4. Social representations: their role in the design and execution of laboratory experiments

ROBERT M. FARR

Individual and collective psychologies

When we trace the origins of psychology as an experimental and social science we note that Wundt chose to separate his experimental science from his social psychology. This he did deliberately and consciously, though historians of psychology as an experimental science have often failed fully to appreciate his reasons for this decision. Wundt had a very clear perception of the limitations of the laboratory science which he had established at Leipzig in 1879. Those who came after him, and who readily acknowledged him as the 'founding father' of experimental psychology, did not share his reservations concerning the limitations of his experimental science. This is an aspect of what Danziger has called 'the positivist repudiation of Wundt' (Danziger, 1979). Wundt's own answer to the limitations of his laboratory science is to be found in the ten volumes of his *Völkerpsychologie* (1900–20). These volumes have been virtually ignored by the official historians of psychology as a science.

Wundt's experimental science was a psychology of the individual whilst his social psychology (or *Völkerpsychologie*) was a collective psychology. The objects of study in his *Völkerpsychologie* were language, religion, myth, custom, magic and cognate phenomena. Had he been writing in the closing, rather than in the opening, decades of the twentieth century he would almost certainly have included science as an appropriate object of study in his collective psychology. He thus took cultural products as the objects of study in his collective psychology. He was prepared to make inferences about the nature of the mind of 'primitive man' on the basis of an analysis of the structure of the languages he spoke. Wundt thought it was necessary to supplement his laboratory science with the study of mind in society outside of the laboratory. The objects of study in his *Völkerpsychologie* were, in their

origin, the products of the collective experience of 'folk communities'. Language, religion, myth etc. could not have been invented, Wundt argued, by individuals, neither could these collective phenomena be reduced to, nor explained in terms of, the consciousness of individuals which Wundt had taken as the basis for his new laboratory science.

Not only could collective mental phenomena not be 'reduced to' phenomena at the level of the individual mind, but the techniques of investigation which were appropriate at this latter level were *inappropriate* for investigating phenomena at the collective level. The limitations which were inherent in Wundt's laboratory science derived from his reliance on introspection as his preferred method for investigating mental phenomena. He readily acknowledged that introspection was not an appropriate methodology for investigating man's higher cognitive processes. He argued this on the grounds that the mind of the individual could not by itself become conscious of forces of which it was the product, that is the processes of historical change and development. The study of the human mind as the product of evolutionary and historical change was the subject-matter of his *Völkerpsychologie*. This was his reason for treating this as a *separate*, though related, discipline to his laboratory science. Thus, *at the outset*, Wundt drew a clear distinction between psychology considered as an experimental laboratory science and psychology considered as a social science which treats seriously the evolution of mind in man. We can thus credit Wundt for this initial appreciation that *both* a collective *and* an individual psychology were necessary. His decision to separate the two, however, had certain unforeseen historical consequences which we must now trace.

The most important consequence, for the purposes of this chapter, can be seen in Wundt's influence on Durkheim. The many different ways in which Wundt exercised an influence on Durkheim are common knowledge amongst Durkheim scholars (see e.g. Lukes, 1973*a*; Giddens, 1978; Mauss, 1950, 1979). This is something of which most psychologists are ignorant as they only rarely read the literature of social sciences other than psychology. Durkheim had visited various German universities, including Leipzig, during the year 1885–6. He was impressed by what he observed. He was interested in the precision and rigour of Wundt's experimental research; and in his strategy of gathering around himself a team of fellow-workers who shared in the task of developing a new field of study and who initiated a journal in which they could publish the results of their research. He was subsequently himself to adopt a rather

similar strategy when he established sociology as an academic discipline in France from his base in Bordeaux. Amongst psychologists Durkheim enjoys a reputation for having been the most antipathetic to psychology of all the major sociological theorists. This reputation derives, in large measure, from his insistence that 'social facts' are not to be explained in terms of psychological facts, for example in his classic study of suicide. This insistence, however, is little more than an echo and an amplification of Wundt's earlier insistence that his *Völkerpsychologie* was a separate enterprise to his laboratory science and involved a difference in levels.

The psychology to which Durkheim was so vehemently opposed was the psychology of the individual. He made a distinction between what he called 'collective' and 'individual' representations (Durkheim, 1898). He was quite happy to leave the study of 'individual' representations to the psychologist. They corresponded to the type of 'personal constructs' which George Kelly was later to study (see the chapter in this volume by Fransella). The study of 'collective representations', however, fell clearly within the province of sociology. Indeed Durkheim envisaged that some day there might be a sub-specialism within sociology (i.e. social psychology) exclusively dedicated to the study of 'collective representations'. The answering echo to Durkheim's plea was not to come until after World War II (see below). These collective representations were very similar to the objects of study in Wundt's *Völkerpsychologie*, that is language, religion, myth, magic, and cognate phenomena. What for Wundt had been a distinction between two forms of psychology – individual and collective – became, with Durkheim, a difference which separated two academic disciplines – psychology (focusing almost exclusively on the individual) and sociology (focusing almost exclusively on 'society'). This difference in 'levels' was the significant issue at stake in the famous Durkheim/Tarde debate in 1903–4 at the Ecole des Hautes Etudes Sociales in Paris (for an illuminating account of the debate and its background, see Lukes (1973*a*, chap. 16, pp. 302–13)). Each gave a lecture with the title 'Sociology and the social sciences' and this was followed by a third meeting in which they publicly debated their differences 'with much heat'.

The creative tensions which might have arisen from operating simultaneously at two different levels (i.e. the 'individual' and the 'collective') never really bore fruit. Even Wundt addressed himself to these separate, though related, issues during *different* decades in his long and highly productive career. McDougall wrote two text-books in social psychology – one at the level of the individual (1908) and the other at a

'collective' level (1920). The first was intended only as an 'introduction' to social psychology and the two together were originally intended as an outline of 'social psychology'. A major world war, however, intervened between the publication of the two volumes and each, separately, aroused significant opposition. The first volume was controversial because McDougall applied the term 'instinct' to account for human behaviour, whilst his second volume – *Group mind* – was controversial because he appeared to assign 'agency' to entities other than individuals. Both attacks forced him to revise his original formulation of the issues involved. F. H. Allport, the behaviourist, who attacked McDougall for assigning agency to such supra-individual entities as 'groups' also contributed both to 'individual' and to 'collective' themes within social psychology, for example his writings on institutional behaviour and public opinion, as well as his better-known text-book of social psychology (Allport, F. H., 1924, 1937). F. H. Allport differs from the other writers discussed above in that he did not believe that a *difference* in levels was involved when one moved from the individual to the collective. For him the ultimate reality was behaviour and only individuals behave. It was Allport's views, rather than McDougall's, which came to prevail, at least in the development of experimental social psychology in America. This accounts, at least in part, for the non-social ethos of much contemporary American and British social psychology. This helps to highlight the distinctly different perspective of contemporary French research on 'social representations' as set out in this volume. Deriving, as it claims to do, from Durkheim, it is a much more inherently 'social' social psychology than much contemporary research in American and British laboratories. It thus constitutes an important contemporary critique of the 'individual' bias of much social psychology. Herzlich (1972), in her review of French work on social representations, contrasts it with the dominant Anglo-American tradition: '. . . the concept of "social representation" relates to a different tradition: European and essentially sociological' (p. 303).

Instead of the tensions between different levels of analysis being creatively resolved within the one discipline (i.e. psychology) they ceased to be creative tensions once they resulted in a division of labour *between* academic disciplines. Durkheim (1898) cited, with approval, the arguments used by William James for psychology being a field of study quite independent of physiology. He made a similar plea that 'collective representations' ought to be studied quite independently of 'individual representations'. This led to a sharp separation between sociology and

psychology. This separation posed problems for social psychologists who could develop their discipline on either side of this rather oversharp divide. This is how it came about that there are now sociological, as well as psychological, traditions of social psychology. Elsewhere I have addressed myself to the issue of how one might set about the problem of reconciling the various traditions of social psychology which vie with one another in seeking to gain the attention of the modern reader (Farr, 1978*b*).

Social representations

The origins of the contemporary tradition in France in the late 1950s

The recent renaissance of interest amongst French social psychologists in Durkheim's concept of 'collective representations' commenced with the publication, in 1961, of Moscovici's study: *La Psychanalyse: son image et son public* (Moscovici, 1961). Whilst this book is now in a second edition in French it has yet to be translated into English. In choosing it as an illustration I am influenced by the fact that it is not yet available in English. In this pioneering study Moscovici refers to 'social representations' as this 'neglected' or 'forgotten' concept of Durkheim. This whole tradition of research in French social psychology represents a renewal of interest in the study of modes of knowledge and of the role of symbolic processes in relation to human action. Researchers within the contemporary tradition, consistent with the Durkheimian origins of the concept, stress the primacy of social, over individual, factors in the determination of human conduct.

Psychoanalysis was a convenient example, for Moscovici, of a 'new science' which was relevant to an understanding of human behaviour. Psychoanalysis, through its very circulation in society, becomes transformed into a social representation. 'A science of reality thus becomes a science *in* reality ... at this stage its evolution becomes the affair of social psychology' (*ibid.* p. 19, my translation). It is thus when a scientific theory is *published* that it may fall within the legitimate area of interest of the social psychologist. Lagache, in his preface to the original edition, felt it was necessary to explain to his readers why a social psychologist, who is not a psychoanalyst, should be writing a book on psychoanalysis. A theory, if it is to be effective, needs to be expressed in words (i.e. to be made *public*) and needs to be transmitted in written and/or oral traditions. The creative act comprises the original formulation of

the theory in some form of symbolic communication. The link here with studies in the sociology of knowledge is an obvious one.

In his study Moscovici traced how a scientific language became a common 'dialect' – how it pervaded judgement and directed human actions. Using the conventional tools of opinion polls and surveys Moscovici traced how a knowledge of psychoanalysis had diffused within various sectors of the French population in the late 1950s. He showed how the social representation of something as complex as a new scientific theory is not fundamentally an impoverishment, but is rather a transformation, of this theory. The appearance of a new language, he argued, is both a consequence of psychoanalysis being absorbed by a particular society and the means by which this process comes about. The existence of a dictionary of psychoanalytic terms (Laplanche and Pontalis, 1967) is further evidence, if such were needed, of just how widely psychoanalysis has diffused within French culture.

Moscovici used empirical methods to study the diffusion of a particular science within a particular culture at a particular point in historical time. It would thus serve as an excellent example of Gergen's thesis that social psychology is an essentially historical discipline (Gergen, 1973). This historical dimension is important in the work of the contemporary French school – especially in their field studies. Herzlich (1973) notes, by graphic references to the history of medicine, how the social representations which people form of health and illness have differed in different historical epochs *within* the one culture. She also notes, by referring to anthropological studies, how the social representations of health and illness, at any one particular point in time, may differ quite markedly as a function of culture. Similarly Jodelet and Moscovici (1976) are interested in studying *changes*, over historical time, in people's representations of their own bodies. Some of this work is cited in Jodelet's contribution to this volume.

Moscovici claims that any new theory or the application of any previously unknown technique might, potentially, have a similar impact in changing the culture within which it is conceived. The atomic bomb, he notes, through the political choices which it entails and the fears which it nourishes, has been a formidable school of physics for the majority of mankind. The world can never be the same place again once such a nuclear device has exploded amongst a population of humans. Theories can be just as explosive in their impact as nuclear devices, though the effects of their fall-out may not be so immediate. New theories could arise, for instance, from scientific voyages of discovery

One could choose, for example, Darwin's voyage on the survey ship, HMS *Beagle*, or the space missions completed by American astronauts of the Apollo programme. The social, cultural and scientific repercussions of Darwin's theory of evolution have been profound. At a totally different level of initial impact we could cite photographs of the earth taken from outer space. These provide the average citizen of today with much more graphic and convincing evidence that the earth is round than the traditional arguments he might have learned at school against believing it to be flat. Pioneers are often themselves aware of the social repercussions of their own discoveries. This is evident in Darwin's reluctance to publish his theory until he had accumulated a great deal of evidence for it. It is also reflected in the remark that Freud is reputed to have made to Jung as they disembarked in New York at the turn of the century: 'We are importing the plague.' Moscovici quotes this incident, with evident approval, in the preliminary remarks to his study. They epitomise the point of Moscovici's own study, that is that scientific ideas and theories can change the nature of the world in which people live.

La Psychanalyse: son image et son public is a detailed case study carried out within one particular culture at a particular point in time. It is, therefore, only *illustrative* of Moscovici's more general thesis. His argument is thus much more general than the particular case study he initially chose as an illustration. He later goes on to make essentially the same general point in his theory of minority influence. Here he used experimental methods and the majority of studies were carried out in the laboratory rather than in field settings. The point of his theory of minority influence is that all truly creative geniuses are, almost by definition, in a minority of one until they persuade the majority to think in the way that they do or to see things as they see them. He is, thus, more interested in studying innovation and change than he is in studying conformity and the maintenance of the *status quo*. The time scale needed, however, for the successful adoption of an innovation within a society, is much greater than can be captured by any laboratory simulation of the process.

In the first part of his study of psychoanalysis Moscovici sampled the views and opinions of various sectors of the French population concerning psychoanalysis and their knowledge of this theory. Here he used the well-tried techniques of social enquiry – structured, and occasionally unstructured, questionnaires. The various samples comprised a total of just over 2,000 persons. In the second half of his study he carried out a careful content analysis of all articles relating to

psychoanalysis which appeared in some 241 different journals, reviews, and newspapers during a fifteen-month period in the early 1950s. Moscovici thus not only sampled the diffusion of a knowledge of psychoanalysis – he also intercepted and analysed the information and propaganda circulating in the mass media relating to the object of his study.

The starting-point of Moscovici's study was the publication of a new scientific theory. Science is a topic as worthy of study as the religion, myth, and magic which were of interest both to Wundt and to Durkheim. As in his laboratory studies on minority influence, the interest lies in identifying how original ideas came, over time, to be accepted. As a theory psychoanalysis challenged traditional conceptions of man. He traces, in the second half of his study, how the Catholic press responds to a secular form of the confessional and how the Communist press handles a popular science which is non-Marxist etc. For a devout Catholic the *meaning* of psychoanalysis will be sought in the writings of churchmen and apologists on the topic. It may even be crystallized for him in the form of a papal encyclical. At the time of Moscovici's original study (i.e. the early to middle 1950s) the attitude of the Catholic Church in France to psychoanalysis had initially been one of suspicion followed by a period of amelioration and assimilation. The secular became assimilated to the sacred and this made it 'safe' for believers to take an interest in the new science. The response of the Communist Party in France, however, to the new science was, at that same particular point in time, a totally different one. This was the immediate post-war era when the cold war between America and Russia was the dominant theme in the arena of international politics. The Communist press handled psychoanalysis by rejecting it. Moscovici treated this press coverage as a case study in the dynamics of propaganda.

Discussions of how to 'handle' issues are common talk amongst those who work in the communications industry. It is a question of what sort of an 'attitude' one adopts or what 'stance' one takes with respect to a given issue. The term 'attitude' is used here in an almost Darwinian sense, which one rarely encounters in modern psychology, that is to refer to the often whole-bodied 'posture' of an organism towards some significant 'object' in its environment (see Darwin, 1872). In the second half of Moscovici's study it is social institutions, rather than organisms, which adopt these 'attitudes' of acceptance or rejection. An 'attitude' is thus a stance or a behavioural posture and is best conceptualised as such. The whole of the work of the French School is a critique of the more

conventional research in social psychology which treats 'opinions', 'attitudes', 'personal constructs', 'images' etc. as more or less purely individual representations (for a critique of conventional research from this French perspective see Moscovici, 1963). In the field studies of the French School these 'attitudes' and 'stances' are socially negotiated and are usually studied by means of a content analysis of the messages diffused by the various media. Just as Mead was interested in analysing the 'conversation of gestures' which occurs when one animal orients itself with respect to another, so Moscovici is interested in analysing the dialogue and the war of words which pass between the institutions of a society and the mass of its citizens as they adapt to some momentous event or react to the emergence of some new science or idea.

The discrepancy between the field and the laboratory studies within the French tradition

In his foreword to the interesting study by Herzlich of people's representations of health and illness Moscovici describes

> 'social representations' in the following terms: . . . cognitive systems with a logic and language of their own . . . They do not represent simply 'opinions about', 'images of' or 'attitudes towards' but 'theories' or 'branches of knowledge' in their own right, for the discovery and organisation of reality . . . systems of values, ideas and practices with a two-fold function: first, to establish an order which will enable individuals to orientate themselves in their material and social world and to master it; secondly to enable communication to take place among members of a community by providing them with a code for social exchange and a code for naming and classifying unambiguously the various aspects of their world and their individual and group history. (Moscovici, 1973, p. xiii)

This definition of 'representation' is sufficiently majestic to encompass within its scope the study of any religion or myth or form of magic that might have appealed either to Wundt or to Durkheim. The idea of introducing science as a new category of representation into the pantheon established by the founding fathers is a modern and innovative idea. Moscovici's pioneering study of psychoanalysis was followed by others, some of which have already been alluded to above. There is Herzlich's study of the social representations of health and illness; the work of Jodelet and Moscovici on the social representation of the human body and changes, over time, in this representation. Jodelet is currently working on the social representation of mental illness, using the techniques of participant observation. Madame Chombart de Lauwe

has written extensively concerning the 'myth' of childhood (1971). She has shown how 'childhood' is a social representation created by adults with powerful and real consequences for the children of those adults. These are refreshingly original studies which would stand up well in the searchlight of international scrutiny.

I am less sure in my own judgement about the possible international standing of the many laboratory studies carried out within this tradition of French research on social representations. This does not really surprise me because I believe there are important conceptual problems involved in applying the idea of social representations to laboratory science. If I understand the notion correctly, then one's representations of what science is and of what laboratories are for are bound to be of much greater significance than any particular pieces of research that might be carried out *within* those laboratories. Moscovici's characterisation of the social nature of representations (quoted above) is more directly applicable at the level of a scientific theory or of a research paradigm than at the level of a single experiment within such a paradigm. Is his description of a social representation not virtually synonymous with the common scientific endeavour which helps to keep a given group of scientists communicating with each other? It must relate to social exchanges which occur at the level of the scientific community. How can it possibly be captured and confined to something which occurs *within* the course of a single experiment, or even of a series of experiments? Social representations were born and nurtured in the wider society outside of the laboratory. They relate more directly to Wundt's *Völkerpsychologie* than they do to his laboratory science. There are some re-entry problems which need to be faced if the notion of social representation is to be successfully applied to the design and conduct of laboratory experiments. This is the issue I now wish to tackle in the second half of this chapter.

The laboratory as a social representation

The world of the observer

In the laboratory the world of the observer (i.e. of the experimenter) and the world which he observes (i.e. subjects performing laboratory tasks) are usually two quite different worlds. The implications of this distinction are rarely made explicit in the standard manuals of research methods. The sharp distinction between these two worlds first came into

psychology with the acceptance of behaviourism. In Wundt's laboratory at Leipzig observer and observed were one and the same person. This early experimental psychology was thus inherently non-social in form. This was due to Wundt's reliance on introspection as his main technique of investigation within the laboratory.

When, with the advent of behaviourism, psychology became an 'objective' science, it was non-social both in conception and ethos though, in actual research practice, it was highly social. Observer and observed were now two different persons (or, perhaps more accurately, different organisms) and it was *behaviour*, rather than *experience*, which was the subject-matter of psychology. This opened up the possibility that observer and observed might inhabit two quite different worlds even though they encounter one another face to face in the context of the research laboratory. One of the early behaviourist textbooks, in which the author maintains a consistent methodological stance, was intriguingly entitled *The psychology of the other one* (Meyer, 1921).

I am indebted to Edgar Morin (1977) for first highlighting the importance of distinguishing between 'the world of the observer' and the world as it appears to that observer. He notes, particularly in the natural sciences, how the scientist, like the photographer, fails to include himself in that which he records. '... science has no scientific knowledge of itself and lacks the means of knowing itself scientifically. There is a scientific method for considering and controlling the objects of science. But there is no scientific method for considering science as the object of science still less (for considering) the scientist as the subject of this object' (*ibid.* p. 14; my translation). Whilst Morin is more broadly concerned with the effects of positivism and of science on man's conception of himself I am here concerned, more narrowly, with the effect of positivism on psychologists' conceptions of what a laboratory is. Positivism, within the history of psychology, took the form of behaviourism.

The world of the observer, in the psychological laboratory, is the world of other experimenters – those whom he has in mind when he *designs* his experiments and for whom he writes up an account of his investigations. There is indeed an agreed language, and even an agreed style, amongst those who comprise *the community of experimental psychologists* in terms of which members communicate with each other concerning the events which have occurred in their laboratories. Initially the future researcher is socialised into this community of other experimenters during his undergraduate and postgraduate studies. Surely it is at the level of the

'scientific community' of other experimenters (what Crane (1972) calls 'invisible colleges') that one ought to look for, and study, the operation of social representations? This brings us back to the whole conception underlying Wundt's development of a 'folk psychology'. Social phenomena, according to Wundt, are to be located at the level of 'folk communities', that is those who use a common language and, hence, share a common social reality. We have noted above links between Wundt's 'folk psychology' and Durkheim's notion of 'collective representations'. I think an error of translation has taken place, in the work of the contemporary French School, in the translation from field to laboratory. Instead of looking for the operation of social representations *at the level of the scientific community*, experimenters, instead, have been content to demonstrate how the representations which their subjects form of the experimental situation actually influence how they act *within* the milieu of the laboratory.

If one wishes to explore the role of social representations *in the world of the observer* then perhaps it is better to adopt the research techniques and general orientation of Lemaine and his collaborators (Lemaine *et al.*, 1977). In the context of accounting for the diversity of French research on sleep they investigated how rival teams of scientists in different laboratories came to adopt the research strategies which they did. This work generally is not considered to be within the tradition of research on social representations. However, it might be more profitable to follow up this line of investigation than to continue the current practice of conducting experiments *on* social representations using the traditional methodology of the laboratory experiment. The approach of Lemaine *et al.* is much more in keeping with the ideas and methods which inspired the field studies of social representations. They enable us to visualise and to articulate better the processes which are likely to be involved in studying how social representations operate in the world of the observer.

If social representations are a guide to conduct – and this is how Moscovici conceives of them – then how experimenters/observers behave in the context of the laboratory may be a function of how they represent to themselves and to others the nature of the scientific activity upon which they are engaged. If experimental psychologists believe, for example, that they are engaged in carrying out research in natural science (as opposed, say, to social science) then this must have a profound effect upon how they interact with their subjects and upon the design and layout of the laboratories in which they work. Thus

'representations' which the experimenter takes into account (or assumes) at the *design* and planning stage are likely to translate fairly directly into research practice in the actual *execution* of the experiment. Perhaps one ought to be looking here at the relationships between theory and practice in the psychological laboratory.

Elsewhere I have sought to demonstrate that *in actual research practice* there might be such a thing as a distinctly 'social psychological' *style of experimenting* (Farr, 1976). Differences in the theoretical orientation of the experimenter (e.g. a phenomenal/cognitive bias versus a behavioural bias) are consistently related to variations in actual experimental practice. Cognitive theorists, as distinct from behaviourial theorists, are much more likely to be concerned with how their subjects 'interpret' the experimental situation. In another article (Farr, 1978*a*) I sought to identify the social significance of the 'artifacts' which can arise in laboratory experiments. These artifacts arise because an experimenter 'represents' a social event (i.e. the experiment) as though it were not a social event. This may be because he subscribes to a model of the experiment which he derives from the natural sciences. This model affects not only his own planning and running of the experiment but also, afterwards, how he reports it to the scientific community. In describing 'the world *as* he observed it' the experimenter may fail adequately to take into account his own role as an observer and the effect he may have had on the world which he observed.

My evidence for the views expressed in the previous paragraph came from interpreting the 'accounts' which experimenters give of their research. Perhaps the best way to detect social representations is to use some sort of content analysis of written and/or oral communications. This is how they are usually detected in the field studies within the French tradition. By focusing on the 'accounts' which an experimenter provides for the benefit of *other* experimenters one is searching, *at least in the right place*, for evidence of social representations, that is in the world of the observer and at the level of the community. These 'accounts' can be 'interpreted' in much the same way as Harré and Secord (1972) suggested for the analysis of oral accounts. The main difference is that one is dealing, here, with the written text provided by an experimenter and intended for the eyes of other experimenters. Fortunately these texts are widely available in the learned journals. One is involved, here, in the explication of a written text rather than in the interpretation of an oral discourse.

The world as observed

This corresponds to the 'results' section in the formal report of most experimental investigations. Typically it comprises behavioural data relating to the performances on laboratory tasks of experimental subjects. The world of the experimental subject is not, however, the same world as that of the observer. Whilst the two meet face to face in the brief encounter of an actual experiment it would be wrong to assume that they *share* the same experience. It may seem strange, at first, to suggest that persons in face to face contact inhabit different worlds whilst the 'significant others' who determine the actions of the observer (i.e. the invisible community of other experimenters) are not themselves physically present in the laboratory during the course of the experiment. Experimenters and subjects cannot easily adopt *the same perspective* in regard to the significance of their social interaction. Most experiments inevitably involve an element of deception. It would be difficult, for example, for Milgram and one of his 'teacher' subjects to agree on the *significance* of the latter's button-pushing behaviour.

The *social* nature of the relationship between experimenter and subject is most salient at the point of recruitment and then again on the *threshold* of the laboratory, that is when the experimenter welcomes the subject on arrival and de-briefs him on departure. What happens in between arrival and departure is highly programmed and is rarely negotiable. This provides the experimenter with the 'data' for the 'results' section of his experimental report. The encounter is quite unlike anything which occurs spontaneously in the social world outside of the laboratory. In social interaction each interactant is normally free 'to assume the role of the other' with respect to himself (Mead, 1934). The interaction proceeds smoothly in so far as each of the participants is skilled at doing precisely this. In a typical experiment the most important 'other' (in the sense of Mead's 'significant other') for the subject is likely to be the experimenter in whose experiment he has just agreed to participate. His normal capacity to assume the role of this particular other may make him apprehensive on the grounds that he believes this 'other' will be evaluating him as a person on the basis of how he performs (Rosenberg, 1969). For a fuller exposition of the possible consequences of the subject's belief, see my paper on the social significance of artifacts in experimenting (Farr, 1978*a*). When, however, the experimenter 'assumes the role of the other' with respect to himself and to how he intends to act, that 'other' is usually another experimenter, that is some

member of the scientific community. Experimenters rarely adopt the perspective of a subject in one of their own experiments. For certain notable exceptions to this general statement (e.g. Lewin, Asch, Festinger, Aronson and Carlsmith, etc.) see Farr (1976; 1978*a*). It is entirely consistent with the theme being developed here that the difference in perspective which characterises these exceptions should result in a different 'style' of experimenting, that is that the experimenter's representation of the human subject is likely to affect how he treats the latter in the actual conduct of the experiment.

It is worth highlighting a little more fully, perhaps, the social/non-social nature of the relationships between experimenters and subjects. In his highly original essay on the nature of the doctor/patient relationship Goffman (1961) traced some vicissitudes in the social history of the tinkering professions. When tinkers used to call at their clients' domiciles they had to exercise great caution in how they handled the property which was entrusted to their care. They often had to tinker with the object under the watchful eye of its owner. When tinkers ceased to be itinerants in search of work, however, and set up shop instead, this changed the social nature of the relationship between tinker and client. Customers now brought articles, which were in need of repair, to the shop. A wall or partition often separated the 'front' from the 'back' of the shop. As in much of Goffman's work this distinction between 'front' and 'back' regions is full of implications for the *types* of social interaction which can occur in the two locations. The separation between the two halves of the shop enabled the tinker to establish, and to maintain, a social relationship with his client in the front of the shop. Here the tinker could take his client's property into custody whilst being able to assure and, if necessary, to re-assure, his customer of his own technical competence to repair it. The tinker also returns the repaired article to its rightful owner in the 'front' of the shop and monitors the latter's response to the results of his work. In between these two social events the tinker can become highly technical in how he handles and relates to the object without his actions coming under the surveillance of its owner. He carries out this technical work in the 'back' of the shop, separated both in time and in space from his continuing 'social' relationship with the owner of the object.

Goffman then identifies the doctor's dilemma. The doctor needs to establish a social relationship with his client. As the same time he may need (to be able) to become highly technical about the latter's body. The problem is more acute for the doctor than it is for the tinker as the patient

appears to be attached inextricably to his body. How the doctor sets about separating the patient from his body need not detain us here though it is an interesting topic in its own right. There may be some parallels in the relationship which experimenters seek to establish with their subjects. There is a certain phase within the context of the social relationship described earlier during which the experimenter assumes the highly technical role of being a scientific observer. We usually read about this phase in the 'results' section of most scientific reports. It constitutes 'the world as observed' by the observer. The experimenter, during this phase of the experiment, may adopt an attitude of 'civic inattention' to any item of a personal nature which the subject may wish to place on the agenda.

The laboratory

Just as the tinker lays out his shop to suit his needs so the laboratory is designed by the experimenter for his own purposes. Thus laboratories are likely to reflect, and to enshrine, their creator's conception of science. A laboratory is a place in a definite location (i.e. it has an address) as well as being a social institution. Subjects are invited to 'enter' it and are greeted on arrival. They are usually, but not always, volunteers. They enter, and leave, with their own impressions of what a psychological laboratory is. Laboratories have their own special 'atmosphere' or 'ethos' for those who work there as distinct from those who merely visit. A sociologist or anthropologist could easily, by means of participant observation, study the 'micro-culture' of such a laboratory. There is, however, a great dearth of such studies. The study of Latour and Woolgar (1979) of life in the Salk Institute: *Laboratory life: The social construction of scientific facts* is one such pioneering effort. It would be interesting to see similar studies carried out in psychological institutes.

One could explore the conceptions which experimenters have of the psychological laboratory. This has not been done so far as I know. It should be possible to predict just how such a social representation would vary as a function of the theoretical commitments of the person providing the account. One might expect, for example, clear differences between those who consider psychology to be a branch of natural science and those others who consider it a social science. Interesting new representations are beginning to emerge from those who consider psychology to be a 'cognitive science'. There were, of course, important historical differences in the design and layout of laboratories as between

those who believed psychology to be the science of mental life and those others who believed it to be the science of behaviour.

In the world's first psychological laboratory Wundt accepted verbal reports of immediate conscious experience as the legitimate data of science. His stress on the immediacy of the experiences being reported led him to rule out retrospection as a legitimate form of introspection. For Wundt introspection was a type of 'inner perception'. In his laboratory he obtained introspective data under highly controlled conditions. The scientific rigour of the Leipzig laboratory stood in sharp contrast to the earlier informal use of introspection which philosophers used to indulge in from the depths of their armchairs. The preoccupation with precision in measurement on the part of these early experimental pioneers is reflected in the brass instruments of this early German laboratory science.

The laboratory is a device for isolating phenomena from the social contexts in which they occur naturally in the 'real world' outside. Historical events occurring between t_1 and t_2 in the course of an experiment threaten its 'internal validity' (Campbell, 1957; Campbell and Stanley, 1966) and, if not controlled for, may be confounded with the effect of the independent variable. Good experimental control is thus virtually synonymous with the isolation of the events studied from their location in space/time within a particular culture. The isolation and control which Pavlov achieved in his laboratories was an ideal towards which others have aspired. Scientists usually believe the results of their laboratory research to be generally true until someone else proves otherwise, that is, at least conceptually the experiment is, in some ways, the very antithesis of a historical event. The laboratory is, in a sense, 'a world apart'. It is so both geographically and conceptually. Madame Chombart de Lauwe (1971), in her highly sensitive field study of the world of childhood, identified how different groups of adults (e.g. writers, film-makers, city planners, architects etc.) create the world of childhood in which their children have to live. She entitled her study *Un monde autre*. The world of the child has to be understood by its contrast to the world of the adult. Similarly the laboratory might best be understood by way of contrast to the world outside of the laboratory. The 'laboratory' is an *other world* which experimenters create for their subjects to enter. Perhaps it too can be explored with some of the same imaginative techniques used by Madame Chombart de Lauwe in her study?

The very success of scientists in isolating the laboratory from the world outside makes it less likely that the social representations which incubate within its walls will readily diffuse in the society beyond those walls.

Freud's consulting-room was not so hermetically sealed off from the wider culture outside of it. Freud, as a scientist, responded to the real world problems and events which his clients brought with them into his consulting-room. It is thus perhaps natural that the results of his thinking, when made public, should reverberate within that wider culture outside of his consulting-room. Thus Moscovici can readily study the social representation of psychoanalysis in French society. It would be more difficult to study the social representation (and hence the cultural repercussions) of behaviourism or of experimental psychology. Elsewhere I have discussed behaviourism as a social representation (Farr, 1981).

With behaviourism there came into laboratory life the crisp separation, noted above, between the world of the observer and the world which he observed. It resulted in a dramatic reduction in the status of the human subject in the research process (Schultz, 1969; Adair, 1973; Farr, 1978*a*). It was no longer possible to say that experimenters and 'subjects' shared the same laboratory culture as they had done in Wundt's laboratory at Leipzig or at the Institute of Psychology in Berlin during the early days of the Gestalt movement. I refer here to the micro-culture *of* the laboratory rather than to the wider culture which experimenters and subjects obviously share by virtue of living in the same society. Many of the experimental studies of the French School on the role of social representations *within* the laboratory (e.g. the researches of Codol, Abric, Flament, Plon, Apfelbaum and others) depend for their efficacy on this wider culture which experimenters share with the subjects of their research. This shared culture is most obviously present in the common language which they speak. In those studies where it is assumed that a subject's representation of some aspect of the laboratory environment is the independent variable the experimental manipulation is invariably effected by means of subtle variations in the experimental instructions. Large differences in behaviour have been shown to depend on whether, in the context of an experimental game, one's opponent is described as a 'machine' or as 'another student like yourself' (Abric, 1976); or whether one is playing against 'chance' or against 'nature' (Faucheux and Moscovici, 1968); or whether the experimental task is described as a 'problem-solving' one or as a 'creative task' (Abric, 1971); or whether it is described as involving 'deduction and logical thought' or else as requiring 'the resolution of problems by several individuals collaborating together' (Codol, 1974).

In these experimental studies there is still a separation between

the world of the observer and the world which he observes. The experimenter still remains 'outside of' his subject's cognitive representation of the experimental situation. He can only record the behavioural evidence on the basis of which he will be justified in claiming that the social representation made the difference. Whilst the experimentalists at Aix-en-Provence have conducted many interesting studies on the role of social representations in the dynamics of laboratory groups the experimenter inevitably remains on the outside of such groups. Indeed the individuals comprising the groups may form a group principally *because* the experimenter constitutes an important part of their environment. In these French studies the world of the observer and the world he observes still remain separate worlds. The experimenter, by means of his instructions, introduces the 'representations' into the minds of the individuals whose behaviour he then observes. The 'representations' are cognitive/individual but their mode of delivery is social, that is they are mediated by the experimental instructions. They are implicit in the language which experimenters and subjects speak and understand. It should not, therefore, be too surprising if the laboratory studies carried out within the French tradition do not appear too remarkably different from studies in cognitive science carried out in laboratories outside of France. They do, however, shed interesting further light on the social psychology of the experiment. The French themselves, however, do not seek to relate their laboratory studies to the predominantly American literature on the social psychology of the experiment.

On the social nature of representations and on the difficulty of investigating them experimentally

Whilst Durkheim chose to contrast 'collective' representations with 'individual' ones the members of the contemporary French School choose, instead, to talk only of 'social' representations. This could be a sign either of caution or of strength. It would be a sign of *caution* if they were unsure as to whether or not they can convincingly locate the social phenomena which they describe at a 'collective' Durkheimian level. Here 'social' is used in the *weak* sense of not being equivalent to 'collective'. In relation to Herzlich's study of people's representations of health and illness I have questioned whether, in actual practice, the social phenomena which she so sensitively portrays could be located at the 'collective' level (Farr, 1977). The structure of the accounts which she

obtained seemed to me then to reflect the social context of the research interview in which they had been elicited rather than to reflect the structure of any arguments that might emerge at a 'collective' level. The choice of 'social' rather than 'collective' as the appropriate adjective to qualify 'representation' might merely indicate that the author is not claiming to be a follower of Durkheim. They are, after all, social psychologists rather than sociologists. The field studies within this French tradition are much more obviously 'social' in content than are the experimental laboratory studies. As forms of social psychology they are much more sociological than psychological. The laboratory studies, however, are much more psychological than sociological.

It could be a sign of *strength*, however, if 'social' is being used as a substitute for 'individual', that is there are no purely 'individual' representations. Indeed this is the precise strength of the French critique of the 'individual' nature of most Anglo-American so-called social psychology. If the term 'social representation' were to be adopted within the symbolic interactionist tradition of social psychology it would be used in the strong, rather than in the weak, sense, that is to deny that there is any such thing as an 'individual' representation. It seems to me that the term 'social representation' is often used in this 'strong' form in the writings of the contemporary French School, that is they are less gentlemanly than Durkheim had originally been about leaving scope for the non-social psychologist to explore 'representations'. In his foreword to the second edition of *La Psychanalyse: son image et son public* Moscovici talks of his ambition of setting out from this notion of 'social representation' in order to re-define the problems and concepts of social psychology. Social psychology could then be defined as being the study of social representations in much the same way as it was described, during the twenties, as being the study of social attitudes. By using the strong form of the notion it may even be possible to transform not just social psychology but psychology in general.

A strong form of the argument could be developed along the lines that *all* representations are social because language is social and language is involved in the creation and transmission of representations. This line of argument is consonant with Rommetveit's contribution to this volume. Language, it will be recalled, was part of Wundt's *Völkerpsychologie*. Wundt was aware of the close relationship between language and man's higher cognitive processes. Modern psychology has caught up on him. It is now highly cognitive and language-based. As language is inherently social, both in origin and use, it is psychology which is now the socia

science and not just social psychology. By this criterion even the experimental laboratory studies *within* the French tradition would qualify as a form of social psychology. They are not, as I have argued above, as inherently social as the field studies because the investigators were looking in the wrong place for the social representations, that is they were looking for reflections of them in the behaviour of their research subjects, rather than trying to identify their role in the world of the observer of those subjects. It thus takes the *strong* sense of the word 'social' as qualifying 'representation' in order to make the laboratory studies within the French tradition qualify as a *weak* form of social psychology.

Have contemporary French social psychologists now discovered the secret, which eluded Wundt, of being able easily to move between laboratory and field settings whilst still remaining faithful to the nature of the object of their study? Or, is there still an uneasy relationship between the field and the laboratory studies *within* the French tradition? I personally incline to this latter view. As previously mentioned in the transition from the field to the laboratory an error of translation occurred whereby the laboratory studies became confined to an exploration of the role of representations in the world of subjects, leaving completely unexplored the role of representations in the world of the observer. The role of social representations in the design and execution of laboratory experiments can, however, be explored *provided* one knows *where* to look for them and has some idea of *how* they operate. It is best to regard 'social representations' as being what Moscovici stated them to be, that is 'theories' or 'systems of knowledge' with a logic and language of their own. Properly applied the notion of 'social representation' could help us to arrive at a better understanding of how psychology came to be the sort of science it is and how it came to have the sorts of laboratories it does. The unit of analysis ought to be something as all-pervasive as a psychological theory, for example behaviourism. The implications of the theory for what goes on in laboratories can be studied by observing laboratory life. I have tried to suggest where and how social representations might be operating within the context of laboratory experimentation. I am not alone in feeling that there might be a difference in both style and content as between the field and the laboratory studies *within* the work of the French School. Herzlich (1972), in her review of the field, treats them separately and also implies that there is a clear distinction between them.

It is assumed in much of the above that 'individual' = 'non-social'.

This is true in terms of the way in which psychology has developed historically. It is also the force of the French critique of current orthodoxies in American and British social psychology. There is, however, an important other sense in which the notion of the 'individual' is a wholly social one. This, for example, is true of the study by Lukes (1973*b*) of 'individualism' as a key concept in sociology. In this study the individual almost has the status of being a 'collective representation' which characterises certain societies during certain epochs. It is, perhaps, one of the strongest representations to emerge in Western culture out of the Renaissance. It can best be understood, perhaps, by contrast to the caste system in India as described in Dumont's classic study (1980): *Homo hierarchicus*. The tendency in Europe and America to treat the individual as being responsible for his own outcomes is explicitly stated by Ichheiser (1949) to be a collective representation on the basis of which we praise and blame people for their successes and failures. It is also the representation which explains Lerner's 'just world hypothesis' (Lerner, 1980). This is the representation which is threatened if people do not get what they deserve and do not deserve what they get.

The American social philosopher, G. H. Mead, spent forty years of his life trying to inter-relate 'the facts of individual consciousness' with 'the facts of society'. Wundt, as we noted at the beginning of this chapter, chose to separate these two realms of phenomena. The one realm, that of the mind of the individual, was, for him, an inherently non-social one. The other realm – language, religion, myth, magic and cognate phenomena – was an inherently social one. Mead chose to inter-relate what Wundt had chosen to separate. Mead had been an enrolled student of Wundt's at Leipzig in the winter semester of 1888/9. Back in Chicago he reviewed the early volumes of Wundt's *Völkerpsychologie* as they appeared off the printing presses in Leipzig. The whole of Mead's social psychology developed from Wundt's concept of the gesture (Mead, 1934) and he saw in language the key to understanding the social nature of mind in man. Mind, for Mead, emerged out of interaction within a community of others who share a common language. Mind is thus rooted in social experience. Man's awareness of himself emerges from his interactions with others. Hence the individual is the *product* of social experience. It is now no longer possible to conceive of such a thing as a 'non-social' individual. Mead's thinking is still reflected in the symbolic interactionist tradition of social psychology within American sociology. Both Mead's social behaviourism and Blumer's symbolic interactionism

are consonant with the strong version of the contemporary French research on social representations.

The notion of the individual which the philosopher Strawson developed provides Harré and Secord (1972) with their model for a new methodology in the social sciences. An individual is someone who can monitor his own behaviour and give an 'account' of it. This is an inherently social model of the individual. It is entirely compatible with the social behaviourism of the philosopher G. H. Mead. It is incompatible, however, with the forms of behaviourism which prevail within psychology. This is obvious from a reading of Harré and Secord's volume. This is because the social representation of the individual which prevailed amongst those behaviourists (principally J. B. Watson and B. F. Skinner) who were influential in the development of psychology as a branch of natural science, was a non-social one. Harré and Secord spell out the methodological implications of their social model of the individual. It would lead to a very different conception of the research process and of the laboratory to the one analysed earlier in this chapter. In the context of the behavioural orthodoxies, mainly of experimental research in psychology, their proposed 'new' methodology might appear quite radical. The term new 'paradigm' has been advanced to capture the radical nature of the break with the past which they propose. I am not so convinced that it is such a radically 'new' methodology (Farr, 1977), especially within social psychology where it has been fairly standard practice for some time to elicit self-reports. I also differ quite radically from them in seeing an important future role for laboratories and experimental research within psychology. My own approach is to use the social behaviourism of G. H. Mead in order to make sense of changes in states of awareness within laboratory contexts (Farr, 1978*a*). The simple point I wish to establish here is that there are important implications for *how* one does research depending upon whether one's representation of the individual is, or is not, a social one. This is true irrespective of whether one is interested in experimental research within laboratory contexts (Farr) or non-experimental research in field settings outside of the laboratory (Harré and Secord).

The idea of 'social representations' which emerges from the field studies of the French School is a potentially powerful force for the renewal of concepts within social psychology. In alliance with the views of philosophers and sociologists outside of France who similarly portray the individual as a social representation it could succeed in the process of re-socialising psychology, and not just social psychology.