

Are you a 'real' scientist?

While the modern social psychologist does indeed need experimental, statistical, and computer skills, he needs also historical perspective. He needs immersion in theories (both macro and micro). Above all, he needs an ability to relate his problem to the context in which it properly belongs. Sometimes the context lies in the traditions of academic psychology, often in sociology or anthropology, sometimes in philosophy or theology, occasionally in history or in economics, frequently in the political life of our day. Sometimes the science of genetics or clinical experience provides the context. (Allport 1966: 17)¹

Soon after I received my Doctor of Philosophy degree in 1974, I had what I thought was a peculiar encounter. A man sitting next to me in an airport lounge must have noticed the 'Dr' in front of my name on my luggage tag. He caught my attention and asked me what kind of doctor I was. What I remember was that he took his wrist pulse in a very automatic way while he asked me. I answered with equal ease 'Not the kind that takes the pulse'. We smiled and lapsed back into silence. It was almost as if that small gesture were *the* behavioural indicator differentiating the 'real' doctor from an imposter. Real doctors take the pulse, prescribe drugs, and treat the physical body and presumably it was medical advice that my fellow traveller was seeking. To him I was that other kind of 'doctor'. I might have been a clinical psychologist generally identified with treating the minds of troubled persons and sometimes suspected of knowing what a person is really thinking. When conversations such as these continue and I put forth that I am a social psychologist, I am frequently confronted with a sort of 'Oh, that's nice dear' kind of smile. If I explore the other person's everyday hunch about what it is I do, various intriguing guesses arise – a 'doctor' of the social pulse, a pollster and, once, a census taker.

Well, if those I meet on my travels aren't very sure what a social psychologist is or does, one would expect that at least within the world of the Academy it is somewhat clearer. Yet even here, questions of self-definition often troubled and preoccupied social psychologists to the extent that a literature of 'crisis' developed throughout the 1970s (Elms

1975; Gergen 1973; Israel and Tajfel 1972; Strickland *et al.* 1976). At the core of these writings were concerns about how best to conceptualize the work of social psychologists. Are we to see ourselves primarily as scientists, practitioners, teachers, social activists or social critics?

I was preoccupied during my graduate school years and for at least a decade after with these matters of definition, not surprisingly because they are bound up with theories about what counts as knowledge, how research should be done, and what techniques for gathering evidence are allowable (Harding 1987). This series of essays is about a transition in my way of understanding social psychology. Each essay reflects another way in which I have come to practise a social psychology that is grounded in historical and cultural contexts. For me, the study of experimental social psychology that I began in 1970 has connected to a larger story of the history of psychology and the social sciences.

One part of the history of social psychology involves the emergence of a particular model of human activity that gathers momentum in the Enlightenment period of European intellectual development (Venn 1984). Mind itself became a natural object detached from social relations. As mind became an object with measurable properties the stage was set for the practice of a social psychology about persons as interacting objects, similar to the interaction of objects in nature. A view of minds (and persons) capable of being studied out of their historical and social context fostered a set of research practices that focused increasingly on quantifiable accounts of individual action. The approach has been variously understood as mechanistic and ahistorical, influenced deeply by the models within general experimental psychology, which itself was modelled on a nineteenth-century understanding of the physical world (Brandt 1982).

The history of how and why North American psychological science arrived at models of representing human activity taken from the natural and physical sciences is beyond the scope of this book. Indeed, the mystification of social psychology as a 'real' experimental science and its location in systems of social engineering and control are the subject of much recent research in the philosophy of social science, feminist psychology, history and the sociology of psychological knowledge (Apfelbaum 1986; Collier *et al.* 1991; Danziger 1990; Henriques *et al.* 1984; Lubek 1993a; Morawski 1988; Parker 1989; Wilkinson, S. 1986).

My simpler purpose in this essay is to look at the stories that are currently told to incoming undergraduate students about what social psychology is and how it came to be. The stories are best told in introductory textbook accounts of social psychology's past and through 'histories' of the discipline more often read by graduate students. These are powerful devices for constructing our collective identity as scientists rather than philosophers, researchers rather than political activists.

THE DISCOVERY OF HISTORY²

Until quite recently, Gordon Allport's history of social psychology was the main source for anyone interested in the origins and definition of the field. His 'Historical background of modern social psychology' first appeared in the 1954 *Handbook of Social Psychology* (Allport 1954). It was reprinted in the 1968 *Handbook* with minor modifications throughout the text and a new conclusion (Allport 1968a). In the 1985 *Handbook*, it was retained with some modifications (Allport 1985). Originally, Allport wrote a history of ideas or concepts that had emerged in post-Second World War social psychology. His historical presentation celebrated continuity and progress as social psychology developed from philosophical speculation to scientific respectability. For example, a concept used in the present, such as 'reinforcement theory', is seen as a more scientific restatement of an earlier philosophical notion, in this case Jeremy Bentham's 'hedonism' (Allport 1954: 10–13). Allport maintained that '(O)ur intellectual ancestors, for all their fumbling, were asking precisely the same questions that we are asking today' (Allport 1954: 3). He wrote that '(W)ith few exceptions, social psychologists regard their discipline as an attempt to understand and explain how the thought, feeling, and behavior of individuals are influenced by the actual, imagined, or implied presence of others' (emphasis in original, Allport 1954: 5).

Allport's historical vantage point has been criticized for its presentation of a false continuity with earlier thinkers such as Auguste Comte (Samelson 1974). It can be argued also that it constructs a narrative with an overly presentist and ethnocentric bias. Allport wrote that 'A study of the history of social psychology can be justified only if it shows the relevance of historical backgrounds to present-day foregrounds' (Allport 1954: 1), and that social psychology reached its peak in America because 'It seems that in the United States, the soil of western thought, fortified by practical meliorism, proved most fertile for the assertive growth of social psychology and related disciplines' (Allport 1954: 4). Allport celebrated American social psychology's objectivity while he saw the emerging Russian social psychology as ideologically biased (Allport 1954: 47–8). Allport's history served to legitimate social psychology as a pragmatic, coherent and progressive experimental science aimed at the prediction and control of social behaviour. Yet, Allport came to have concerns about extreme positivism. By the second edition, he lamented that 'The arrival of the positivism that Comte advocated has led to an essentially non-theoretical orientation' (Allport 1968a: 69) and that the over-production of 'snippets of empiricism' (Allport 1968a: 68) was not a good sign. As mentioned above, his history was included in the third edition of the *Handbook of Social Psychology* (Allport 1985) but the critiques brought to bear by theoretical and historical scholarship were not referenced or acknowledged. The section on Auguste Comte was simply excised.

Archival work in the Allport Papers at Harvard University Archives provided me with a better sense of how Allport's history of social psychology evolved. As Gardner Lindzey and Eliot Aronson set to work (in 1963) on the second edition of the *Handbook*, Allport was given the opportunity to revise his history and was asked to write a chapter on current trends in the field. He was satisfied for the most part with his historical analysis and had his manuscript to the editors by May 1965. It was copy-edited by a student in 1967 as Allport was by then too ill to do this task. He died on 9 October 1967. While he had declined the task of writing a 'current trends' chapter for the 1968 *Handbook*, he did present a paper entitled 'Six decades of social psychology' at a Conference on the Teaching of Social Psychology in December 1966. The conference was held in New York under the auspices of the National Science Foundation and the Division of Personality and Social Psychology of the American Psychological Association. Allport's paper was subsequently published in the conference's edited proceedings (Allport 1968b).

In Allport's conference presentation, he expressed serious concerns about a social psychology that developed without attention to history or context. His 1968 *Handbook* chapter is worthy of understanding in contrast to this conference paper;³ the former has much to do with Allport's love of grand recurring themes in the history of thought and what he called in his conference paper 'an appropriate eclecticism in psychological theory' (Allport 1966: 5). In the conference paper, Allport expressed his concerns more strongly that the student was being 'fed fragments, not theory' (Allport 1966: 13) and that the 1950s and 1960s predisposed us to 'inflate our methods into "methodologies" because we are so conscious of them and so childishly proud' (Allport 1966: 11). He spoke of this obsession with methods that 'seems to betoken a drift whose significance is not yet fully clear' (Allport 1966: 11). While still attached to 'clear methods', 'an accurate handling of data', 'severe self-scrutiny in research' (Allport 1966: 11) and as an optimist to the end about what science could offer society, Allport none the less concluded that 'We do well to face frankly our failure thus far to demonstrate our claim to be a powerful theoretical and applied social science' (Allport 1966: 15).

In Allport's view, social psychology was a young science and had become sidetracked by endless empiricism. He reported his observation that 94 per cent of the references in articles in the July and August issues of the *Journal of Personality and Social Psychology* for 1966 were to work done after 1950. His caution to students anticipated the oncoming crisis, even if he was too ambivalent and diplomatic to take a stand in print. The following passage in the unpublished version (Allport 1966) was omitted from the published version (Allport 1968b) as 'too censorious':

And so my final word is this: Let no student think that by grinding out a hasty dissertation to refute some equally hasty dissertation of the

previous year, he becomes thereby a permanently trained and qualified social psychologist – not even if his dissertation scintillates with an awesome 'methodology'.

(Allport 1966: 17)

What followed in the second chapter of the *Handbook's* third edition was a lengthy piece by E.E. Jones (1985a) entitled 'Major developments in social psychology during the past five decades'. Unlike Allport, his goal was to survey a shorter past. He planned to 'concentrate on American social psychology with only occasional references to European developments' (Jones, E.E. 1985a: 47) in a time-frame involving post-Second World War social psychology as practised within the disciplinary infrastructure of psychology. While he acknowledged that 'many outstanding contributors to the field were European refugees' (Jones, E.E. 1985a: 47), without a deeper historical treatment Jones, among others, could not explain '(W)hy Lewin was invited to the Iowa Child Welfare Station in 1934 or why attribution theory flowered in Kansas, to which Fritz Heider moved in 1947' (Jones, E.E. 1985a: 54).⁴

By excising social psychology's temporal and spatial history, the field was inevitably distorted. Other cultures were made to appear static and backward:

Social psychology would be less prominent, if it existed at all, in more homogeneous and traditional cultures. In such cultures behavior priorities within settings are rather well established by unquestioned cultural norms. Conflict is minimal as everyone more or less follows the traditional ways. The presence of competing normative options may account, in part, for the distinctive flowering of social psychology in the United States.

(Jones, E.E. 1985a: 53)

Within the American context, his focus on social psychology as a product of large urban universities forced distortions on the rest of American society. It was in the cities, he argued, that 'problems of intergroup conflict, prejudice, deviance, and attitudinal differences were the most salient. Indeed, a rural social psychology is almost a contradiction in terms' (Jones, E.E. 1985a: 53).

Social psychology was contextualized historically by Jones as inherently liberal and progressive, a 'troublesome disrupter of the status quo' (Jones, E.E. 1985a: 53) but those parts of its history that have either been more radical or more preserving of the *status quo* were missing. Why this narrowing of focus in all aspects of social psychology's history?

My reading of this 'history' is that it was written to serve a particular purpose, namely, to legitimate the social psychologist as a 'real' scientist. Within the context of the discipline of psychology, Jones applauded the

arrival in the late 1960s of social psychology as modestly respectable given 'additional impetus stemming from a new perception of social psychology as constructively linked to the experimental method and therefore entitled to a place in the psychological mainstream' (Jones, E.E. 1985a: 54). It remains to be seen whether Jones's reconstructions of the antecedents of many fields of social psychological inquiry will withstand the test of historical scholarship. What is clear is that the real social psychologist is a scientist.

In an overlapping paper (Jones, E.E. 1985b) entitled 'History of social psychology', the discipline as an expression of a natural science model was more succinctly framed. In that paper, Jones took issue with Allport's 'impressive continuity in the problem focus of nineteenth-century social theorists and contemporary investigators' (Jones, E.E. 1985b: 373). The central advance brought by the 'availability of experimentation as a research paradigm' (Jones, E.E. 1985b: 374) was the cornerstone of what Jones took to have been progress in the field. The false start with Wilhelm Wundt's disbelief that there could be an experimental social psychology was overturned. Jones wrote: 'sometimes people don't get the word, labels are ignored, and new paths are opened as if by accident' (Jones, E.E. 1985b: 374). Triplett's experiment in 1898; Floyd Allport's behaviouristic textbook *Social Psychology* published in 1924, followed by other milestones in social measurement; experimental method; and even the rise of theory were selected to represent advancement (Jones, E.E. 1985b: 375-7 and *passim*).

All of this led Jones to conclude that the challenge to social psychology in the 1970s posed by a literature that suggested a crisis in social psychology of intellectual, ethical and practical significance was not fundamental. The acceptance of a naturalistic metaphysic remained unshaken although he acknowledged that 'There is an articulate minority of social psychologists who may view my remarks as the dying gasp of a maladaptive breed and who question the very possibility of effective and illuminating experimentation on social psychological processes' (Jones, E.E. 1985b: 404). Jones remained convinced that one could best understand social life with the present 'cumulative, objective, science of social behavior' that has the 'special advantages of control, quantification and comparison' (Jones, E.E. 1985b: 404). Jones's 'history' tells a story of discontinuity. It is less a history and more a polemic; it is a story about how social psychology became a 'real' science, how much better off we are than in the pre-scientific days gone by and why it is important to stay the way we are.

Telling the history of social psychology as a story about continuous progress from the pre-scientific past or as a story about an enlightened present that is discontinuous with the past allows for two self-conceits. In the first case, one can take pride in having the good sense to join in the

march of progress, that is to be on the winning team. In the second case, one can revel in how much more modern one is than the superstitious ancestors. Both versions bolster the view of the social psychologist as arrived, that is, as a 'real' scientist.

Not everyone has abandoned the possibility of an historically contextualized social psychology. For example, Carl Graumann in his 'Introduction to a history of social psychology' starts out with the question 'Why study history?'. Concerning disciplinary history, he argues that:

We *can* learn from history, if it has not exclusively been written for identificatory and justificatory purposes as is mainly the case in 'presentist' history. In order to be useful a history of a discipline must allow for the discontinuities, drawbacks, failures and dead ends as well as for continuity, success and progress. It must not pretend unity if there is pluralism as in social psychology. . . . For disciplinary history the context is not only the system of sciences, but the social, political, and economic system within which an individual discipline develops. (Graumann 1988: 4-5, emphasis in original)

Graumann locates social psychology in the broader context of social philosophy and argues that 'The decision how far to extend the past or the history of social psychology and whom to include is a function of a writer's present conception of the social and the psychological' (Graumann 1988: 5). His acknowledgement of pluralism begins to tell us that there are several histories. Graumann would likely agree that the answers to why Lewin was in Iowa, Heider in Kansas, or Tajfel in England, require an historical examination of Hitler's Europe that 'emptied most of Europe of whoever and whatever there was in social psychology' (Graumann 1988: 15). Indeed, Graumann achieves a reconstruction of a pluralistic social psychology by contrasting its development in America with European social psychology before and after the Second World War. Social psychology developed within the context of world political and economic events: the Depression years in the United States; Hitler's murder of European intellectuals or their forced migration; and the American economic reconstruction of Europe after the war. By examining the long past of social inquiry and the stubborn particulars of how social psychology has been practised in different cultural and institutional contexts, one can then say more of what the 'history' of social psychology has been.

What is called for is critical historical work about disciplinary development that would include an analysis of traditional historical authorities, of the historians' own assumptions and of the assumption that the discipline has followed a steady path to progress (Danziger 1984). Critical historical work can show us, for example, that the very debate about the role of the

social psychologist as political activist or detached scientist has a long history and preoccupied discussion in the 1920s and 1930s in a manner similar to the 1970s (Minton 1984; Nicholson 1991). Similarly, an even stronger critical historiography pushes us to look at the very objects of our study, as phenomena that 'do not occur in nature as raw givens but are the product of human construction' (Danziger 1984: 100). This allows for the possibility of examining the psychological objects of social psychology in the context of the social interests that are served by them.

Current pedagogical practices for introducing social psychology to large numbers of students frequently sidestep the historical dimension. Textbooks are now the primary resource for pedagogy in social psychology, especially in North America. They tell the contemporary story of social psychology as an experimental science and of the social psychologist as an experimental scientist (Lubek 1993b). Despite evidence to the contrary, textbooks provide the 'origin myths' for discussing Comte as the founding father of social psychology, McDougall's social psychology text as the first one in the field, and Triplett's experiment as the first in experimental social psychology (Haines and Vaughan 1979; Rudmin 1985; Samelson 1974). The presentation of social psychology as a cumulative science, originating from founding fathers, texts or classic studies and continuously making progress and new discoveries is conveyed in such a routine way that textbooks become a standardized and uncritical form of pedagogy.

Textbooks in social psychology have been acknowledged as the most pervasive means of communicating the discipline to incoming students. Alcock wrote that our textbooks served two main purposes, 'first to transmit knowledge from one generation to the next, and second, to inculcate specific values in its students, even though these values are often taught without conscious awareness by the teacher' (Alcock 1978: 2). Jones, in his review of five decades of social psychological research, noted that 'textbooks in social psychology have played a distinctive role in shaping and integrating the field' (Jones, E.E. 1985a: 49). He further wrote that 'It is hard to think of another field in which textbooks have served as such an important vehicle for theorizing about, and generating influential distinctions within their subject matter' (Jones, E.E. 1985a: 49). Textbooks themselves provide us with a rich archival source of our philosophical and cultural values as well as evidence of our preferred research practices. They are part of building a socio-historical understanding of social psychology.

To gain a better understanding of the metatheoretical positions expressed in textbooks of social psychology, Ellen Corkery⁵ and I undertook a study of thirty textbooks published between 1925 and 1985 selected from the list provided in Jones's review paper (Jones, E.E. 1985a). For her part of the analysis, she used a content analysis that differentiated a natu-

ralistic from an historical conception of science (Levine 1976). Corkery examined the textbooks to see which, if either, of these approaches was predominant in textbooks of social psychology. Levine had previously used this distinction to examine research in journals of social psychology that were psychologically or sociologically based. He noted that the two conceptions of science both assumed that there is an orderly social world that can be known through systematic study. Beyond that, the naturalistic and the historical conceptions were differentiated by assumptions made respectively about stability rather than variability of social behaviour, the researcher's values as separate from rather than part of the research process, and the ahistorical and lawful nature of social behaviour rather than its context and time-dependent nature. These contrasting themes were intensified in the 'crisis' literature that developed in the mid-1970s but Corkery and I had a hunch that the contrast would not be reflected explicitly in the messages conveyed to incoming students of the discipline. In actuality, mention of social psychology's history in our survey of textbooks was scant, largely drawing on Allport's (1968a) history of the discipline, and stressing the empiricist philosophers, the early textbooks by McDougall and Ross in 1908, or the earliest experiment by Triplett. Current knowledge was presented as cumulative and progressive, and fewer authors (e.g. Sampson 1971) explicitly noted the discontinuous and historically contingent nature of the discipline. A 'crisis' or period of re-examination in social psychology was mentioned in three of the six texts that covered the period from 1970 on (Secord and Backman 1964; Shaver 1977; Wrightsman and Deaux 1981).

In fact, Corkery found that while all of the textbooks were attempts at systematic inquiry into the order of social behaviour, a full two-thirds of them adopted the naturalistic conception of science for pursuing this inquiry. At the outset (often in an introductory chapter), the textbook authors took special care to qualify social psychology as 'scientific', 'experimental', and 'objective', and to include in subsequent chapters primarily experimental laboratory research to support this view. About 50 per cent of the textbook authors presented social psychology as a natural rather than an historical science by augmenting their introductory comments with a characterization of the basic principles of science *per se*, using descriptors such as 'objective', 'value-free', 'reliable', 'experimental' and 'ahistorical' and using descriptions of the goals and ideals of scientific practice. Other techniques used to legitimate social psychology as a natural science rather than an historical one involved an emphasis on quantification and a distinction made between social psychology and other fields of social inquiry: sociology, anthropology, philosophy or history itself.

In textbooks where authors used terms such as 'culturally dependent' and 'value-laden', they often went on to argue for cross-cultural

investigations to ensure the universality of social principles or to advocate methodological innovations to reduce or eliminate biases from research. The idea was not acknowledged that hypothesis generation and data gathering were themselves social activities bound by one's cultural context. Neither was the idea that biases in our investigative practices are something we cannot get rid of and thereby achieve an objective measuring standard.⁶

Our investigative practices grow out of particular cultural and social circumstances and, while cultural values can be made explicit, they remain an integral part of the research process. Thus, 'Investigative practices do much more than order observations of a world that is given – they actually prepare the world that is there to be observed' (Danziger 1990: 195). Experimental investigation with human subjects, constructed in textbooks as the mark of 'real' science because it affords the experimenter 'control', has itself a long and varied history and is itself subject to socio-cultural meanings. Danziger writes:

[H]uman subjects in psychological experiments are in fact unable to behave simply as natural objects. Even if they try to do so, which depends entirely on their appraisal of the social situation they are in, they negate this fictional goal in the very act of trying to reach it, because such efforts represent an exercise of their social agency. Psychological experiments are therefore different in principle from experiments in physics because the experimenter and the human data source must necessarily be engaged in a social relationship. This is no 'artifact' but one of the essential preconditions of having a viable experimental situation.

(Danziger 1990: 9)

In departments of psychology, the practice of social psychology legitimated itself professionally through the techniques already established in the medical and physical sciences of the nineteenth century. It was the human subject as natural object rather than as active participant that emerged and took root in North American laboratories in the early part of the twentieth century. While there was considerable variation in investigative practices, the relationship between the person and society with which we currently work became highly stylized in the experimental social psychology practised during the 1950s to 1970s. This period reflected the natural science conception of social psychology that saw the use of investigative practices as value-neutral tools. More consistent with the historical conception was the notion that investigative practices were themselves social and historical products.

Despite the consistency of Corkery's sample of textbooks in conveying a naturalistic rather than an historical conception of science, there were important exceptions that I discovered by reading the textbooks' prefaces,

a section that revealed the variety of reasons textbook authors gave for writing their books. Prefaces⁷ formed a narrative account of how a social psychologist wanted to present the world in the pages that followed. Quite frequently the author would write in the third person, 'the author(s)', which had the effect of creating greater distance, impersonality and the detached objectivity more appropriate to the assumptions of the naturalistic conception of social science. Other prefaces revealed exceptions.

From reading prefaces I learned that the range of stated goals in textbooks before the mid-1970s was quite broad. Authors of social psychology textbooks wrote to present a specific orientation such as field theory, symbolic interactionism or social psychology as an extension of the principles of general psychology. They wrote to convey what they took to be the unique subject material of social psychology, be that person perception or group dynamics. Some authors wrote their textbooks to improve upon existing material or to 'upgrade' the material. Secord and Backman wrote:

We believe that the first course in social psychology should be upgraded. We have experimented with 'toughening' introductory social psychology . . . and the results, even for students of average ability, are encouraging . . . In our opinion, a course which presents 'watered-down' material, which emphasizes practical affairs at the expense of scientific knowledge and methodology, and which omits the more difficult literature does a disservice to the student and to the field of social psychology. Upgrading the subject as this book does should produce a course which is no more difficult than many others taught in the junior year, such as advanced courses in the natural sciences.

(Secord and Backman 1964: Preface)

In contrast, another author surveyed students' preferences for materials of interest, the personal importance of the topic and its help in understanding their own social relationships, and the intellectually stimulating nature of the material. One might expect this to be a textbook of the 1960s but in fact, it was Bird's social psychology textbook of 1940 (Bird 1940).

Whereas textbooks throughout the 1980s became more standardized and focused on comprehensive reviews of research, earlier models were more personal and selective, attempting to make the social psychologist's task more intelligible in some systematic way. Among the textbooks of the 1970s were a few written in the first person and reflecting something closer to an historical conception of social science (Elms 1972; Sampson 1971; Samuel 1975). Some of these textbooks were challenges to the prevailing and quickly solidifying dominance of the natural science conception of social psychology. Sampson warned that 'a text often makes more sense out of a field than there really is' (Sampson 1971:

Preface) and stated that his goal was to interest students in a field in which their reading might lead to action. In subsequent chapters on the American student and black protest movements of the 1960s he dispelled the notion of value-free knowledge by including an account of his own involvement and sympathies for these social movements. He wrote, 'I do not trust anyone who purports to discuss these controversial topics but disclaims bias or personal interest' (Sampson 1971: 357). He argued for considering the observer's framework if one wants to understand the judgements being made in any area of social research and that this is no less true for the textbook author than the subject of research on social perception and judgement. Some of the biases in any textbook account are 'conscious knowns' while others are 'a hodgepodge of more or less unconscious motives, dreams, and blinding defenses' (Sampson 1971: 357).

Likewise, Elms began his textbook with an 'immodest chapter' in which he expressed his personal and professional commitments in social psychology:

During my undergraduate years, I knew few students besides myself who looked for relevance in their courses. When I found it, I took it as a bonus, over and above the pleasures of the scholarly games to which my professors seemed devoted. But even as I continued to search, a large new audience of undergraduates were growing up to demand relevance as their due. Students' demands have become an occupational hazard of college teaching, but this is one I can welcome.

(Elms 1972: 2)

Furthermore, Elms intended to write about areas of research with which he had first-hand personal involvement and in 'nontechnical terms, in hopes that they may gain a wider audience among concerned laymen, whether in or out of college' (Elms 1972: 2). In this way he let the reader know that he was writing 'a distinctly personal book, rather than an impartial survey of social psychology' and that 'It won't always be possible to make sense of the data' (Elms 1972: 10).

C.W. Sherif's textbook, *Orientation in Social Psychology* (1976) was an even stronger example of a textbook reflecting an historical approach. Sherif began with the premise that social psychological knowledge reflected not a set of accumulated facts but a way of going about knowing. She emphasized research as a process not an outcome. Social behaviour was not neatly and tidily understood through unchanging laws but rather it was the way in which some individuals found it useful to approach persistently pressing problems in society. It was one way of making the world intelligible that was itself embedded in an historical and cultural context. The newcomer to social psychology was not provided with a discrete set of facts and findings in Sherif's textbook but

rather with an approach filtered through a personal vision, without being simply anecdotal or unsystematic.

C.W. Sherif prefaced her book by reflecting on her purpose in writing it. She wrote, 'I started writing this book because I wanted to help newcomers find their bearings in social psychology' (Sherif 1976: xi). She went on to say, 'In all conscience, I could not write about social psychology as an established, coherent body of "knowledge" that I could parcel out in neat bundles' (Sherif 1976: xi). She would not include a complete summary of research but rather her aim would be to show how research comes into being. In this regard, she did not present a view of herself as the objective social scientist putting knowledge into the heads of eager students. She presented herself more as a model for how anyone might go about trying to make sense of complex issues of social behaviour.

Sherif stressed that all knowledge is filtered through a conscious self in interaction with others and that science is best understood as a personal process. This meant that the social psychologist 'is inevitably part of what he or she is studying' (Sherif 1976: 362). The personal aspect was also evident in her attempts to bring students into the discipline. In her preface she wrote:

The only way that I could translate my aims into this book was to regard the challenge as personal. In writing, I am very much there in first-person singular . . . I've not hesitated to bring up controversial issues that will continue to be important or to pass my own judgments about them. Here the first person singular may be a distinct advantage for the reader. It should be easier to take issue with my personal statement than with a judgment rendered implicitly through impersonal discourse or attributed to a mythical collective wisdom.

(Sherif 1976: xii-xiii)

C.W. Sherif wrote a candid comparison to her previous textbook, co-authored with her husband and colleague, Muzafer Sherif (Sherif and Sherif 1969) that reflected their differences in approach. About writing in the first person, she said, 'The experience was at times disturbing to someone like me, accustomed to writing impersonal journal articles and books, often behind the doubly protected façade of co-authorship with M. Sherif' (Sherif 1976: xii). Of the 1976 text, she informed us that it was written 'without the invaluable guiding hand of the senior author of her earlier work' and further noted, 'He did his best to persuade me to excise from the manuscript the personal context for his work. Unfortunately, I could not do so without severing part of the first-person singular from my narrative' (Sherif 1976: xiii).

C.W. Sherif observed that 'There is a strong tendency in social psychology to concentrate on stability, regularity and continuity in

social behavior' (Sherif 1976: 362) and that investigative methods derive from this search for regularities. It is often personally disturbing to the social psychologist when 'social change exceeds the bounds of what social psychologists' own self-system and reference groups define as "normal" or "desirable"' (Sherif 1976: 362). The dilemma for social psychology, she said, was that 'There can be no valid laws or principles about social behavior that do not accommodate change as well as stability and continuity in social behavior' (Sherif 1976: 362-3). Her analysis of social movements – a field noticeably absent from social psychology textbooks of the 1980s – was a compelling call to use these movements as laboratories for researching social change. Interestingly, research methodology was woven throughout her textbook, about which she said:

Out of context, issues of research methods can easily appear to be nit-pickings, or worse yet 'technical matters' of concern only to technicians. They come alive within their proper contexts, which are efforts by human beings to inquire into mysteries and significant issues of the human condition . . .

(Sherif 1976: xii)

Through a dialogue with the reader, this text reflects something closer to the historical conception of science with its emphasis on social change, its description of the cultural and historical contexts for research studies and its insistence that social psychology and the social psychologist are neither value-free nor value-neutral.

To present social psychology within a natural science framework requires that it be written not as the personal life's work of an individual or group of individuals interested in the social world but as the most up-to-date account of 'the latest findings' and 'breakthroughs' on given topics. Research studies are reported for their significant findings and decontextualized from the researcher's sociocultural background and from the history of the field itself. What creates the illusion of a coherent and 'real' scientific social psychology is a set of standardized topics and a central commitment to reporting the latest experiments on these topics.

To present social psychology within an historical framework requires teaching from a perspective that acknowledges a broader field of social enquiry to which social psychology belongs and to locate social psychology in its diverse practices around the world and at different historical moments. In this regard, it is a useful starting place to compare textbooks and 'histories' of social psychology according to how they have constructed the discipline. Where and when did contemporary American social psychology originate? Out of what political, economic and cultural context? Historical research examines both the continuities and discontinuities in social theorizing and investigative practices, looking at what

has fallen by the wayside and what has been systematically suppressed (Danziger 1990). Historical research examines how social psychology has developed in other parts of the world (Moghaddam 1987; Strickland 1991) and within the context of national disciplinary infrastructures (Hilgard 1987). Within an historical framework we are more likely to uncover the cultural, economic and political assumptions in our work and the implications these have for social action. Our work is more likely to become inclusive of epistemic differences to the extent that we can become less preoccupied with our identities as 'real' scientists.