themselves to be and the forms of life which they inhabit and construct. Now, we think that there are at least two ways in which this relates to our question of creativity in the social sciences.

In the first place, this social and human creativity clearly occurs at a different tempo to that of the natural sciences. In the natural sciences, as we know, creativity can be rapid in its timing and more or less immediately revolutionary in its effects: think, for example, of the creation of nuclear fission. We do not think that the social sciences are generally like this: it takes longer to create new kinds of humans and new forms of life. Social

sciences have to work with what might be described as a lower 'tempo of creativity' than do the natural sciences. Perhaps this provides a clue to that age-old question as to the relative efficacy of the natural and social sciences. For the social sciences may be more successful – or at least not so unsuccessful – than traditional themes in the sociology and philosophy of science might lead us to think. But their success may only become visible when viewed within a different temporality. With some of the more successful branches of the natural sciences, there is an exponential relation between the concentrated 'spatial mix' of their concerns and the correspondingly rapid 'tempo of creativity' of their findings and discoveries. This is precisely because the more concentrated their activities, the greater the incidence of new creations. Hence the common image of quite stupendously successful natural sciences. The social sciences are not typically faced with this kind of opportunity, simply because of the kind of subject-matter they operate with. This is, of course, to smuggle in a quite necessary element of epistemological realism into our argument, not so much about the kinds of creatures that humans are, but rather, about what they are capable of being and doing, and how their capacity and possibility is activated. Why do we think that there are different temporalities involved for the social sciences to be effective in the sense of activating and reshaping individual and collective human capacity? Let us remind ourselves, we are talking not merely of certain theories or representations but the practices and interventions into which they are assembled. For the social sciences to be creative, they have to be quite well diffused in logistical terms by means of such assemblages. And it is of the nature of their human subject-matter that the transformation of its capacity can take place only at quite a limited pace. The links between spatial mix and creative tempo in the social sciences, together with their characteristic mode of action, means that they only rarely generate an exponential tempo of creativity of phenomena.

Second, and this is our final point, the kind of creativity that characterizes the social sciences is, inherently, rather difficult for its subjects to perceive. Hence the social sciences have often received rather a raw deal from certain kinds of meliorist historiography. For a central aspect of their creativity lies in their impact on humans themselves – in the terms of our case-study, the creation of 'opinioned' persons. It is not surprising, then, that their successes are apt to remain largely invisible. For there can be no clearly-defined 'outside observers' of such changes. If humans themselves are changing, there is no obvious stable point to visualize a 'before' and 'after' scenario, no static dimension with a fixed scale allowing the measurement of relative success or failure. We are all 'opinioned' now. From that 'opinionated' perspective, who could now have the counterfactual sensibility or even the imagination to have expected otherwise? Such a thought should impress upon us the importance of a certain kind of historical work in the social sciences. For research of this type is capable of revealing to us the surprising, even occasionally extraordinary, nature of the kinds of beings that we have become, and the forms of life we have come to lead

– the kinds of human beings, and forms of life, that the social sciences have themselves helped create.

(Date accepted: October 1998)

Thomas Osborne
Department of Sociology
University of Bristol
and
Nikolas Rose
Department of Sociology
Goldsmiths College

NOTE

1 This article is based on work conducted as part of on-going research on the history of empirical social thought (see Osborne and Rose 1997 for an overview), made possible by grant R000221847 from the Economic and Social Research Council, UK. We are particularly grateful to Daniel Robinson for allowing us to draw upon the discussion of George Gallup's work in his unpublished Ph.D. thesis (now published as Robinson 1999). Our thanks also to two anonymous readers of an earlier version of the paper for their perceptive criticisms and helpful suggestions.

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