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A SPANNER IN THE WORKS:  
A CRITIQUE OF SOCIAL PSYCHOLOGY.

BY

KEVIN CHRISTOPHER WILBY

Thesis submitted for the degree of  
Doctor of Philosophy  
at the  
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## ABSTRACT

A critique is made of experimental social psychology, in which its claim to give a "deep" understanding of human social behaviour is examined, and found to be groundless.

Social psychology's methodological and theoretical assumptions are traced to the work of Kurt Lewin, whose advocacy of a "Galilean", scientific approach to psychology seemed to allow the use of experimentation on human motivations and higher order thought processes. Examples of contemporary social psychological research are examined, revealing the implicit Lewinian assumptions they make use of.

It is shown that Lewin's assumptions are without foundation, because they distort acknowledged facts about human beings by their imposition of a causal explanatory system onto all instances of behaviour. Lewin's "topological psychology" involves, amongst other things, the translation of common sense notions into a quasi-technical terminology. It is contended that this terminology contributes nothing to a more sophisticated understanding of people's behaviour. This contention is supported by an examination of the methodological implications associated with Lewin's terminology, and also by contrasting it with accounts that emphasize the diversity of explanatory forms in ordinary use.

It is also shown, through a discussion of philosophical skepticism, that social psychology's justifications for assuming a fundamental inadequacy of common sense knowledge of behaviour, and the appropriateness of a general scientific approach to behaviour, are over-extended, and that while the motivations of behaviour are not always readily apparent, there is no serious reason to disregard all ordinary explanations of behaviour.

Having shown the weaknesses inherent in social psychology's assumptions and justifications, it is concluded that social psychology cannot claim to have discovered a replacement for ordinary, common sense, explanations of behaviour.

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## CHAPTER ONE

Social psychology portrays itself to the world, to its students, and to the general public, as a science, and it claims the respectability and status that the title "science" carries with it. Social psychology promises to provide knowledge, not opinions, about people's behaviour, and amongst other things, about the way in which people make judgements, come to decisions, and present themselves in social situations. In this thesis it will be suggested that social psychology does not actually provide any "superior" knowledge, but instead supplies truisms and confusions when dealing with behaviour, making its promises empty, and its claim to give a "deep" understanding of human social behaviour unfounded.

Showing the source of social psychology's errors involves a multi-disciplinary approach in the first instance, because to understand its errors and the temptations to make them, we need to go "back to basics". A historical approach is necessary initially, to show the sources of many important ideas which are simply taken for granted in contemporary social psychology, and which are therefore difficult to identify from an analysis of contemporary usage. When these ideas are seen clearly, a philosophical approach is needed to assess the strengths and weaknesses of these ideas, or concepts, for the job which social psychology sets them. Accordingly, the first part of this thesis is concerned with an exposition of social psychology's fundamental concepts, and the contexts in which they became accepted.

This exposition is necessary for two main reasons; to delineate important ideas by seeing them as solutions to definite historical problems, and to help in understanding social psychology in its own terms; to provide an account of social psychology which is substantial enough for any criticism made of it to be meaningful. Making this exposition allows philosophical issues to be raised, and social psychology's claims and promises assessed.

Social psychology does not have a long history, a fact which social psychologists themselves often mention, and one which we will have reason to discuss in due course. Social psychology seems to have emerged as a field of enquiry around the turn of the century, its interest being in studying people's dealings with each other, and with "society" in general. Within such a broad subject area there have been, as might be expected, many different approaches and many different methods used to study the interaction of individual and society. Looking back briefly into social psychology's past can help to distinguish between these different approaches, and can also help to show the philosophical and methodological assumptions which have been made in the past, but which are still in use as the implicit foundation upon which modern social psychology stands. Showing these assumptions, in their original and purest forms, and subjecting them to critical scrutiny, can go a long way towards diagnosing the dissatisfaction which many people, psychologists and non-psychologists alike, have felt about social psychology.

As has been mentioned, there is more than one variety of social psychology, so the criticisms which will be made may not be effective against all investigations which call themselves by this name. Specifically, experimental social psychology will be discussed, with the intention of showing why its promise to give a deep understanding of human behaviour, and of the motivations behind behaviour, cannot be fulfilled. As the first step toward carrying out this intention, one of the most important contributions to social psychology's development in its short history will be sketched and examined.

This influential contribution will be shown through the examination of some of the discipline's textbooks. This exercise is worthwhile for several reasons. As Kuhn points out in The Structure of Scientific Revolutions (Chapt XI) [1], both scientist and layman take much of their image of creative scientific activity from an authoritative source, namely textbooks of science (along with the popularizations and philosophical works modelled on them) (p 136). Textbooks (and the other works mentioned) "address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are written. Textbooks...aim to communicate the vocabulary and syntax of a contemporary scientific language" (p 136).

Although he is referring to textbooks dealing with the natural sciences, the same idea holds true with respect

to social psychology textbooks. In fact E.E. Jones, in The Handbook of Social Psychology (1985) suggests that "textbooks in social psychology have played a distinctive role in shaping and integrating the field" (p 49). He goes so far as to say that "it is hard to think of another field in which textbooks have seemed such an important vehicle for theorizing about, and generating influential distinctions within, their subject matter" (p 49).

If textbooks perform the functions Kuhn suggests, and have the importance which Jones and others attach to them, an examination of them should show how social psychology sees itself, and also how it wishes to be seen, if they really are the "pedagogic vehicles for the perpetuation of normal science" (p 137) which Kuhn asserts. Being pedagogic vehicles means that an examination of them will show both what the discipline wants to be known about itself, and also what it does not wish shown. Kuhn suggests that for textbooks to perform their function, a part of which is the pedagogic role, "they need not provide authentic information about the way in which those bases [of the current normal-scientific tradition] were first recognised and then embraced by the profession...there are even good reasons why, in these matters, they should be systematically misleading" (p 137).

Textbooks truncate the scientist's sense of the discipline's history, and then supply a substitute for what they have eliminated:

Characteristically, textbooks of science contain just

a bit of history, either in an introductory chapter, or, more often, in scattered references to the great heroes of an earlier age. From such references both students and professionals come to feel like participants in a long-standing historical tradition. Yet the textbook-derived tradition in which scientists come to sense their participation is one that, in fact, never existed. For reasons that are both obvious and highly functional, science textbooks...refer only to that part of the work of past scientists that can easily be viewed as contributions to the statement and solution of the text's paradigm problems. Partly by selection and partly by distortion the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific. (p 137/138)

Kuhn suggests that textbook histories are re-written after each scientific revolution, so as to make the scientific effort seem cumulative at the same time as disguising the fact that the revolution has taken place. In doing this, an impression is created that the science or discipline "has reached its present state by a series of individual discoveries and inventions that, when gathered together, constitute the modern body of technical knowledge" (p 140). The implication is that "scientists have striven for the particular objectives that are embodied in today's paradigms. One by one, in a process often compared to the addition of bricks to a building, scientists have added another fact, concept, law, or theory to the body of information supplied in contemporary science text" (p 140).

There has been some debate in the literature [2] as to whether social psychology has a paradigm in Kuhn's sense, but in some senses this debate is otiose. R.I. Watson (1966) [3] suggests that psychology as a whole may lack a "Kuhnian"



paradigm, but he also notes that this does not mean that it proceeds randomly in its studies. He argues instead that psychology as a science "functions at the level of guidance by prescriptions" (p 111). What he means by "prescriptions" are the various guidelines which, although they do not have the all-embracing unifying effect of defining the field of enquiry in the way that a Kuhnian paradigm is meant to, still guide what the psychologist does in his or her work. Thus Behaviourism, or Gestalt psychology, in as much as they are a set of interlocking prescriptions espoused by a group of scientists, can serve this function.

There are undoubtedly guidelines which social psychologists adhere to as they go about the business of research, as we will see. These guidelines function as a paradigm, in that they are basic principles which workers in the field agree about, this agreement allowing them to share ideas and discuss problems with each other. Such ideas are the basic foundations upon which social psychology is built, meaning that they must be taught to new social psychologists as part of their training. It seems obvious therefore that social psychology textbooks will play the pedagogic role Kuhn believes they do, and that there is much to be gained by looking at the principles that they try to impart to their readers.

A recent example of the textbook tradition in social psychology is Hewstone et. al.'s Introduction to Social Psychology published in 1988 [4]. Their hope as editors of this textbook was that the best research in United States and

European social psychology could be presented in such a way as to give a representative survey of the field. At the same time they hoped to contribute to the future shaping of the field. This second hope arose from what they saw as the function and usefulness of a textbook. They note that "once upon a time there were textbooks of social psychology that were at the forefront of the field", playing the role we have seen Jones chronicle. These books were read by students and teachers alike, were widely quoted in the research literature [5], and provided an impetus for new and challenging research, and thus helped social psychology to develop.

Hewstone et. al., in attempting to place their own textbook alongside the great and important textbooks of earlier generations, set themselves the goal of creating a book that is representative of, and at the same time, a guide for, social psychology as it continues to develop, and to progress.

Given that the book has these aims, and that its contributors and editors are acknowledged experts in the areas they write about (or they would not have been asked to contribute) and could thus be expected to give a "'state of the art' presentation of both European and American work" (Preface, XVI), it does not seem too unreasonable to use this text as a resource for an investigation into social psychology. Other texts will also be used, but with the same aim; to give as clear a picture of social psychology as it is at present, and of its development, as possible. The Hewstone

et.al. text will be used mainly to exemplify aspects of the present state of social psychology, while the other texts will be used to show what many social psychologists have felt to be an important influence on social psychology reaching its present state of development.

As a guide to past developments in social psychology we will use the historical chapters in The Handbook of Social Psychology (1985). The chapter written by E.E. Jones, which has already been mentioned, will be used as a principal source in the analysis, because The Handbook in which it appears is generally seen to be a "standard" work (as its editors recognize in their preface) and so it should fulfil the functions which Kuhn sees textbooks as having. It is at present in its fourth incarnation, the first being Murchison's 1935 Handbook, the other editions being in 1954, 1968, and 1985. Jones is a recognized and respected researcher himself. His textbook, written with H.B. Gerard, is one of the books Hewstone et.al. regard as being a significant contribution to social psychology, and his other works are frequently cited in the literature, so we can trust him to produce a pedagogic vehicle for the perpetuation of social psychology, and thus, to show the historical path which modern social psychology has chosen as its most flattering portrait.

When characterizing or identifying social psychology, it is important to be clear about what it is we are discussing. As has been mentioned, there is more than one kind of social psychology available to anyone interested in

studying human social behaviour. In fact, it is interesting to find that analysts who have engaged with the question of what social psychology entails have agreed that there are several varieties "on the market", but they have not reached a consensus as to what the different forms of social psychology are. There is an extensive literature on the subject, in which the following papers, showing the problems involved in a discussion of social psychology, are frequently cited.

For example, House (1977) reports identifying three kinds of social psychology: 1) Psychological social psychology, which focuses on psychological processes in relation to social stimuli, using laboratory experiments: 2) Symbolic Interactionism, which is considered to be the sociological variant of social psychology, and is characterized by the study of face-to-face interaction via naturalistic observation: 3) psychological sociology (or social structure and personality) another sociological variant of social psychology which relates macrosocial phenomena (eg. organisations, societies and aspects of the social structures thereof) to individuals' psychological attributes and behaviour, usually using quantitative but non-experimental methods (p 161/162).

Farr (1978) also isolates three varieties of social psychology, but does so in a combination slightly different from the above analysis. He considers three separate traditions of social psychology: 1) The symbolic interactionist tradition within American sociology: 2) The

French tradition of research on "representations sociales" which looks back to Durkheim for inspiration: and 3) Experimental social psychology. [6]

However the social psychology cake is sliced, there appear to be two main orientations, sociological social psychology (SSP), and psychological social psychology (PSP). This division is not all that unexpected, if we consider that psychology deals with people, and sociology deals with these people in the institutions and structures which they create to help run their lives. Sociology is concerned with what is social in this sense. What could have happened in the genesis of social psychology was that a synthesis be created, in which aspects of sociology and psychology were brought together to form a social psychology which dealt with the phenomena that inhabit the borders between the two disciplines. However, such an expectation is confounded by the reality of what actually occurred.

Wilson and Schafer (1978), drawing on the work of House (1977) (and Stryker, 1977), acknowledged that although "social psychology is often recognized as an interdisciplinary endeavour, bringing together the fields of psychology and sociology...the two disciplines have taken different approaches to the study of social psychology. A typical textbook in each discipline reveals many differences in terms of theories, topics studied, and methods used" (p 548). Wilson and Schafer tried to assess to what extent social psychology was an interdisciplinary cross of sociology

and psychology by asking a sample of self-acknowledged social psychologists questions about the topics they researched, the methods they used, the textbooks they used, the journals they read and published in, the social psychologists who they felt had made the greatest contribution to the field, as well as the training they had received.

They found that "little overlap existed with regard to such data between the groups of sociological and psychological social psychologists" (abstract: p 548). "The two camps largely ignore each other, consider each other irrelevant, or are unaware of each other" (p 551).

This division between camps is present very early in social psychology's history. Social psychology as we know it today can be said to date from around the turn of the century. Historians disagree over the dates of the first textbooks and the first experiments, but conveniently for them, the appearance of two textbooks sharing the title Social Psychology in 1908 show that social psychology was beginning to become established at that time, in that there was enough material in existence to use in a textbook. The two books also show that it was not a unified field of study.

Of the two 1908 books, one was written by E.A. Ross, Professor of Sociology in the University of Wisconsin, using a sociological stance to study social behaviour, and the other was written by William McDougall F.R.S., a Reader in Mental Philosophy in the University of Oxford, who used a

psychological theory of instincts to explain social behaviour.

Interestingly from the point of view of this thesis, Wilson and Schafer found that adherents of PSP and SSP accept different "histories" of social psychology, with different "pioneers" and heroes. For psychological social psychologists Kurt Lewin was felt to have made the greatest contribution to the field, followed in order by Festinger, Schachter and Asch, Campbell, and Allport. For the sociological group the ranking was Mead, Goffman, Freud, Homans, and Bales (p 550). Presumably, PSP would see McDougall as a pioneer, whereas SSP would acknowledge Ross as performing that role.

Although these different orientations exist, the most important form of social psychology is normally taken to be what has been characterized as the experimental, psychological type. "The label 'social psychology' is most commonly applied to, and probably most semantically appropriate for, the tradition of social psychology within psychology" (House: p163). Within PSP the main focus is on individual psychological processes (perception, cognition, motivation, learning, attitude formation and change etc.) as they operate in relation to social stimuli and situations. Jones and Gerard's textbook (1967), which House calls "a definitive and sophisticated text of this tradition" (p 163), defines social psychology as the "subdiscipline of psychology that especially involves the scientific study of the behaviour of individuals as a function of social stimuli" (p 1). House believes that methodologically, PSP embodies the

tradition of experimental behavioural research, "which has increasingly characterized all of psychology since the 1920s" (p 163), and he again quotes Jones and Gerard (1967: p 58) as characterizing the conceptual paradigm of such experiments as S-[O]-R: Stimuli (S) are varied and behavioural responses (R) are observed in order to make inferences about the psychological processes and nature of the "organism" (O) or person.

Jones asserts that "social psychology has clearly evolved as a sub-discipline of psychology" (p 48). He notes the existence of the other kinds of social psychology, but from his supporting evidence, eg., the increasing domination of the social psychology textbook market by psychologists (p 48/49), and the proportionate volume of social psychological literature in psychological journals (p 48/49), he suggests that social psychology is a part of psychology and that it therefore uses methods appropriate to psychology, namely experimental ones, and studies the interpersonal process which would be expected if social psychology were a sub-discipline of psychology. Farr agrees, arguing that PSP is largely characterized by the use of the experimental paradigm (p 510).

By studying psychological, or experimental, social psychology, we will be investigating the dominant strand of thought within the discipline. Having restricted our analysis to the experimental tradition, it is worth considering briefly at this stage what phenomena the experimental social psychologist is interested in.



Jones (1985:p 47) accepts G.W. Allport's definition of social psychology as being concerned with the study of actual, imagined, or anticipated person-to-person relationships in a social context as they affect the individuals involved. [7]

The European social psychologists, Tajfel and Fraser [8], clarified aspects of this definition in their 1978 textbook Introduction to Social Psychology. They suggested that "the aim of social psychology is to analyse and understand human behaviour" (p 17), "the principal aims of social psychology being to study, as systematically as possible, various aspects of the interaction between individuals, between and within social groups, and between individuals and social systems, small or large, of which they are a part" (p 22). However, they themselves admit that it is very difficult to give a definition of the "social behaviour" which social psychology is meant to study. They also note that its subject matter, social behaviour, is often shared with other disciplines, such as sociology, anthropology, or political science. Sharing subject matter with other disciplines, and definitional problems, led them to suggest that social psychology's identity has to be established in another way.

They offer instead a characterization of social psychology's identity in terms of the particular aspects of the knot of problems which it studies, even though the

problems and phenomena studied are shared with other disciplines. They suggest that the particular perspective social psychology takes is concerned with studying the relations between the individual and society, especially those which are mediated through face-to-face interaction with others.

The social psychologist is interested in information about how the various social structures, social systems or groups affect an individual's way of viewing the world in which he lives and of acting in it; and about how his 'nature' (ie. his motives, emotions, perceptions and interpretations) will in turn affect his functioning in groups and the relations between groups. (p 25)

Tajfel and Fraser suggest that this perspective is different from those found in sociology and the other disciplines, even though there are considerable overlaps between social psychology's interests and those of other forms of inquiry. This fits in with Allport's analysis. He notes that "in spite of this apparent lack of autonomy, social psychology has its own core of theory and data and its own special viewpoint. Its focus of interest is upon the social nature of the individual person" [9]

Generally then, social psychology is interested in the individual person, and the influences which operate on the individual arising from other people, groups, and social systems. This emphasis distinguishes social psychology from other kinds of inquiry, and it also shows the emphasis on the psychological, rather than the sociological, which agrees with the previous analysis in placing the mainstream of

social psychology firmly in the psychological camp.

We now have a rough outline of the discipline we will be considering. Social psychology, as it is to be understood for the purposes of this analysis, is an experimental sub-discipline of psychology, and is concerned with the individual and the influences on their behaviour arising from their interactions with other people, however this interaction might take place.

#### CHAPTER ONE: FOOTNOTES

[1] Kuhn's work has been very influential, and has generated much interest and research, not all of which found Kuhn's position unproblematic. For example, Lakatos and Musgrave's Criticism and the Growth of Knowledge (1970), consisted of a number of papers dealing with Kuhn's presentation of paradigms, criticising many of his assumptions and conclusions. For the purposes of this thesis it will be assumed that in broad outline, Kuhn's thesis is correct, and that a community of thinkers partly identifies itself, and is identified by others, by the ideas, or paradigms, which it shares and accepts. Therefore the beliefs and assumptions of the community of scholars and researchers who see themselves as social psychologists should be able to be identified by an analysis of their shared paradigm.

[2] E.g., as Elms (1975) reports, and as Secord (1977) argues.

[3] Watson was a leading figure in the professionalization of the history of psychology, and was instrumental in starting the newsletter which evolved to become The Journal of the History of the Behavioural Sciences.

[4] Hewstone, M., Stroebe, W., Codol, J-P., & Stephenson, G.M., eds., Introduction to Social Psychology. Oxford: Basil Blackwell, 1988. The editors of this book are well respected members of the European social psychological community. Hewstone has published extensively on issues of social identity and group processes, Stroebe is Professor of Psychology at the University of Tübingen, Codol is Professor of Social Psychology at the University of Provence, and Stephenson is Professor of Social Psychology at the University of Kent at Canterbury. All of them are regularly cited in the literatures of their respective specialities.

[5] Hewstone et. al. cite the first generation of textbooks by Asch and Newcombe, and the second generation by Jones and

Gerard, Brown, and Secord and Backman, as being significant contributions to the field both in terms of new theoretical ideas and as being unique conceptions of social psychology (Preface, XV-XVI)

The textbooks cited by Hewstone et.al. are:

Asch, S.E., Social Psychology. New York: Prentice-Hall, 1952.

Newcombe, T.M., Social Psychology. New York: Holt, Rinehart and Winston, 1950.

Jones, E.E., & Gerard, H.B., Foundations of Social Psychology. New York: Wiley, 1967.

Brown, R., Social Psychology. New York: Macmillan, 1965.

Secord, P.F., & Backman, C.W., Social Psychology. New York: McGraw-Hill, 1964.

[6] There is also an analytic social psychology, which has developed within the psychoanalytic tradition. Erich Fromm, in books such as The Sane Society (1956), is one practitioner within this broad school.

[7] "With few exceptions, social psychologists regard their discipline as an attempt to understand and explain how the thought, feeling, and behaviour of individuals are influenced by the actual, imagined, or implied presence of others. The term "implied presence" refers to the many activities the individual carries out because of his position (role) in a complex social structure, and because of his membership in a cultural group." (G.W. Allport, Handbook of Social Psychology, 2nd. edn., Vol. 1, 1968, p 3.)

[8] Henri Tajfel was Professor of Social Psychology at Bristol University, and wrote and edited many books, including; The Context of Social Psychology (1972), Human Groups and Social Categories: Studies in Social Psychology (1981), The Social Psychology of Minorities (1978), and The Social Dimension: European Developments in Social Psychology (1984). Colin Fraser is a lecturer in Social Psychology, and Fellow of Churchill College Cambridge.

[9] In Lindzey and Aronson, Handbook of Social Psychology, vol 1, (1968), p 3.

## CHAPTER TWO

Now that we have established an outline of the discipline under scrutiny, the next step is to discuss the work of Kurt Lewin, whose influence on social psychology will become clear, and whose theoretical and metatheoretical assumptions will be shown to be fundamental to PSP. Lewin's work is one point where the pressure of a philosophical criticism of social psychology should be brought to bear.

It makes sense to place the weight of criticism on Lewin's work for two interrelated reasons. Firstly, social psychologists themselves see Lewin as the founding father of their activity, and secondly, because it was Lewin's philosophical and methodological arguments which constituted that foundation. He is regarded by many as a "master of social psychology" [1], and if we pay attention to Kuhn's advice about the textbook tradition, to examine his work as the founder of social psychology and find it lacking should cast a shadow of doubt over the whole enterprise. After all, if the work regarded as seminal is found to be deficient, what price lesser efforts? [2]

As we have seen from the survey by Wilson and Schafer (1978), amongst the psychological social psychologists who had replied to their questionnaire, Kurt Lewin was felt to have had the most impact on their field, and to have been the most important figure in it. This judgement still holds good. For example, Rupert Brown (1988) calls Lewin one of the founders of modern group psychology (the others being Muzafer

Sherif and Henri Tajfel: preface IX). Brown also says that Lewin had a profound effect on the thinking of a whole generation of group dynamics researchers, including Cartwright, Deutsch, Festinger and Rabbie, even though "the details of [his] rather abstract and formal theory need not concern us, especially since the theory itself has attracted little systematic research" (p 28).

Graumann (1988) [3] also emphasizes the importance of Lewin for social psychology, suggesting that Lewin moved into the field of social psychology when he applied his field theory to social groups. Graumann stresses that this "field theory" was more of a general methodology than a tight theory. Lewin's approach focused on the principle of interdependence, "emphasizing the primacy of the whole (situation or field) over the parts, and made use of constructive rather than classificatory methods" (p 14) [4].

Lewin's ideas were formed with the help of two main influences, the philosophy of Ernst Cassirer [5], and the psychologists working at the University of Berlin who became known as the Gestalt school, Wertheimer, Koffka, and Koehler. Although influenced by these men, he was not an orthodox follower of their ideas. He was interested in motivation rather than perception, and in practical applications rather than understanding for its own sake, which distanced him from the main body of Gestalt psychologists. He did however, accept the basic Gestalt principles of holism, organization, and dynamic explanation (see Schellenberg: p 64; and Marrow: pp 14/15). Indeed, he expresses his admiration for

Wertheimer and Koehler in A Dynamic Theory of Personality  
(1935):

I need not emphasize my debts to these outstanding personalities. The fundamental ideas of Gestalt theory are the foundations of all our investigations in the field of the will, of affection, and of the personality. (p 240)

Importantly, Lewin's ideas and methods were promoted and used by his associates and students, and Graumann quotes Marrow (1969), Lewin's biographer, and Festinger (1980), the second most influential social psychologist according to Wilson and Schafer's survey, to suggest that the list of these associates and students is probably the most impressive and influential ever associated with one scholar after Wundt. "Although Lewin died in 1947 it was largely Lewinians like Cartwright, Deutsch, Festinger, French, Kelley, Schachter and Thibaut who shaped social psychology in America after the Second World War, and consequently, in Europe" (Graumann: p 14/15). Schellenberg (1978) lists Lewin's students and associates who went on to become leaders in American social psychology, and suggests that "together these names constitute at least half of America's most prominent social psychologists of the 1950s and 1960s" (p 77) [6].

Schellenberg (1978) makes a very astute observation relating to the influence Lewin's ideas have had. He suggests that those social psychologists who have been trained in psychology departments tend to give most prominent attention to cognitive theories, which deal with the way the individual

subjectively creates the framework within which they act. His observation is this:

And here the traditions of Gestalt psychology and Lewinian field theory are especially strong. It is not that many social psychologists use the terminology of Lewinian field theory any more (that has largely been abandoned) but the underlying assumptions that behaviour is cognitively mediated, subjectively organized, and based especially upon an interpreted social environment - these are the key themes following very much in the Lewinian tradition. (p 127)

Schellenberg's observation suggests a connection between the various discoveries we have already made about social psychology. He suggests that psychological social psychologists are concerned with the individual, using cognitive theories to explain social behaviour, which necessarily focus upon the individual in the social situation. This is a conclusion which we arrived at from our discussion of the orientation of social psychology. He then ties the Lewinian approach into the aims of PSP by way of its underlying assumptions, which he sees as being very much in the Lewinian tradition. Although this statement in itself does not prove that the link between Lewinian theory and PSP exists, it is indicative of the view held by Jones and others that Lewin helped to shape social psychology as it exists today. It will be shown later that the influence Lewin exerted is as great as his disciples maintain, and it will also be shown that the assumptions he made are still in use today.

Schellenberg's observation is astute in another way.



He says that the terminology of Lewinian theory has largely been abandoned, which is easily shown by glancing through the current social psychological literature. Lewin's name however, is not forgotten. It is often brought forth, in passing, to show the ancestry of an idea, or to provide a kind of touchstone when mentioning the founders of social psychology, to show the historical legitimacy of the enterprise. From the way Lewin is treated it looks as though his major contribution to social psychology, his metatheorizing, has been accepted, and is still in use, while his other work has been superseded by the progress social psychology has made in the time since its publication.

The fact that the fashion in social psychology is for regarding papers as past their "sell-by date" if they are more than twenty or thirty years old [7] seems to support this explanation of the neglect of his more concrete theorizing. However, returning to Kuhn's analysis of textbook writing, one of the points raised earlier was that histories are often re-written so as to make a discipline's progress seem continuous, by neglecting inconvenient earlier work. It appears that a similar process may be at work with Lewin's achievements. What is acceptable today is mentioned by psychologists in glowing terms, while what is an embarrassment is hidden by saying that it has been abandoned, though not because it was incorrect, but because the details of the theory are of no concern in the present day, "especially since the theory itself has attracted little systematic research" (R.J. Brown, 1988: p 28). It seems therefore that history has been selectively re-written to

make continuous progress in social psychology plausible.

Further, on the basis of the above suggestion, it seems obvious that the details of Lewin's theory do concern us, because they were an essential part of his system of thought, and that therefore, if one part of the system, the "details", had to be hidden to avoid embarrassment, doubt is cast onto the adequacy of the whole of Lewin's system. If Lewin's system of thought is a cornerstone foundation of social psychology, and if Lewin's system is found to be inadequate, then it seems reasonable to suggest that social psychology's prestige is called into question too. It will be shown, in the body of this thesis, how Lewin's system is inadequate.

The conclusion which seems justified therefore is that although Lewin's name is rarely mentioned these days, except in reference to his status as a founder of experimental social psychology, and as a creative thinker in its early years, his influence is still felt [8]. It will be suggested that Lewin's influence is a result of the argument he used to justify a new approach to understanding social behaviour, an approach which allowed experimental social psychology to overcome difficulties it had experienced previously. The need which was felt within social psychology at the time for such a change, and the manner in which Lewin set about fulfilling this need will be outlined briefly.

Jones (1985) presents a discussion of the major

developments of social psychology over the past five decades, roughly from the 1930s to the 1980s. He notes that in the 1920s social psychology became an area capable of textbook demarcation, but that it was a field lacking in relevant theory or distinctive methodology (p 60). Social psychologists used theories and methods which had been developed in other fields of social science, but found that they were often of little use in dealing with social psychology's interests. Part of the reason for this shortcoming was the ambitious agenda which social psychology set itself.

As we have seen, social psychology textbooks normally define their subject's area of interest in terms of the study of responses people make to social stimuli [9], in terms of the interaction between people and their environment, and also in terms of the way people affect one another. As Deutsch and Krauss (1965) point out, in Theories in Social Psychology, these areas of interest can include situations (or social behaviours) such as a mother tenderly nursing an infant, an angry white man throwing a rock at a black student, two friends conversing, a candidate making a political speech at a street corner, a football game, a man writing at his desk, a teacher instructing students, a patient and his therapist, and so on (p 1).

These situations are extremely varied, both in their contents, and in their meaning for the people involved in them, as well as in many other ways. To study the different individual motives, for example, in all of these cases, would

be an enormous undertaking.

Jones notes a related problem. The social stimuli which are part of social psychology's subject area are often "implicit", meaning that "the individual's behaviour may be conditioned by his or her consideration of how significant others or reference groups might react if they were present" (p 59). That is to say, the stimulus is not actually present, but it still affects what the person does. My teachers might not be present, but I might still take their advice when sitting an exam. Similarly, when dealing with people's responses to these stimuli, social psychologists are interested in more than "overt behaviour", what a person says or does at a given moment. Jones suggests that social psychologists see this behaviour as being transient and determined by so many different factors that it is useless as a basis for psychological understanding. This being the case, psychologists search for underlying dispositions which they believe guide complex "molar behaviour", namely "values, attitudes, and beliefs that persist as influences on the things we say and do in diverse settings" (p 59) [10].

If they agree with this line of thinking, social psychologists find that they have to deal with inferred dispositions influencing responses both to external stimuli as well as implicit or imagined stimuli, inferred, because of course, no-one can see a disposition directly. Having these problems, Jones believes, "could lead to the avoidant reductionism of animal experimentation, to the metaphoric

syntheses of the humanities, or to methodological invention. The advance of the kind of social psychology celebrated in this chapter [PSP] was clearly dependent on the latter" (p 60).

Methodological invention was necessary to give access to the various stimuli affecting the individual. If Jones is correct, this invention was provided by Lewin and his followers, "and various solutions to the problems of stimulus identification became triumphant achievements within the experimental movement in social psychology" (p 60). Lewin recognized social psychology's needs, and in "Field Theory and Experiment in Social Psychology" (1939), he set out to show what these needs were, and how they were to be fulfilled (eg., p 132). The ideas presented in this important paper will be discussed in due course.

Before Lewin's ideas took centre stage in social psychology though, we should remember that it was primarily a non-experimental discipline. The first Handbook Of Social Psychology (Murchison, 1935) consisted, with the exception of one chapter, of essays in comparative psychology. Although there had been studies dealing with the effects the presence of other people had on individuals' performance of tasks (social facilitation), and some other studies, these were conducted with variables which could be easily manipulated, such as the presence or absence of other people. The "psychological" aspects which could be involved in situations were more or less ignored until the contributions made by Sherif in 1936, and Lewin. Even so, these contributions could

have remained merely suggestive of a way of dealing with social behaviour without the support of Lewin's theorizing.

"One can argue however, that experimental demonstrations in social psychology would have been isolated and selective without the metatheoretical underpinning provided by Lewinian field theory" (Jones: p 67), and it is this metatheoretical underpinning that is Lewin's seminal contribution to social psychology.

In American psychology during the 1920s and 1930s the importance of theory as a bridge between concrete experimental findings was emphasized, and in social psychology researchers became more interested in the conceptual generality of their findings than with the representativeness of their samples (Jones: p 67).

Theories were useful to the extent that they predicted superficially different but conceptually similar relationships in a variety of contexts. It was Kurt Lewin who, more than anyone else, stimulated and provided the philosophical rationale for this approach. (p 67)

Obviously, given the agenda we have seen social psychology set itself, a theory that could provide conceptualizations linking the seemingly disparate social activities dealt with would be a huge asset.

Lewin's solution to social psychology's problems was to create a field theory, which he described as "a method: namely a method of analysing causal relations and building

scientific constructs" (1952: p 45). Lewin's work borrowed terminology from force field physics (which is a typical Gestalt move, see Koehler Gestalt Psychology, 1929), and from mathematics. "Borrowing" terminology and methodology from physics and mathematics was a symptom of Lewin's desire to create a more "scientific" psychology and social psychology than had existed before. His use of scientific concepts was a deliberate attempt to help psychology to progress. His main justification for the use of these concepts is found in "Aristotelian and Galilean Modes of Thought" (1931) which will be discussed shortly, but it is worth noting at this point that the analogy he created between psychology and the advanced sciences led him into difficulties. This may be because, as Szasz points out in a different context [11], it can be tempting to mistake a metaphor for a description, and to take an analogy which has been created to illuminate a problem too literally, over-extending its use in a way which is distorting of the subject matter it is applied to, and which leads to confusions in explanation. This idea will be explored in more detail in future chapters, where it will be suggested that the temptation of the analogy between physics and psychology led Lewin to create an "essentialist", "mechanistic" picture of human behaviour which distorts our understanding of people. Lewin's mistake is in employing a single model of explanation, which seems simply inappropriate for many kinds of behaviour.

In Lewin's terminology, organisms "locomoted" through a field of bounded "regions", impelled by "forces" or drawn

by "valences" along power "vectors". Importantly for social psychology, Lewin drew away from conceiving of people as "a bundle of propensities confronting a structured social system. For certain purposes he conceived of a person as a point in psychological space, constrained to move in certain directions by the field of forces operating in that space" (Jones: p 68). A conceptualization of this sort can lead to experimentation, because, as Jones says, viewing people in this way emphasizes the contemporaneous perceptions and related actions they share with others in the same position. "Through experimentation, one hopes that such common action patterns can be determined" (p 68). The other kind of view, of humans as being the product of a long developmental history, creating a bundle of propensities, emphasizes the distinctiveness and uniqueness of their responses to a common environment, and thus requires explanations to be given with reference to these histories, a task which is difficult because seemingly endless.

Lewin's contribution was basically atmospheric according to Jones, but this must surely be an understatement if we take seriously the influence he seemingly has had. Although the details of his theory were not taken much further empirically, and are largely neglected today, it seems safe to assert that the assumptions they were based on have helped to form the foundations of experimental social psychology. Lewin's "mode of conceptualizing" fed easily into experimental interventions, as Jones suggests, and provided a rationale for theory-based experimentation, as well as for the idea of conceptual generality of a relationship across



contexts rather than simple empirical generality across samples.

Schellenberg (p 85) notes that:

Lewin's insistence upon examining behaviour as a field of forces and not as a set of isolated events built social psychology more naturally into general psychology than was possible for any other system. This led to the ready conceptualization of social facts as central ingredients in individual behaviour.

Lewin's metatheoretical work was based around his conviction that psychology needed to emulate the physical sciences in their adoption of the Galilean rather than the Aristotelian conception of science. Adopting this conception of science allowed Lewin to provide a rationale for conceptual generality across contexts, and also for the use of experimentation in a thorough way. We have seen that this contribution has earned him adulation amongst social psychologists, but to decide whether this adulation is deserved we must examine his advocacy of a Galilean stance toward science, and its adequacy for use in psychology and social psychology.

## CHAPTER TWO: FOOTNOTES

[1] The title of a book by James Schellenberg (1978).

[2] In his book Psychology Exposed: Or The Emperor's New Clothes (1988), Paul Kline defends himself against the charge that he deliberately selected weak or absurd topics to discuss, so as to support his thesis. He argues that, on the contrary, he has not "selected" the topics he discusses, but rather that he used those regarded as most important in two textbooks which were themselves considered excellent summaries of the field. In choosing topics which are considered by textbook writers to be significant aspects of

their subjects, he argues both that his criticisms will be more telling, and that if the topics chosen are trivial, then the whole field must be so as well, an observation that would support his thesis as strongly as a relevant criticism. My reasons for choosing where to place my arguments are therefore similar to Kline's.

[3] In Hewstone et.al. Introduction to Social Psychology. Graumann teaches at the University of Heidelberg, and has contributed to many social psychology textbooks, as well as writing and editing others, for example, Changing Conceptions of Crowd Mind and Behaviour (1986), which he edited with Moscovici, one of the foremost European social psychologists.

[4] R.J. Brown (1988) also emphasizes the importance of interdependence for the theory, and notes that the two key ideas which emerged from Lewin's work relating to the understanding of elementary group processes are types of interdependence, namely interdependence of fate and interdependence of task for the group (p 28).

[5] See Lewin's paper, "Cassirer's Philosophy of Science and the Social Sciences" (1949), in which he enthusiastically endorses Cassirer's work, which he seems to see his own as extending.

[6] I will give two examples of Lewin's influence. Marx and Hillix (1979) in Systems and Theories in Psychology go so far as to say that "it is hardly hyperbole to describe American social psychology as a Lewinian development", and Jones and Gerard, in the second generation of text book writers (and one of the most up to date if we count Jones' contribution to the latest edition of the Handbook) acknowledge their indebtedness to the tradition. "Our decision to highlight the experimental approach made it a foregone conclusion that we would emphasize the broad strategic contributions of Kurt Lewin" (preface VI, 1967) It hardly seems worthwhile to list all the social psychologists who admit that Lewin was their hero, and who have been influenced by his ideas.

[7] See for example, Sarason (1978) who notes that there is a feeling in social psychology that the longer ago a study was performed, the less there is to be learnt from it.

[8] This is a point Lewin's biographer, A.J. Marrow, agrees with. He says that "many of his concepts have been so widely accepted that they figure as intrinsic to the science itself and their origin is not remembered. Often enough ideas and techniques Lewin originated are discussed without any reference to him" (IX). Marrow goes on to say: "He has turned social psychology in all its aspects in new directions and given the psychological study of human relations more precise yet more humanely oriented methods. The variety of his concerns and the richness of his contributions joined to serve a single purpose: 'to seek deeper explanations of why people behave as they do and to discover how they may learn to behave better'" (X/XI) (Marrow, 1969).

[9] It has been pointed out that people are influenced in many ways by non-social stimuli too. For example, Harold Searles, in The Non-Human Environment (1960), argues that "The non-human environment, far from being of little or no account to human personality development, constitutes one of the most basically important ingredients of human psychological existence" (p 5/6). It could be held therefore that social psychology has already, by means of the definitions it uses, distorted what it studies, in that it systematically ignores much of what is important to humans. It is my contention that this is only one area in which social psychology distorts its subject matter, thereby making its findings of peripheral interest when dealing with real "concrete" human beings.

[10] Social psychology's interest in dispositions like these should be distinguished from a philosophical interest. For example, Stevenson, in Ethics and Language, discusses the meaning of ethical terms, and tries to characterize the general methods by which ethical judgements can be proved or supported. To this end he suggests that ethical disagreements involve disagreements of a dual nature, in belief and attitudes, about and towards a subject. He argues that the central problem of ethical analysis "is one of showing in detail how beliefs and attitudes are related" (p 11). Although this concern seems very similar to that expressed by Jones, there is a difference in that Stevenson is concerned with the logical and linguistic relationship of attitudes and beliefs to ethical judgements, whereas Jones, and social psychology, sees the person's attitudes and values as part of an explanation of their behaviour, in a way which can be empirically investigated and verified. See for example, Ajzen and Fishbein (1980) Understanding Attitudes and Predicting Social Behaviour. Thus, the apparent shared interest in attitudes and beliefs shown by social psychology and philosophy does not express a common viewpoint or methodology.

[11] In The Myth of Psychotherapy (1978) for example, Szasz comments on a remark Freud made about a patient who had been referred to him by another doctor. Freud "prescribes" a cure for the patient's husband's impotence. Szasz says: "The imagery and vocabulary of treatment are used here in an obviously metaphorical, even humorous, way. But in the nearly one hundred years that have elapsed since that episode, what was said in jest came to be taken in earnest; what had been metaphor was systematically redefined as literal" (p 8). I will suggest that a similar misfortune may have happened to Lewin's conceptualizations about social psychology.

### CHAPTER THREE

Social psychology is concerned, as we have seen, with a wide variety of behaviour, and one of Lewin's main goals was to find a way of providing a comprehensive explanation of behaviour which was flexible enough to cover all eventualities. He felt that social psychology as he found it in the 1930s and 1940s was unable to do this. Both the conception of science it followed and the theoretical assumptions it made about the people whose behaviour it explained were inadequate to do justice to the reality of what Lewin saw as occurring. He had observed, obviously, that individuals act differently in different situations, and that different people do not all necessarily act the same way in the same situation. He saw that the kind of person you are, as well as the situation you are in, will affect what you do, and it is this insight he sought to systematize.

He expresses this wish in "Field Theory and Experiment in Social Psychology" (1939). He says that it is possible to establish evidence that "social influences enter every action of the individual, even actions which seem to have nothing to do with society" (p 131). Thus, to explain even the most private acts, social influences must be taken into account.

Although this insight might be valuable, it is only the beginning of the psychologist's problem in Lewin's eyes. Acknowledging the insight means that other difficulties must be overcome. He suggests that these difficulties include,

amongst others:

- a) The integration of vast areas of very divergent facts and aspects: The development of a scientific language (concepts) which is able to treat cultural, historical, sociological, psychological, and physical facts on a common ground.
- b) The treating of these facts on the basis of their interdependence.
- c) The handling of both historical and systematic problems.
- d) The handling of problems relating to groups as well as to individuals.
- e) The handling of all "sizes" of objects or patterns (social psychology has to include problems of a nation and its situation, as well as of a play group of three children and their momentary struggle).
- f) Problems of "atmosphere" (such as friendliness, pressure, etc.).
- g) Experimental social psychology will have to find a way to bring large-size patterns into a framework small enough for the technical possibilities of experimentation. (p 132)

Discussing the first difficulty he has identified, Lewin observes that the range of subjects, or "problems", that social psychology is interested in is frightening in its scope. Factors which must be taken into consideration include "psychological" problems (eg. intelligence), "physiological" facts (eg. health or size), and also physical facts (where the person lives etc.). These factors, along with values, ideologies, and cultural influences, all seem intuitively to play a part in those aspects of the interaction between individual and society that social psychology is interested in (p 133). Lewin is not frightened by the extent of what must be done in social psychology though, because he has a clear idea of how to deal with the scope of social psychology's interests.

However, we should note that not only is the range of social psychology's problems daunting, in terms of subject matter, but the way in which Lewin proposes to deal with this range is also daunting. He wants to provide an integration of the divergent facts in terms of their interdependence, yet he does not say how this interdependence of facts is to be limited. If cultural and psychological facts are interdependent, it is not clear how we will know which facts are relevant to an explanation. It appears that to explain behaviour we have to refer to cultural, historical, sociological, and physical facts, as well as psychological ones, meaning that to explain an action we have in effect to explain the world as well. As Wittgenstein says of Freud:

Freud's theory of dreams. He wants to say that whatever happens in a dream will be found to be connected with some wish which can be brought to light. But this procedure of free association and so on is queer, because Freud never shows how we know where to stop - where is the right solution.  
(Lectures and Conversations: p 42)

With Lewin the problem is even worse. Everything is held to be interdependent with many other things, and unless a way is found to limit the ever spreading tentacles of interdependence, it looks as though the "right" solution will be one which involves explaining an action in terms of the world as it is at the time of the action, a task which would defeat many people. Interdependence and its role in Lewinian explanation will be investigated further later.

Fundamental to Lewin's thinking was his utter rejection of the scientific conception which he thought

psychology was using at the time. He says: "It is utterly fruitless and merely a negative scientific treatment to put these facts into classificatory pigeonholes, however accurately built and fitted they may be" (p 133). He felt that by merely classifying facts, social psychology did not really produce proper explanations of behaviour, as we will see.

The alternative he suggests is, he thinks, a way of bringing all these different sorts of facts, psychological, physiological, and physical, together in a way that makes it possible to treat them all "on one level" without losing sight of their individual characteristics; how to treat the bodily changes, shift of ideology, and group-belongingness that are found for example, in "the problem of adolescence", within "one realm of scientific language, in a single realm of discourse of concepts" (p 133). As we have seen, this realm is what he calls a "field theory" conception.

We have already seen R.I. Watson's suggestion that psychology operates as a science by the use of guidelines or prescriptions. Watson remarks (in a footnote to page 100) that he felt that Lewin was "groping" towards advocating something akin to a prescriptive approach to psychology in his discussion of the conflict between the Aristotelian and the Galilean modes of thought as regards science [1]. This discussion provides the background of the argument in the above passage where he denigrates the use of "pigeonholes". In rejecting the use of classificatory pigeonholes Lewin was



rejecting the Aristotelian conception of science in favour of what he describes as the Galilean conception, arguing that psychology should use a Galilean conception of science.

Lewin's discussion dealt with the possibility of finding general laws in psychology through the use of experiments, and addressed issues relating to the possibility of applying such quantitative and objective methods to the motivations for our behaviour. This discussion forms the very foundations of his theorizing in psychology and social psychology. Therefore his advocacy of Galilean conceptions is of great importance in assessing the worth of his experimental social psychology, and therefore of social psychology generally [2]. Thus, it is necessary to show how Lewin argues for a change in prescription, or conception, in psychology, to show how his arguments are related to the theories he proposes, and to show how these theories were linked necessarily to experimentation. An example will then be given to show that the conceptions he championed are still at work today, even though, as has already been suggested, Lewin's influence is no longer explicitly acknowledged.

His discussion was centred around what he considered to be the fundamental change that had taken place in scientific thinking and conceptualizing that occurred when Galileo broke away from Aristotle's ideas on how to conduct a scientific enquiry. There are two main changes that Lewin argues Galileo advocated, and they are related, a change in one necessitating a change in the other. According to Lewin, Galileo argued for a change in the way we understand and



obtain laws, and consequently, a change in the way in which we view explanations [3].

Lewin's discussion was structured so as to stress the rationality and usefulness of the analogous change he was calling for in psychology. He considered Aristotelian conceptions of science and physics in the first sections, and then showed how Galileo's ideas about physics had replaced the older conceptions, with the implicit moral being that a very great improvement had been made by allowing such a change to occur. He then discussed the concepts psychology was using at the time, finding them to be thoroughly Aristotelian in content, a fact which he suggests caused the struggles and theoretical difficulties psychology found itself in. These two discussions were just preliminaries, however, to his "theoretical treatment of the dynamic problems" in psychology (p 29). The "dynamic problem" in psychology is fundamental for Lewin, and is fundamental to understanding both his use of "Galilean" concepts, and the methods he advocates for psychology, and eventually social psychology, as will be shown.

The Aristotelian view of science, which was, in his opinion, embedded in psychology, had defects for Lewin which were to be found at base level in the general substantial and philosophical presuppositions it made:

Too meagre scientific courage in the question of the lawfulness of the psychical, too slight demands upon the validity of psychological laws, and the tendency, which goes hand in hand with this leaning

toward mere regularity, to specifically historic-geographic concepts. (p 41)

These presuppositions will be examined briefly, along with Galileo's, and Lewin's, dissatisfaction with them.

A major concern of Lewin's was with the lack of belief in the lawfulness of the psychical at the time he was writing. Examining the foundations of this belief, and drawing an analogy from the natural sciences, Lewin suggested that in Aristotelian physics not all physical processes are seen as being lawful. For Aristotle and his contemporaries, the Universe contained "as much that was chaotic as that was lawful" (Lewin: p 5). Just as for other young sciences, as physics was in Aristotle's day [4], Lewin argues that it was an open question whether physical processes were subject to laws, and if so, to what extent. In Aristotelian physics, the way this question was settled was to decide whether a given event was regular. Only events which had a regularity, such as the motions of the stars, were assumed to be subject to laws. Thus Lewin notes:

The ambition of science to understand the complex, chaotic, and unintelligible world, its faith in the ultimate decipherability of this world, were limited to such events as were certified by repetition in the course of history to possess a certain persistence and stability. (p 6)

For Aristotle, things or events were intelligible only if they occurred without exception, and were therefore lawful. This has a further consequence relating to Aristotle's classificatory scheme. If things are lawful in

that they appear without exception, they may be placed in a "class" of phenomena by virtue of the property which they possess without exception. The membership of an object or event in a particular class was critically important, because the class defined the essence or essential nature of the object and thus determined its behaviour. Because light objects "relatively frequently go up" (Lewin: p 7) and flames do too, they can be assigned to the same class of objects, namely ones that have a tendency upward.

This scheme of classification is also related to the preference Aristotle shows for historic-geographic concepts. "So long as lawfulness remained limited to such principles as occurred repeatedly in the same way...the concept of lawfulness had a fundamentally historic, a temporally particular significance" (Lewin: p 8/9). Stable events in the historically given world were important, unstable ones were not [5].

Lewin gives two examples of the Aristotelian conception at work in psychology. In child psychology, whatever is common to children of a given age is set up as the fundamental character of that age.

The fact that three-year-old children are quite often negative is considered evidence that negativism is inherent in the nature of three-year-olds, and the concept of a negativistic age or stage is then regarded as an explanation...for the appearance of negativism in a given particular case! (p 15)

Similarly, Lewin suggests, the concept of drive or instincts is nothing more than an abstract selection of features common to a group of acts that are of relatively frequent occurrence. "This abstraction is set up as the essential reality of the behaviour and is then in turn used to explain the frequent occurrence of the instinctive behaviour" (p 16). He suggests that most of the explanations given of expression, character, temperament, ability, talent (and similar concepts employed by intelligence testers) are in the same state, "reduced to explanation in terms of Aristotelian essences, a sort of explanation which has long been attacked as faculty psychology and as circular explanation, but for which no other way of thinking has been substituted" (p 16).

As we have seen, Aristotle's criterion of scientific lawfulness was the predictable and orderly repetition of a phenomenon, this repetition serving to explain the phenomenon under observation. This being the case, it is necessary for a scientific investigation to study as many similar cases as possible to establish laws of general validity. As Lewin says, "the Aristotelian concepts show...an immediate reference to the historically given reality and to the actual course of events" (p 12) in that they must refer to events which have repeatedly happened.

In Galileo's science a much different stance is taken. It is not important whether a particular process occurred "only once or was very frequently or invariably repeated in the course of history" (p 12). Lewin puts the

Galilean case as follows:

The law of falling bodies, for example, does not assert that bodies very frequently fall downward. It does not assert that the event to which the formula  $s=1/2gt^2$  applies, the "free and unimpeded" fall of a body, occurs regularly or even frequently in the actual history of the world. Whether the event described by the law occurs rarely or often has nothing to do with the law. Indeed, in a certain sense, the law refers only to cases that are never realized, or only approximately realized, in the actual course of events. Only in experiment, that is, under artificially constructed conditions, do cases occur which approximate the event with which the law is concerned. (p 12)

For Galileo, all events were lawful, none were chance, or random (or chaotic). The motion of a falling stone is subject to a law, even if it is not clear from appearances what this law is.

Lewin characterized part of the transition that Galileo had made from Aristotle's rigidly defined, essentially descriptive classes, to more modern concepts, as being the differentiation of "phenotype" from "genotype" (p 11), terms which Lewin "borrowed" from contemporary biology. Phenotypes are simple appearances, whereas genotypes are concerned with underlying, determining tendencies. Lewin gives as an example of the change in classification produced by this differentiation, the classification of events such as the orbits of the planets, the free falling of a stone, and the movement of a body on an inclined plane. If these were classified according to their phenotypes they would be in different classes, because they appear so different from each other, whereas when classified according to genotypes, they

prove to be expressions of the same law of gravitation. "For Aristotle the immediate perceptible appearance...was hardly distinguished from the properties that determine the objects' dynamic relations" (p 11). For Galileo, and for Lewin, what was more important was now the genotype, and not the immediate appearance.

These are basically the differences between Aristotelian and Galilean science as Lewin described them. He then turned his attention to a discussion of the concepts used by psychology at the time, arguing that they were still in the main part Aristotelian. His evidence for this assertion was that laws in psychology were formulated on the basis of a calculation of the average in commonly occurring cases, while individual cases were seen as holding no scientific interest at all, and as being beyond the concern of the scientific realm. Explanations were made in terms of essences, as we have seen Lewin exhibit, these essences being part of the abstractly defined class into which events were placed. However, although psychology's practices were in the main part Aristotelian according to Lewin's point of view, he nevertheless found evidence to suggest that a development was occurring which seemed "clearly and irresistibly to be pushing on to modifications that may ultimately mean nothing less than a transformation from Aristotelian to Galilean concept formation" (p 22).

He supports this argument by pointing out that part of the Aristotelian classificatory scheme was to form pairs

of opposites. Such pairs could be cold and warm, or dry and moist for example. Lewin (p 4) notes that in modern physics such dichotomous classifications have been entirely replaced by continuous gradations. Similarly in psychology, he detects a change in which the grouping of events and objects into paired opposites "is being replaced by groupings with the aid of serial concepts which permit of continuous variation" (p 22). So between the two opposites some kind of continuum exists, which consists of "transition stages" located between the two extremes.

Lewin suggests that this particular change had advanced furthest in sensory psychology, especially psychological optics and acoustics, and also to an extent in work on feeling. He also notes that Freud's work had contributed to the abolition of the boundaries between the normal and the pathological, which had been seen as dichotomous classifications, and that in child psychology and animal psychology there was no longer the absolute need to choose between two alternatives, regarding, for example, the child as a little adult and the animal as an underdeveloped inferior human, "or trying to develop an unbridgeable gap between the child and adult, animal and man" (p 22/23).

Lewin provides these examples to attempt to show that psychology was already moving in the direction he feels is right for it. What is more important for our purposes is the next piece of evidence he gives to support his thesis, the supposed fact that "it is coming to be realized that every psychological law must hold without exception" (p 23)

in the same way that laws in Galilean physics cover the physical world. To use the gravity example again; "The same law governs the course of the stars, the falling of stones, and the flight of birds" (p 10). He calls the idea of a single law embracing many different sorts of events "homogenization" (p 10).

Having noticed this process of homogenization apparently operating in the same manner in which it found expression in Galilean physics, he makes a crucial assumption, the importance of which cannot be over-estimated for our discussion. He notes that particular laws in psychology have shown a capacity for fruitful application to fields that were once qualitatively completely separated, and given this evidence he assumes that "the thesis of the general validity of psychological laws has very recently become so much more concrete" (p 24), that is to say, that there is evidence to suppose that the thesis could be true. If the thesis is true then it should be safe to say that particular psychological laws apply generally, in the same way that the law of gravitation applies generally in physics. Lewin makes a further, related, assumption, that psychology is best thought of in dynamic terms, as physics was at the time.

To support his conclusions, Lewin says that:

The investigation of the laws of structure - particularly the experimental investigation of wholes - has shown that the same laws hold not only within the different fields of psychological optics



but also in audition and in sensory psychology as well....Further, the laws of optical figures and of intellectual insight have turned out to be closely related. Important and similar laws have been discovered in the experimental investigation of behavioural wholes, of will processes, and of psychological needs. In the fields of memory and expression, psychological development appears to be analogous. (p 24)

Lewin gives as references for this section (in a footnote) works by Wertheimer, Koehler, Koffka, and himself, showing that these laws have all been discovered by Gestalt psychologists [6]. As has already been mentioned, Lewin's work was greatly influenced by the Gestalt psychologists, even though his interests and methods diverged from the mainstream of Gestalt thought in some ways. It is clear, from the references he makes to the Gestalt psychologists here, that his acceptance of their ideas was an important part of his thinking, so it is worth noting that many Gestalt explanations were couched in "dynamic" terms, in terms of forces of one kind or another producing the phenomenon under scrutiny.

In Principles of Topological Psychology, Lewin speaks of the spatial structure of the life space depending on psychological dynamics. "These are such concepts as cause of change, tendency, resistance, solidity, equilibrium, force, tension, etc." (p 63). Similarly, Koehler, in Gestalt Psychology (1929), spoke of dynamic forces operating within the nervous system to produce the forms of sensory experience which we have phenomenological access to. Koehler's dynamic explanations were a reaction against the atomistic type of associationist psychology which had been popular, and thus it

was perhaps natural for Lewin to see an analogy between this reaction in psychology, and the changes produced by Galileo in physics when he used dynamic concepts. Part of the analogy's message of course, is that progress was made by making such a change [7].

It is worth pointing out though that Lewin never proves that the Gestalt principles which he advocates really do cover the whole range of psychological interest. In what could be said to amount to an intellectual sleight of hand, he suggests that dynamic, Gestalt type explanations and laws hold in some parts of psychology, that other parts look as though they will be explicable by similar laws, that physics did much better for itself when an analogous thing happened to it, and that therefore it is reasonable to suppose that since Aristotelian ideas of science in psychology are about to be discarded, they really ought to be if psychology is to progress. It will be a major theme in this criticism of Lewin's work, and therefore of the basis of social psychology, that this "dynamic" assumption is more of a convenient methodological hypothesis than an accurate description of social interaction etc., and that this hypothesis is inadequate in a number of ways as a means of characterizing and explaining human behaviour.

Having set up his discussion of Aristotelian and Galilean concepts in physics and then in psychology, Lewin turns his attention to his fundamental interest, dynamics.

As we have already noted, Aristotelian science had difficulty in accounting for dynamic processes. This was mainly due, Lewin thought, to Aristotle's idea that the direction and force of dynamic movements could be accounted for by an explanation in terms of the nature of the object concerned (p 28). Thus, "the tendency of light bodies to go up resided in the bodies themselves; the downward tendency of heavy objects was seated in those objects" (p 29). Lewin was impressed by modern physics' replacement for Aristotle's notions in this area, and suggests that physics employed the notion that "the existence of a physical vector always depends upon the mutual relations of several physical facts, especially upon the relation of the object to its environment" (p 28).

What Lewin means when he refers to a "physical vector" is the amount of change that has been produced by the interaction of the physical facts. If an object has been moved, the physical vector, that is to say, the distance, direction and so on that it has traversed, depends upon the forces that were acting on it. Thus the distance it moved can depend upon how heavy it is, as well as factors in its "environment", such as how hard it was being pushed, whether it was on a slope, the kind of surface it was moving over, etc. Thus, the physical vector depends upon the mutual relations of the physical factors acting on it.

If we relate this conception to psychology, as Lewin does, we find that explanations in terms of the essential nature of the object will no longer suffice. Light objects

may well often go upwards, but whether an object does on a particular occasion will depend upon the other factors in its environment. Likewise, three-year old children may be inherently negative, but their actual behaviour on a particular occasion will depend on what is happening around them, whether people are being nice or nasty, and so on.

By using Galilean conceptions it appears that Lewin can explain the "insight" mentioned earlier, that people's behaviour depends upon the individual and the situation they find themselves in, in terms of the advocacy of a scientific conception which stresses the interconnectedness of all relevant factors. We can also see how bizarre behaviour, and singular events, can be explained more easily using the Galilean system. On the Aristotelian view, if someone does something very "out of character", it is, by definition, very hard to explain in terms of their regular behaviours or character. For the Galilean, and Lewinian, conception on the other hand, it is easier, because, as Lana for example suggests (1969: p 84), to understand why a person suddenly breaks down in a fit of hysterical weeping in the middle of a social evening, one must know what the person's perceptions of the host, the party, and the particular moment, were. The weeping fit may never have happened before, and may never happen again, but knowing the relationship between the individual and their environment allows an explanation of that particular action on that particular occasion to be made. Being a "one-off", it would be hard to explain it coherently in terms of someone's character, relatively

independent of the situation, but when the situation is included it is possible to explain it without too much trouble.

Accepting the importance of the "whole situation" has a consequence for the formulation of laws of dynamics in physics. Lewin writes that since the dynamics of a process depend not only the object, but also "primarily, upon the situation, it would be nonsensical to try to obtain general laws of processes by excluding the influence of the situation as far as possible" (p 30), this method of exclusion being favoured by Aristotle. What is important is not to abstract from the situation, but to look for those situations in which "the determinative factors of the total dynamic structure are most clearly, distinctively, and purely to be discerned" (p 31).

It is this argument that is the foundation of the use of experiment in physics. Lewin refers to Galileo's investigations of the laws of falling bodies to show the principle in use. Galileo did not investigate the falling bodies themselves, but was more interested in the "process" of movement on an inclined plane, showing, Lewin asserts, that the dynamics of the event were no longer conceptually related to the isolated object as such, but were seen to be dependent upon the whole situation in which the event took place (p 29).

By varying certain factors, the slope of the plane for example, relationships become plain which were hidden

before. "The dependence of the essential features of the event (for example, its velocity) upon the essential properties of the situation (the slope of the plane) becomes the conceptual and methodological centre of importance" (p 29).

Taking the situation into account allows relationships such as  $s=1/2gt^2$  to be formulated (the relationship between the distance covered and the time elapsed in unimpeded free fall), the formulation being obtained by manipulating the situation and observing the consequences. The fact that such a relationship exists is assumed; all that needs to be done is to find it, and the same assumption, that "law and individual are no longer antithesis" (p 35) means that genotypes can be discovered through the use of "rare and transitory events, such as most physical experiments are" (p 35).

Having established the superiority of Galilean science over Aristotelian, especially in dynamics, both in respect of allowing explanations of individual events and also in terms of discovering relationships between factors which can lead to the formulation of laws, and thus having established the importance of a form of experimentation leading to the formulation of laws, Lewin attempts to show, as we have seen, that dynamic theories are appropriate to explaining various psychological phenomena, and that therefore Aristotelian concepts, which are inadequate for explaining dynamic processes, are outmoded and unwanted.

Thus in the psychological fields most fundamental to the behaviour of living things the transition seems inevitable to a Galilean view of dynamics, which derives all its vectors not from single isolated objects, but from the mutual relations of the factors in the concrete whole situation, that is, essentially, from the momentary condition of the individual and the structure of the psychological situation. The dynamics of the processes is always to be derived from the relationship of the concrete individual to the concrete situation, and, so far as internal forces are concerned, from the mutual relations of the various functional systems that make up the individual. (p 41)

Lewin suggests that to accomplish this task a workable representation is needed which will show the concrete psychological situation and the concrete structure of the psychological person "and its internal dynamic facts" (p 41). He hints that this representation will be achieved with the help of topology, a young branch of mathematics.

Lewin's preference for "Galilean" type conceptions in psychology is clear. We now need to see the way in which he used these Galilean conceptions. Rather than criticise his argument for a conceptual transition immediately, let us turn to the way in which he used these conceptions in practical terms, and then to an analysis of the adequacy of his overall scheme for the task he set himself. In this analysis it will be suggested that Lewin sets up a "mechanical" account of behaviour which distorts and confuses what we ordinarily accept as facts about human behaviour, and that this mechanical account is flawed in several ways. To begin this analysis the conceptual apparatus he uses in his social psychology, which is clearly reliant on his Galilean, dynamic



conception of science, will be briefly discussed, to show how the theoretical proposals he used in his psychology were related to the metatheoretical foundation he has built by championing Galilean conceptions.

### CHAPTER THREE: FOOTNOTES

[1] This groping is seen most clearly in Lewin's 1931 paper, "The Conflict Between Aristotelian and Galilean Modes of Thought in Contemporary Psychology".

[2] We must remember that in 1931 Lewin's interest was in psychology as a whole, and that therefore his criticisms and suggestions are aimed towards the wider discipline. Although this is the case, he used the arguments developed here when he addressed himself to the problems of social psychology, as will become clear, so examining these arguments is of great importance in assessing the adequacy of Lewin's contribution to social psychology.

[3] The accuracy of what Lewin said about Aristotle and Galileo will not be discussed in detail here. What will be analysed are his opinions about them, and the consequences of these opinions.

[4] This could be a reference to Koehler's chapter, "Psychology as a Young Science" in his Gestalt Psychology (1929). Koehler mentions the Aristotelian-Galilean transition (pp 107/8/9) but does not elaborate upon it to the extent that Lewin does.

[5] Lewin seems to have understood the main arguments of Aristotle's conceptions fairly well. Crombie (1959), an eminent historian of science, characterized Aristotelian scientific explanation and investigation as a two stage, inductive and deductive, process (p 82). Observations are included in a generalization by the use of induction, the generalization leading eventually to the "universal form". These forms are the intelligible and real identity persisting through and causing the changes observed, and defining these forms was necessary because all effects were considered to be attributes of some substance, and the cause of an effect was shown when the effect could be predicted as an attribute of a defined substance. "It was essential to Aristotle's conception of scientific demonstration to reduce all science to subject-predicate propositions" (p 82/83). Crombie notes though, that this conception was to prove inconvenient when dealing with scientific problems that can only be properly expressed in terms of numerical relations, such as rates of change. This fact will prove important in Lewin's work.

[6] Wertheimer, M., "Untersuchungen zur Lehre von der Gestalt, II". Psychologische Forschung, 4, 1923, pp 301-350.  
Koehler, W., Gestalt Psychology. New York: Liveright,



1929.

Koffka, K., The Growth of the Mind: An Introduction to Child Psychology. Trans. by R.M. Ogden, London: Kegan Paul, 1924.

Lewin, K., Vorsatz, Wille, und Bedürfnis, mit Vorbemerkungen über die psychischen Kräfte und Energien und die Struktur der Seele. Berlin: Springer, 1926.

Translations of some of these obscure works can be found in A Source Book of Gestalt Psychology, W.D. Ellis, London: Routledge and Kegan Paul, 1938.

[7] In Principles Lewin also quotes Koehler as having shown that the same concept of "dynamic whole" or "gestalt" can be used in physics and in psychobiology; and also that the fundamental Gestalt laws are equally valid for both sciences (p 64). However, this is again only an assertion; no real evidence is given to support it.

Boring (1950), in his History of Experimental Psychology, makes an interesting comment which relates to Lewin's use of dynamic concepts. In a discussion of dynamic psychology, he notes that the word "dynamic" carries with it conceptions of both "force" and "activity":

Even in physics the two are related, for force is defined in terms of the rate of change of movement which it will induce. Pressure is tension, and tension released is action. (p 701)

Boring sees a close connection between the use of the concept in the two contexts. He also helps to place Lewin's work in its contemporary milieu. He points out that "dynamic psychology is a field, the psychology of motivation" (p 692), and as such it is an area of interest which can be approached from a variety of different directions. Lewin's interest in motivation places him in proximity to hedonists such as Hobbes, Locke, Bentham, James and John Stuart Mill, "activity" psychologists such as Leibnitz, Herbart, Brentano, and James Ward, and psychopathologists like Mesmer, and thinkers in the French tradition such as Liebeault, Charcot, Bernheim, and Janet (p 693). Although being a dynamic psychologist by interest, there was no necessity for Lewin to use "dynamic" concepts. The fact that he did so is probably, as we have noted, due to his Gestalt background and his beliefs about science, but it seems likely that these factors were enhanced by the circumstances in which he found himself. Boring reports that by 1917 Lewin was already prepared to make a Gestalt psychology of motivation his main concern:

That meant then a scientific adaptation of the only thorough-going psychology of motivation extant, the Freudian system. (Boring: p 723)

Lewin says in Principles that the only approach to the deep problems of motivation was the "brilliant work of Freud" (p 3). He goes on to say, though, that methodologically, the psychoanalytic attempt to base general laws on case studies and therapeutic work alone seems unsound to most scientists. There is no reason to be pessimistic though, because:

Within recent years a great number of studies have shown that in spite of the general skepticism an experimental attack on fundamental problems...including problems of Freudian psychology, is quite possible. (p 5)

Freud, of course, used dynamic concepts in his psychology, so Lewin must have found their use both natural and methodologically sound.

## CHAPTER FOUR

In the 1939 paper "Field Theory and Experiment in Social Psychology" which we have referred to before, Lewin shows off his "field-theoretical approach" (p 134) by way of a discussion of "the problem of adolescence and the definition of a social group" (p 134). In discussing these problems he hopes to show some of the major aspects of the field theoretical approach as they are applicable to social psychology. Given Lewin's intention, this seems to be an excellent paper to use to try to find out just what Lewin's approach means in a practical setting.

Lewin chose to analyse adolescence in this paper because, he says, the changes in behaviour associated with this period of life can seem to be associated with biology (hormones and bodily growth) and thus can appear to be explicable in a way which emphasizes only the biological changes taking place. Lewin concentrates instead on the way in which bodily and social factors work together and against each other, "integrating the concrete behaviour of the adolescent" as he puts it (p 135). This concentration is part of a response to the contemporary argument over the status of adolescence as a purely biological, or a purely social effect. The function of his example is to show that the conceptual apparatus he champions is better able to deal with the facts of adolescence than both of these alternatives explanations separately, since it is supposedly able to show and explain the integration of social and biological influences, and the consequences of this integration.

Lewin's use of the Galilean approach is obvious here. Individual characteristics, physiological changes, and social influences are all seen as factors in adolescent behaviour. The assumption that these factors interact, entails that it is more profitable to choose a case for analysis "not so much according to the frequency of its occurrence as according to the amount of insight it offers into a constellation which is typical at least for a part of the setting in question" (p 135).

Therefore, Lewin does not attempt to find those adolescent behaviours which are found universally, but instead tries to find a principle which underlies the behaviour, in the same way as the law of gravitation underlies and explains falling objects. The "architecture" of the interaction is important, not how this architecture is manifested on a particular occasion. Counting instances of a behaviour will not give insights into the structure of the interaction causing the behaviour.

Lewin would probably agree that there need not be even one case which approximates to the formulation he sets up to provide an insight into adolescent behaviour. What is important is providing this insight into the genotypical configuration of factors at work in producing such behaviour. This stance appears to be justified from Lewin's point of view because it is the same stance that Galileo took in his famous experiments on unimpeded free fall. How such an insight is to be gained though is something which Lewin does

not elaborate on in any detail. He always stresses the need to maintain a flexible approach to the data under investigation, so that he apparently has no hard and fast rules. It is instructive to see his method of gaining access to genotypical processes in a concrete example therefore.

Lewin characterizes his approach thus:

In regard to the problems of adolescence, it might be helpful to refer first to cases which show the so-called "typical" difficulties of adolescent behaviour. A field-theoretical analysis of such a situation should give some hints as to what conditions would increase or decrease these symptoms. (p 135)

He suggests that in so-called typical cases of adolescent behaviour a period of transition occurs, and he implies that without this transition there would be no "adolescence", making such a transition virtually a definition of adolescence. He then goes on to attempt to characterize the nature of the transition that occurs at adolescence in general or "typical" terms. His characterization includes factors such as these below. For example:

a) Adolescence can be viewed as a change in group-belongingness, from belonging to the group of children to that of adults. Such a change in group-belongingness is a "social locomotion", "that is, it changes the position of the person concerned" (p 137).

b) This change is a shift to a more or less unknown

position, where it is not clear where actions will lead and how one has to approach a certain goal. This lack of clarity "is one of the major reasons for the typical 'uncertainty of behaviour' to be found in unknown surroundings" (p 138).

c) "Psychologically one's own body can be treated in some respects in the same way as one's environment" (p 139). During adolescence, changes occur in one's body which can make "this part of the life space, which is so close and vital to the individual, strange and unknown" (p 139). The strangeness can lead to a fundamental doubt about the world, leading to increased uncertainty of behaviour and conflicts, or, alternatively, to aggressiveness.

d) Lewin notes that some adolescents flock to extreme political parties. To explain this observation, he suggests that in the transition from region A to region B (child to adult), the adolescent can be in a kind of halfway position, belonging to neither region fully. This is not a stable position, and so makes the adolescent more malleable. Lewin expresses this suggestion thus:

The psychological environment has to be regarded functionally as part of one interdependent field, the life space, the other part of which is the person. This fundamental fact is the keynote of the field theoretical approach. In psychology it has become, in various forms, more and more recognized and can be expressed simply by the formula: Behaviour = Function of person and environment = function of life space. ( $B = F(P, E) = F(L \text{ Sp})$ ). The instability of the psychological environment leads, in some respects, therefore, to greater instability of the person. (p 140)

In this characterization of adolescence Lewin has introduced some new technical terms such as "region", "social locomotion", and "life space". To understand Lewin's conception of adolescence, and also to understand how his approach to social psychology as a whole was translated into working concepts, we will digress for a moment, and discuss these and the other technical terms he uses as part of his field theory, and the functions they play in the field-theoretical approach.

As we have seen from his discussion of Galilean concepts, finding the genotypes concerning and underlying behaviour involves representing the dynamics of the situation. Lewin attempted this by representing the individual and their environment pictorially, using topology, which is a non-quantitative mathematics of spatial relations. An egg-like oval (or "Jordan Curve", an irregular, closed, curving line) was used to identify the psychological field, or "life space". Within the oval, a number of "regions" would be marked, which are conceptualized as representing various objects, events, or goals (literal or figurative) of current importance to the individual. One of these regions would ordinarily be identified as the present state or location of the person ("P"), and other regions would be marked with plus or minus signs to characterize their tendencies to attract or repel the person, these tendencies being termed valences [1].

The total area within the oval is termed the life space (L Sp), the space outside the oval representing the

non-psychological aspects of the universe. The area within the oval, but not including the region P, is the psychological space.

The life space is considered to contain the totality of possible facts that are capable of determining the behaviour of the individual. It includes everything that has to be known in order to understand the concrete behaviour of the individual in a given psychological environment at a given time. By representing the key features of the life space as factors contributing to behaviour, Lewin felt that he was able to conceptually explain, and represent, what happened to the individual as the product of a "field" of forces, and not just as being related to particular characteristics of the individual.

For example, in his discussion of adolescence, Lewin produces two diagrams which compare the spaces of free movement for children and adults [2]. The child's diagram has a number of regions which represent activities they are able to do, all other activities being inaccessible to them: for instance, they are able to perhaps belong to a boys' club, but not to drive a car. The adults' diagram has the same regions, but they are allocated differently. The adult can drive a car, but not join boys' clubs, so although the range of activities available to the adult is larger, it does not include everything that was previously available to the child. The diagram for the adolescent [3] shows the space of free movement as it might appear to an adolescent. Lewin



suggests that in the adolescent's indeterminate, transitory position, some regions in their life space may only be vaguely defined, and the adolescent may be unsure of them, leading to some of the "problem" behaviours which appear to be characteristic of adolescence.

In "Behaviour as a Function of the Total Situation" Lewin gives an analogy to show both the aims and advantages of using topology and his technical terms [4]. A novelist who is interested in telling the story of an individual might give the reader information about the individual's parents, siblings, character, intelligence, occupation, friends, and status. These data are given in their specific interrelation, as part of a total situation which goes towards explaining the individual's behaviour. Lewin believes that psychology must fulfil the same task, but with scientific means and not poetic ones, and that these means should be analytical, in that they must specifically distinguish the different facts which influence behaviour, and not just vaguely suggest possible influences.

In science, these data have also to be represented in their particular setting within the specific situation. A totality of co-existing facts which are conceived of as mutually interdependent is called a field. Psychology has to view the life space, including the person and his environment, as one field. (Lewin: p 240) [5]

The analogy Lewin uses, describing the life space as a "field", is very important, as we will see shortly. By describing the life space as a field, Lewin is suggesting that since all the influences on behaviour are represented,

the description of the motivations for behaviour is also complete, and therefore that explanations which are derived from this description will be adequate for any kind of question which could be raised about that behaviour. This completeness of explanation certainly seems attractive, but as we will see, it is in fact a failing of Lewin's work, which causes distortions and confusions that render social psychology's claims baseless. By concentrating on this single model of explanation, too much that is valuable is left out of an understanding of human behaviour.

Returning to Lewin's discussion of adolescents, we find that he believes that their position is not highly defined, or differentiated, and thus they can move from one region in their life space to another very easily, or in his terms, they can locomote between regions with little difficulty. Adults, as the diagram showing their space of free movement indicates, whose position in their life space is established and stable, with a well defined set of beliefs etc., find the changes in ideology which the adolescent makes easily (point "d" above) very difficult, because of the number of different regions which they must enter and cross to achieve this new position. The adolescent only has to cross a few. This means that ideological shifts are relatively much easier for the adolescent. The difference between adolescents and adults with respect to ideological change is thus explained in terms of Lewin's topological psychology, as resulting from a difference in the structures of the regions of the life spaces for the different groups.

Lewin goes on to outline other characteristics of the adolescent, such as moving into unknown regions, changes in the time dimension of the life space (having goals years rather than days ahead), and being in the "marginal man" position (pp 140-143). He sums up his discussion of the adolescent in the following manner:

a) The basic fact concerning the general situation of the adolescent can be represented as the position of a person during locomotion from one region to another. This includes (1) the widening of the life space (geographically, socially, and in time perspective), and (2) the cognitively unstructured character of the new situation.

b) Somewhat more specifically the adolescent has a social position "between" the adult and the child, similar to a marginal member of an underprivileged minority group.

c) There are still more specific factors involved in adolescence, such as the new experiences within one's body, which can be represented as the baffling change of a central region of the established life space.

From this representation one can derive conceptually:

I. The adolescent's shyness, sensitivity, and aggressiveness, owing to unclearness and instability of ground (follows from a, b, and c).

II A more or less permanent conflict between the various attitudes, values, ideologies, and styles of living (follows from b).

III Emotional tension resulting from these conflicts (follows from a, b, and c).

IV Readiness to take extreme attitudes and actions and to shift his position radically (follows from a, b, and c).

V The "adolescent behaviour" should appear only if the structure and dynamics of the field are such as represented by a, b, and c. The degree and particular type of behaviour should depend upon the degree of realization of this structure and upon the strength of the conflicting forces. Above all, the degrees of difference and separation between adults and children which is characteristic for a particular culture is important; also, the extent to which the particular adolescent finds himself in the position of a marginal man. According to field theory, actual behaviour depends upon every part of the field. It follows that the degree of instability of the adolescent should be greatly influenced also by such factors as general stability or instability of the particular individual. (p 144/145)

This is a good moment to summarize and discuss briefly what Lewin has actually achieved in his discussion of adolescence. There are two main criticisms that can be made at this juncture. Firstly we should note that his analysis and discussion has nothing to do with experimentation as such, so far. He takes various observations and phrases them in accordance with his theory of life space, regions, etc. For example, to note that some people's behaviour changes when they are in their mid-teenage years is just a simple observation. To suggest that this change is based upon the uncertainty felt when entering unknown regions of their life space may also look like an observation, but it is not. It is an explanatory hypothesis based upon a particular theory. That the explanations seem to have an intuitive plausibility is no guide to their truth. Almost anything can be made to seem plausible as an explanation of an undeniable set of observations [6]. Social psychologists today would claim that their explanations are sounder than any alternatives because they have empirical or experimental backing, but at this stage, and indeed throughout the whole of Lewin's paper, no evidence is given to support the theory, and so this defence is not open to him here.

In this respect Lewin is merely carrying on the explanatory tradition as used by McDougall and Ross, whose textbooks were essentially reflections on the people and society they saw before them, couched in terms of a particular theory.

Secondly, the derivations which Lewin makes conceptually from the various facts he has seemingly observed, but has in fact posited theoretically, are not logically related to them: that is to say, they do not logically follow from the facts given. Take for example point b), the adolescent having a social position between the child and the adult. We find that Lewin derives point II from this proposition, ie., the existence of a more or less permanent conflict between the various attitudes, values, ideologies, and styles of living which is experienced by the adolescent. However, simply occupying this position does not, or cannot, mean that there will necessarily be conflict. A person may find that this position gives them advantages that sole membership of one of the groups alone can not provide, or they may simply not care which group they belong to. In fact, all of the "facts" about the adolescent (a, b, c) can be seen in the same way - they do not necessarily entail that any of the "problems" of adolescence will be exhibited for any one individual. Thus, Lewin's assertion that the adolescent behaviour should only appear if the structure and the dynamics of the field are such as represented by a, b, and c is not necessarily true. As Chomsky says of Skinner's form of behaviourism in a different context, "When his formulations are interpreted literally, they are trivially true, unsupported by evidence, or clearly false; and when these assertions are interpreted in his characteristic vague and metaphorical way, they are merely a poor substitute for ordinary usage" [7]. In the same way, Lewin's assertion that adolescent behaviour is caused by a transition is either

trivially true, unsupported by evidence, or clearly false.

It is of course also important to discuss the field analogy Lewin uses. He makes it clear that his field theory is intimately connected with his rejection of phenotypical, Aristotelian constructs. He says that it is "hopeless to link the different problems involved in social psychology in a proper manner" (p 145) by using classificatory concepts such as similarity/dissimilarity. Social psychology should use a framework of constructs expressing "'dynamic' properties - properties defined as 'types of relation' or 'types of influences'". In other words, these constructs represent certain types of interdependence" (p 145).

It is by characterizing events and objects in terms of their interdependence that Lewin is able to treat divergent factors such as group-belongingness, bodily changes, and attitudes, as contributory factors in his discussion of adolescence. Seeing dynamic processes or properties in terms of interdependent factors is a natural consequence of his discussion of Galilean science, in which the main task is to determine how various factors are related in the production of an observed phenomenon (eg.  $s=1/2gt^2$ ). In this paper he calls explicitly for a transition to the use of Galilean conceptualizations (p 145) [8]. Additionally though, he gives an example of the importance of this shift, to constructs based on interdependence, in his definition of a "social group", to show the improvement that the shift can produce when used in social psychology.

Lewin speaks of Gestalt psychology as discovering that a dynamic whole has properties which are different from the sum of its parts [9]. He tries to show how, theoretically, a social group can be conceived of as a dynamic whole, which has properties different from the people who make up the group. To conceive of groups in this way avoids problems of defining groups in terms of the similarity of their members, an approach which was taken at the time to explain the behaviour of groups of people. A man, wife, and baby are considered a group for some purposes, yet, Lewin suggests, they are very dissimilar, even though they certainly are a group of sorts. He concludes:

It is typical of well-organized groups of a high degree of unity to include a variety of members who are different and who have different functions within the whole. Not similarity, but a certain interdependence of members constitutes a group.  
(p 147)

As noted earlier, this is one of the tenets of Lewinian theory that is still explicitly alive and well in social psychology today.

This characterization of a social group is meant to reinforce Lewin's contention that diverse problems such as the behaviour of groups and the problems of adolescence, can and should be treated in the same fashion, in terms of interdependence, even if it is not immediately apparent how this can be achieved.

In his discussion of groups he is trying to overcome

the prejudice that groups are formed of similar people, or of people with similar goals, or even with a common enemy. Not so, says Lewin. The interdependence of the group members is more important. However, we should realize that, firstly, he does not specify the type of interdependence he is suggesting is important, and secondly, that whatever this kind of interdependence is, he is using it to show that it is worthwhile to shift to concepts based on interdependence in other areas, especially in the area of adolescence. What he has in fact done is to create an analogy, to say that it looks as though some puzzling aspects of group behaviour can be better explained by concepts emphasizing interdependence (whatever that is) because this procedure has worked elsewhere, and that therefore we should use concepts of this sort everywhere. This is of course the manoeuvre he used originally to support the transition from Aristotelian to Galilean concepts. In both cases though, the analogy conceals a very bare assertion, by creating the impression that the two cases are just different forms of the same underlying principle.

There is no attempt to show how the two cases are similar, or why concepts working, or seeming to work, in one case should work just as well in the other. In fact, the only argument that is given at surface level is that it is only by using interdependence to characterize events in adolescence that such divergent factors as group-belongingness, bodily changes, and attitudes can be linked! (p 145). There is no suggestion as to how the factors are linked, just the



assertion that they are in fact linked in some way. The similarity of the processes involved is something that Lewin takes for granted. He asserts that dynamic processes are operating, and that therefore these processes can be investigated in a pre-defined way.

The blunt assertion that such a similarity exists is not enough to be convincing though, when we realize that although the factors which influence adolescent behaviour are related to one another in some way, the manner of this relationship cannot be the same as that of the relationship between the members of a family group. In the first case, the factors which influence behaviour may affect one another to produce what is recognized as adolescent behaviour. In the second case, a family is interdependent in a completely different way, in financial terms, in emotional terms, and so on. To say that both cases are cases of interdependence may be true, but the misleading impression can be created that the two cases are similar in other ways apart from being interdependent. If they are not, and Lewin has not shown that they are, then there is no a priori reason to investigate them both using one all-purpose methodology, or to treat them as being conceptually similar. Therefore, although it may be correct to say that in both cases factors influencing behaviour are interdependent, calling them interdependent does not mean that they are interdependent in the same manner. The word "interdependence" can be applied to a variety of very different cases, so the correct application of the word does not entail anything concrete about the phenomena which it describes.

In his conclusion to this paper, Lewin shows the aspects of the field-theoretical approach he thinks he has illustrated by his discussion. He believes the most important aspect to be the possibility of linking in a definite manner a variety of facts which, from the classificatory (Aristotelian) point of view, seem to have little in common, "such as the process of learning and orientation, time perspective, planning, problems of individual maturation, conflicts and tension, group belongingness and the marginal man, and bodily change" (p 148/149). He also believes that the possibility of linking divergent facts can be accomplished by the use of constructs which characterize objects and events in terms of interdependence, rather than the Aristotelian phenotypical similarity or dissimilarity. He argues that such a characterization is easier than the Aristotelian, because:

It is evident that if one characterizes an object or event by the way it affects the situation, every type of fact is placed on the same level and becomes interrelated to every other fact which affects the situation. The problem of whether or not one is permitted to combine concepts of values with those of bodily weight, for example, vanishes when confronted with the simple truth that both facts influence the same situation. (p 149)

It must be remembered that the above formulation is an assumption which Lewin makes. Of all the consequences following from his emphasis on interdependence [10] this is the most important, because it allows him the greatest methodological lee-way, by asserting conceptually that all

factors influencing a situation or a behaviour can be dealt with in essentially the same way.

There are problems with this position. Although factors in my psychological environment have an influence on my behaviour, there is no reason to suppose that they will have this influence on every occasion. For example, I may want to complete a task, meaning that the task has a positive valence, yet on a different occasion, that task may have no valence for me at all. I may want to finish writing this thesis, which could be a long-lasting and all-pervasive desire, yet I can still do things which are not influenced in the slightest by this desire.

There is no need to suppose that the influence shown by different factors is of the same sort; some factors may be qualitatively different from others. I may wish to write a symphony, and I may want a drink of water. These are two desires, which can be represented topologically within Lewin's system as two regions of positive valence, but there seems to be no real way of comparing the two desires; they are so very different from each other. Lewin may say that they are comparable because they both affect what my behaviour will be at a given moment if they are in my psychological field. Even so, it does not seem very useful to say that I had a drink because it had a stronger valence for me than writing a symphony. It may make better sense to say that I decided to have a drink, and to put off writing the symphony until afterwards. To insist that at the moment I decided to have the drink, the drink had a greater valence

for me than the symphony seems to be misleading as a description of my desires.

Similarly, to say that having a drink of water and writing a symphony are interdependent, unless meant very loosely, again seems possibly to be a gross distortion of the truth. Yet on Lewin's account, they both can be in a person's psychological field at the same time, and therefore they must be interdependent, in that they both influence the same situation. For Lewin, if two factors are in an individual's psychological field at the same time, they must necessarily be interdependent, but this assertion can lead to the possibility of distortions and confusions. It may be true to say that these two factors can influence a person's behaviour, but to say that they are interdependent, or that their influences can be compared, could be a distortion of what is more easily explained in everyday terms, by saying that they are factors which are considered when somebody acts. It is easy to conjecture that all relevant factors may be related to each other in some way, but Lewin gives no indication of how they are interdependent in reality. He has made a metaphysical assumption, which he carries with him throughout his work. It is enough to notice at present that this assumption may be unfounded.

#### CHAPTER FOUR: FOOTNOTES

[1] For examples of these pictorial representations, see appendix 1.

[2] See appendix 1 [fig. 16: p 136].

[3] See appendix 1 [fig. 17: p 138].

[4] In Carmichael (1946), and also in Field Theory in Social Science (1952).

[5] Lewin adds a footnote to his definition of a field, relating it to Einstein's work in On the Method of Theoretical Physics (1933).

[6] In The Seven Per Cent Solution a novel by Nicholas Meyer, Sherlock Holmes meets Sigmund Freud, and after several adventures, they solve a case of murder together. Michael Shepherd, in Sherlock Holmes and the Case of Dr. Freud, has pointed out that, improbable as it may seem, there is in fact more than one similarity between Holmes and Freud, the two most important being an interest in cocaine, and "the use of a method" (p 11). Meyer recognizes this similarity of course, and he has Holmes explicitly acknowledge this second fact when he responds to Freud's comment that his professional outlook is akin to medical observation, by remarking; "You have succeeded in taking my methods - observation and inference - and applied them to the inside of a subject's head" (p 196). Holmes' remark is very interesting, but what is more interesting from my point of view is what happens next in the novel. Freud replies to Holmes; "Yet what I have surmised may prove totally erroneous. You yourself have noted the dangers of reasoning with insufficient data at one's disposal" (p 196). Holmes then replies:

Remarkable...Not only does it possess the ring of truth - or of plausibility if you prefer - it also conforms to certain facts and theories I shall now lay before you. (p 196)

This passage serves to remind us that an idea or an analysis can carry with it the ring of truth, which is a certain plausibility, without in fact being true. This is of course a point which Popper makes about psychoanalysis in Conjectures and Refutations. Shepherd elaborates on this theme, reporting that the Sherlockian procedure may appear to be deductive and logical, but that in fact it is really intuitive and illogical (p 20), and that psychoanalysis is more a mythology than a science, a mythology which brings some order into incoherence. Lewin too would say that he was bringing order into chaos in his use of the Galilean method, but my feeling is that he too has only produced a mythology, which makes sense up to a point, and then swiftly becomes incomprehensible and misleading.

[7] Noam Chomsky, "Psychology and Ideology". In For Reasons of State, 1970, p 324.

[8] "The transition from phenotypical concepts to dynamic...constructs based on interdependence is, to my mind, one of the most important prerequisites for any science which wishes to answer questions of causation. Psychology is in the midst of a period of transition to this type of concept. Social psychology, and sociology too, will have to turn definitely in this direction." (p 145)

[9] This is a statement which he is quick to distinguish from

the popular statement "The whole is more than the sum of its parts" (p 146).

[10] Two of the other consequences are that the occurrence of a behaviour will depend upon the entire field in operation at the time, and that representing social psychological facts by dynamic concepts can explain both the usual as well as the unusual case.

## CHAPTER FIVE

We have seen Lewin's call for a conceptual transition, and his insistence that all facts which affect situations are to be dealt with on the "same level" and in terms of their interdependence, and we have seen that there appear to be problems with the position he takes. What we need to show more fully is how these assumptions have been transformed into the rationale for performing experiments, and how these very same assumptions are still operative in modern social psychology.

The way in which this translation is achieved follows from the assumptions we have seen Lewin make. Behaviour is thought of as being a function of the person and their environment ( $B=f(P,E)$ ). The psychologist deals with the life space, and for each individual the life space consists of their needs and psychological environment. As Marrow puts it:

All psychological events occur within the life space; or stated somewhat differently, the 'life space is the total psychological environment which the person experiences subjectively'. (p 35)

Explaining behaviour as a function of the individual and the way they see the world is attractive, because it imposes a coherence on behaviour, by saying that at heart, all behaviour is an expression of the same fundamental interaction. Also, by creating the concept of the life space, the appearance is created that the space, which is ordinarily a physical concept that is applied here to a psychological phenomenon, can be delineated and measured in the same way as

other physical concepts.

According to Lewin's formulation, the life space includes needs, goals, unconscious influences, memories, beliefs, events of a political, economic, or social nature, and anything else that might have an effect on behaviour. Within the field theoretical conception, all these different factors are interdependent, and Lewin felt that the best way to deal with these interdependent factors was through the use of the dynamic concepts of tension and force, concepts whose use seems to follow naturally from the kind of scientific outlook he was using. If we represent the psychological environment as a set of interdependent, yet separate regions or variables, each possessing a certain valence, then the natural way to represent a behaviour, or in other words, a change of position in that psychological field, would be in terms of a vector, a directed resultant of all the valences operating at the given instant. This would, after all, be the way in which the movement of a physical object through a field of physical forces would be represented. It was these considerations which led Lewin to define psychological needs as tension systems and their topological representation as vectors to represent motion.

Once we have the conception of behaviour as a motion, all we need to do to understand the behaviour is to find the forces that are operating on the person in a given situation. Thus, to understand concrete behaviour we need to deal with the concrete situation as it presents itself, and Lewin's



terminology of valences and tensions offered a way of conceptualizing this situation.

We can see now why Jones (1985) has such a high opinion of Lewin's contribution to social psychology. By conceptualizing situations in this way, Lewin offered the possibility of dealing in a structured way with the inferred dispositions which influence people's responses to external stimuli, as well as implicit or imagined stimuli.

In "Defining the Field at a Given Time" (1943) Lewin addressed the problem of how to actually determine the character of a situation, and therefore how to understand behaviour in practise. He argued that there are at least two possible ways of making such a "diagnosis". "One may base one's statements on conclusions drawn from history (anamnesis), or one may use diagnostic tests of the present" (p 49).

To illustrate these two techniques he gives an example of wanting to know how strong an attic floor is. One way to find the strength of the floor would be to find out what materials were used when the floor was built, or to find out whether the architect was dependable, or to find the original blueprints, in which case certain calculations can be made about the strength of the floor.

However, as Lewin points out, the workmen may not have followed the blueprints, insects may have weakened the woodwork, or any number of unknown things may have happened

to the floor. This being the case, the best way to diagnose the condition of the floor is not through an anamnesis, it is by testing its strength as it is now. Testing is preferable to an anamnesis because the anamnesis consists typically of two steps, the testing of certain properties in the past (of the quality, size, and structure of the woodwork), and the proof that nothing has interfered in the meantime (p 49), which is very difficult to achieve.

A test of the present situation is methodologically superior to an anamnesis. Lewin believes that:

The method of determining the properties of a situation ( $S^t$ ) by testing them at that time  $t$  avoids the uncertainties of historical conclusions. (p 50)

To determine the character of a situation we need to test that situation. Given that the situation in psychology is held to consist of a multitude of factors that can affect behaviour, Lewin considered that the best way to discover what the operative forces are in any situation would be to use the same method that Galileo used to discover the relationship between the situation and the free fall of a body, by manipulating the situation and noting the consequences. From his topological assumptions he believed it theoretically possible to isolate the factors which influence behaviour; the next step is to experiment with these different factors to find which of them affects behaviour in which particular way.

To make Lewin's thinking on experimentation clearer it is useful to look at another source. In one of his last papers, "Behaviour and Development as a Function of the Total Situation" (1946) [1], Lewin discusses theories and constructs, and laws and individual cases. He argues that there are two steps which need to be taken to explain behaviour. The first step is to find a scientific representation of the life space, and the second step is determining the function (F) which links the behaviour to the life space. "This function F is what one usually calls a law" (p 919). To explain behaviour we need to know what the life space is (L Sp, or P,E) and the relationship between the person and their environment, which is F, and behaviour can then be predicted from the formula  $B=f(P,E)$  (or  $B=f(L\ Sp)$ ).

To determine what F is, experiments have to be performed. Lewin expresses this assertion thus:

The laws (that is, the relation between behaviour, on the one hand, and the field characterized by certain constructs, on the other, or between various factors determining the field) should be verified by experiment. A law should be accepted as valid only if it is not contradicted by data in any branch of psychology. In this sense a law should always be general. (p 921)

This prescription for the determination of laws is part of Lewin's belief in a theoretical psychology. He says that terms such as "need", "association", "conditioned reflex", "excitatory tendency", "gestalt", "libido", and "super-ego" are examples of the theoretical constructs which different schools of psychology have used to try to

characterize the dynamical or genotypical facts underlying overt behaviour (p 910). Lewin however, feels that it is important that theories should not just be speculative, but should be open to testing. A theory which can produce laws through experimentation, relating behaviour and the individual's life space, is superior to a theory which is speculative, and without such support [2].

There is a problem relating to this procedure though. It is the difficulty arising from the individual differences which exist between people, which may seem insurmountable when trying to formulate general laws. Lewin explicitly recognizes the problem inherent in his approach.

Objectivity in psychology demands representing the field correctly as it exists for the individual in question at that particular time. For this field, the child's friendships, conscious and 'unconscious' goals, dreams, ideals, and fears are at least as essential as any physical setting. Since this field is different for every age and for every individual, the situation as characterized by physics or sociology, which is the same for everybody, cannot be substituted for it. (p 920)

Even though these differences exist, Lewin suggests that general laws can be found, simply because there is a relationship between general laws and individual differences. For example, it has been found that the velocity with which an activity is satiated increases with the degree to which the activity is psychologically central (as against peripheral) [3]. This proposition has, according to Lewin, the nature of a general law. It explains many things, including why both agreeable and disagreeable activities are

more quickly satiated than relatively neutral ones (and also why fashions in women's clothes change faster than in men's clothes !). He argues that this example shows how problems of individual differences, of age level, personality, specific situations, and general laws, are closely interwoven. A fact about the individual's life space, that an activity is psychologically central to them, is related to overt behaviour by such a law.

A law is expressed in an equation which relates certain variables. Individual differences have to be conceived of as various specific values which these variables have in a specific case. In other words, general laws and individual differences are merely two aspects of one problem; they are mutually dependent on each other and the study of one cannot proceed without the study of the other. (p 921)

If we have a law it will show the relationship between variables. It will show why a behaviour is non-existent or very slight by showing that the related variables interact in such a way as to produce this slowness. For example, behaviours are not satiated quickly because they are not psychologically central. This is similar to the way in which the speed of free falling can be explained. In some places it may be so slow as to be almost imperceptible, but in others so fast as to be immeasurable. It can be explained in either case by the law, which shows how the relevant influences vary in relation to each other.

This then is the theoretical background to Lewin's use of experimentation. To understand his use of experiments more fully, it is worth discussing some experiments which he

was closely connected with. There are three reasons for such a discussion. Firstly, Lewin himself thought highly of the experiments we will discuss; secondly, they are held to be shining examples of the power and usefulness of Lewin's theorizing; and thirdly, they show the application of dynamic concepts, such as tension, to problems in psychology. Therefore, if we can cast doubt upon their adequacy, it could be an indication that the foundations which experimental social psychology was built on were not altogether solid themselves.

Experimental work dealing with tension systems and the satisfaction of needs was very prominent in providing support for Lewin's metatheoretical ideas, so it is worth discussing these experiments to show how they were performed, their relationship to the theoretical assumptions Lewin made, and the function they performed in verifying these theoretical bases. By investigating these experiments, we should be in a better position to judge the adequacy of Lewinian experimental theory.

#### CHAPTER FIVE: FOOTNOTES

[1] In L. Carmichael (ed), Manual of Child Psychology.

[2] An essential part of Lewin's prescription for experimentation was that it be used to test theory, theory being indispensable to any science which hoped to progress. Marrow represents Lewin as thinking that theory should fulfil two functions, to account for what is known and to point the way to new knowledge. Hall and Lindzey (1978) give an example of this aspect of Lewin's work. They say that: "[Lewin] saw...the necessity of spelling out in detail the basic assumptions made by psychoanalytic theorists regarding the substitution of one activity for another. When this was done in terms of an organization of segregated tension systems...the way was provided for experimental attack" (p 431). Experimentation should be able to point the way to new

knowledge by showing the mechanisms by which one activity is substituted for another. It accounts for what is known, as does the psychoanalytic theory, but it is able to go further in being able to test the theory.

[3] Karsten "Physische Sättigung". Psychologische Forschung, (1928).

## CHAPTER SIX

Lewin used the term "tension system" to refer to any field in which behaviour is directed toward some goal, or in his own terminology, towards some region with high positive valence, valence being defined as the degree of attractiveness an individual, activity, or object possesses as a behavioural goal. Obviously, behaviour of this sort will be of interest to psychologists, as well as to "lay" people because it is the type of behaviour people are most interested in explaining. Behaviour with a "goal", or with an aim in mind, is what people often see around them, and they are likely to ask questions about it. For example, "Why did you push me into the river?" could be followed by the question "Why do you dislike me?" and these are questions we can provide answers to. There is sometimes the feeling though that we do not know the answer to these types of question, and this is where social psychology takes its cue, and tries to provide an answer for us. Lewin's system is one way of providing these answers.

According to Lewin, tensions can arise from some need within the person, physiological or psychological, from some feature of the environment, or from both together. When a state of tension exists, the individual is set in motion within their psychological field, and this motion can be represented as a vector in the individual's life space. If the goal is reached or achieved, then the tension system is relaxed, and a state of equilibrium is created. When the goal is not achieved however, the tension remains, apparently in



the sense of there being a force acting on the individual, a "pull" towards achieving the goal [1].

It was Lewin's earliest pupils who tried to investigate tension systems, and thereby to chart the subject of motivation, which had previously been the "no-mans" land of psychology as Marrow puts it (p 41). The experiments they performed are generally cited as being important demonstrations of Lewin's ideas [2], and we will look briefly at two of these studies, which Lewin himself saw as being important, and the first of which (Zeigarnik, 1927) he described as being an attempt to "break a first path through a primeval forest of facts and assumptions, using as compass concepts the practical utility of which was still wholly untried" [3]. Additionally, Lewin was enthusiastic about Ovsiankina's 1928 study which expanded on certain points of Zeigarnik's work, so we will discuss her paper as well [4].

Zeigarnik wrote about the retention in memory of completed and uncompleted activities. The rationale underlying this experiment was that if a purpose or intention corresponds dynamically to a tense system, the state of tension in the system should be evident not only in the tendency to complete the activity, but also in its better retention if it is not completed [5].

Lewin had theorized that the desire or intention to carry out a specific task corresponds to the creation of a system of psychological tension, the need to release this tension serving to sustain the goal-directed behaviour until

the desired task is completed. The system thus created makes its influence felt in thought or action (or both), until the tension is discharged. What Zeigarnik was attempting to do was to find whether the "quasi-need", the impulse to release the tension which is created by this process, influences behaviours other than completing the task, such as memory.

Zeigarnik performed a series of experiments where subjects were given a number of tasks to perform. They were allowed to complete half of these tasks, but were prevented from completing the other half. After a while the subjects were asked to recall all of the tasks they had performed, whether they had finished them or not. Zeigarnik presented the amount of recall in the form of a quotient (commonly called the Zeigarnik quotient) which expressed the ratio of the remembered, uncompleted tasks, to the remembered, completed tasks;

$$\frac{\text{Unfinished tasks recalled, IR}}{\text{Completed tasks recalled, CR}}$$

Zeigarnik found that:

The results obtained from our first 32 subjects indicate an average memory advantage of 90 per cent enjoyed by interrupted tasks (ie.  $IR/CR = 1.9$ ) [6]

This finding is taken to be a confirmation of Zeigarnik's hypothesis. She had asked the question, "What is the relationship between the status in memory of an activity which has been interrupted before it could be completed and

of one which has not been interrupted?" (p 300). The hypothesis that was generated from this question, and also from the general theoretical approach adopted, was that an unsatisfied quasi-need probably influences even purely memorial retention.

The hypothesis was based on the theoretical state of tensions, and so on, that Lewin proposes as an explanatory system. Thus, in her summary of the experiment, Zeigarnik suggests that the quasi-need to finish the tasks corresponds to a state of tension "whose expression may be seen not only in the desire to finish the interrupted work but also in memorial prominence as regards that work". Importantly, she also suggests that "prominence of the quasi-need to recall unfinished tasks depends upon the intensity and structure of the tension system, and also upon the strength and kind of quasi-need set up by the experimenter's instructions" (p 313). With this experiment in mind it is worth discussing part of the background to it.

"Tension systems" and "quasi-needs" are hypothetical constructs which Lewin posited to explain what may well have initially been a simple observation. Marrow (p 27) reports that Lewin and a group of his students were in a cafe, and had been ordering different things over a period of time, when Lewin called for the bill. The waiter knew exactly what everyone had ordered, and was able to present an exact bill for them all. However, when Lewin called the waiter to the table about half an hour later and asked him to write the

bill he was unable to. "I don't know any longer what you people ordered", he said, "You paid your bill."

Marrow quotes Donald MacKinnon, an American student studying with Lewin in Berlin at the time, as saying:

In psychological terms, this indicated that a tension system had been building up in the waiter as we were ordering and that upon payment of the bill the tension system was discharged. This approach intrigued me, because it was what I had been saying, that a lot of important problems had to be approached through observations of behaviour in everyday life and that our task was to translate these into some kind of language and method that would bring them into the laboratory and permit some quantification of the phenomena in question. (Marrow: p 27/28) [7]

As we have seen, the way in which this quantification was brought about by Lewin was by conceiving of the person as a kind of complex energy field in which all behaviour could be thought of as a change in some state of the field during a given unit of time.

Thinking of the person in this way led Lewin to postulate that behaviour of every kind, including wishing, thinking, achieving, and striving, is the product of a field of interdependent variables, a result of change in some state of a field in a given unit of time. Thinking of the field which determines behaviour in the same way as he conceptualized the family, for example, as being a whole consisting of interdependent but distinctly separate entities (Mother, Father, Child) allowed Lewin to visualize it in terms of topological regions which are separate from each other yet interdependent. Thus the variables which these

regions represent, which produce changes in behaviour, are seen as being separate and distinct, in the same way as the slope of the plane in Galileo's experiment is distinct from the velocity of the object that moves down it, even though the slope is a factor in that velocity.

Accepting this conceptualization means that the separate variables in the psychological field can be manipulated to produce changes in behaviour, these changes being related to the variables being manipulated through intervening concepts such as tensions and forces.

Relating these ideas to the observation that the waiter had forgotten the bill led Zeigarnik to investigate the situation by making a manipulation involving the factor which was thought to create the effect, namely the task, leaving all other conditions constant. For the waiter, the task was to remember what everyone had ordered, and then to present a bill when asked. When this task was completed there was no longer a quasi-need operating to sustain memory, and therefore no tension either. To investigate the effect of task on memory, Zeigarnik's basic manipulation was to make sure that some tasks were uncompleted, whilst keeping all other conditions the same. The psychological environment is assumed to only have changed with respect to finishing or not finishing tasks, the Zeigarnik quotient showing the difference between the two conditions. The fact that more uncompleted tasks than completed were remembered led Zeigarnik, and Lewin, to believe that proof had been found

for the existence of tension systems and quasi-needs.

Ovsiankina's paper dealt with a related topic, the resumption of interrupted activities. In the experiments she conducted, a person would be interrupted in the task they had been given to perform, and after a while would be left without a task at all ("a situation of relative freedom was created", Lewin, 1935: p 242). Ovsiankina found that "a frequent resumption of the interrupted activity" resulted from this situation. (Lewin, 1935: p 242)

Ovsiankina investigated the influence of various factors on the frequency of resumption, including: 1) The kind of activity; 2) the phase in which the activity is interrupted; 3) the duration of the interruption; 4) the nature of the act of interruption; 5) the presence or absence of the uncompleted task at the end of the interruption; and 6) the attitude and character of the person involved. In all of these conditions it seems that there was a tendency to resume the activity.

Lewin claims that Ovsiankina's paper "contains proof that the effects of a purpose or intention is the formation of a quasi-need, that is, dynamically, of a tension system" (p 242). The tension system which is created by the intention then "drives toward discharge" and "causes activities which serve the execution of the purpose" (p 241). The existence of the tension systems is substantiated by the resumption of the activity, as predicted by tension system theory.

These two experiments are cited by Lewin and others as being proof that tension systems exist, and that they influence behaviour. They are important experiments, because they supposedly show that experimental procedures can be used to deal with questions of motivation, and also because they show the mechanism which lies beneath the completion, or resumption, of tasks. Tension systems explain why uncompleted tasks are remembered, and why interrupted activities are resumed.

The tension system seems to occupy an ambiguous role in the explanation though, because Lewin originally suggests that an intention or a purpose creates it. Once it has been set up, the completion of the activity is seen as a result of the tension system, and strangely, not of the intention that created it. If the tension system is set up as an analogy to explain what is happening, we must ask what purpose it serves. Talk of the original intention is forgotten, and replaced by talk about tensions, so obviously if it is an analogy, it is meant to be taken seriously. The question is, why should we speak of tension systems at all?

Does talking of tension systems perform any useful purpose? Is there an advantage to saying that someone completed a task because their tension system was strong enough, rather than saying that they completed the task because they wanted to? Zeigarnik explains the relationship between the two terms:

When the subject sets out to perform the operations required by one of these tasks there develops within him a quasi-need for completion of that task. This is like the occurrence of a tension system which tends towards resolution. Completing the task means resolving the tension system, or discharging the quasi-need. If a task is not completed, a state of tension remains and the quasi-need is unstilled. The memorial advantage enjoyed by interrupted tasks must be due to this continuation of the quasi-need.

The tension leading to gratification of a need can therefore be seen to operate not only towards completion of the task; it also improves the chances of later recall in cases where such completion has been obstructed. Hence recall serves as a sign indicating the existence of such a tension system.  
(p 305/306)

Recall is used to prove the existence of tension systems within the individual, but it is clear that in fact some kind of re-description has taken place. The "strength" of an intention has been replaced by the strength of a tension. If there is no quasi-need to complete the task, or no intention to do so, then no tension system is created, and so non-completion of the task can be explained by the absence of this tension system. Similarly, failure to recall a task can be explained by the absence of a tension system. Crucially though, it might also be explained, without any loss in content, by saying that if a person had no intention of completing a task, it might be that they put no value on it, and would have no reason to remember the task, they would be more likely to forget it.

What is important though is that the psychologist explains events in terms of a causal, or mechanistic, description, which alters the kind of explanation being given. Changing to this kind of explanation implies that



predictions could be made, because causal connections are found relating to behaviours, which ordinary speech does not always acknowledge. Of course, predictions about people's behaviour are made in everyday, common sense terms, but when these predictions fail, nothing conceptually important is affected by the failure. We say we were wrong, or had neglected some information, or the person perhaps acted willfully. A failure of "causal" explanation is much more important though, because this failure would mean virtually that a law of nature had been broken. If we drop a lead weight from the top of the Leaning Tower of Pisa and it floats upwards, something very strange is going on. Likewise, if Lewin and his followers are serious about finding causal relationships relating to behaviour, which they certainly seem to be, then the implication is that they would, in theory, be able to make predictions about behaviour of the same seriousness as physics does about the physical world, meaning that it should be possible to predict future behaviour without exception. Predictions are stricter, and failures of prediction much more damaging than in ordinary speech. This fact raises the question of whether such a reliance on a causal, mechanistic explanatory system is justified in social psychology, along with the related question, as to whether Lewin's methodology can provide such explanations. These important issues will be discussed more fully shortly.

Returning to Zeigarnik's experiment, we can see more conceptual problems arising. Even when more obviously environmental factors are taken into account in explanations,

for example, Zeigarnik investigates the effects of the structure of the task as an activity with a definite end as against a continuous activity, it is just as easy to form an explanation in terms of intentions.

There are many examples such as this one where what Lewin and his followers suggest should be accepted as an explanation of behaviour seems to conflict with what we might ordinarily say was an explanation. These examples are not far-fetched, and can seem appropriately explained by common sense notions. Therefore, they require some sort of response from the theorist who maintains a contrary position, to resolve this conflict one way or the other. The theorist can either reject their theory, as being unable to deal with what seem quite plainly to be relevant facts, or can try to extend the theory to explain these facts or allow them to be exceptions to the theory. However, the theorist, in this case Lewin, may do neither, and maintain instead that because the theory looks "deeper" into behaviour, it is not surprising that it conflicts with shallow, or naive explanations [8]. Failing to acknowledge such counter examples seems to indicate that in some sense Lewin is not using language in the same way as it is used ordinarily.

For example, to say that we act as we do because we are forced to, makes sense on some occasions. We may be "forced" to attend a meeting we do not want to, or to agree to take on a responsibility we do not want. Of course, there are many occasions when we are not "forced" to do anything,

when we make a free decision and just decide, perhaps on a whim, to do something, not eat our lunch for example. Lewin, however, seems to maintain that all behaviour is the result of a force or tension, which operates in our psychic systems, and thus maintains that examples such as not eating our lunch are not examples of an "unforced" act, but are just examples of mistaking an "appearance" for "reality", the reality being the forces operating to determine behaviour. To extend this use of the word "force", and to refuse to acknowledge counter examples where this use is not acknowledged, means that Lewin must show the superiority of his system of explanation in order to justify using it to replace common sense notions of forces etc. It will be contended that in fact he fails to do so, and that therefore his system should not be taken seriously as an alternative to common sense notions. There may be places where it can be useful to use notions such as Lewin does, but not generally.

Zeigarnik considers objections to the explanations she gives, in her discussion of the experiments. She explores possible explanations of why interrupted tasks were recalled best, considering first that there might be a "shock" related to being interrupted which emphasized these tasks, and made them easier to remember. To try to test this idea, she altered the experiment slightly, interrupting some tasks and then re-presenting them for completion. This manipulation was meant to ensure that completed and uncompleted tasks had the same "shock" value.

However, when the experiment was repeated in this way

it was found that the Zeigarnik quotient was virtually unchanged, seeming to show that the memorial advantage shown for interrupted tasks was not caused by the emphasis given to them by the interruption.

An alternative explanation she considered was that the experimental subjects thought that the tasks which were interrupted would be resumed later, and that therefore they should try hard to remember those tasks. To test this idea, Zeigarnik told some subjects at the time of the interruption, "This task will be resumed later", and others, "You are not to work on this task any more". On the basis of this explanation, she thought that subjects given the first instruction should remember interrupted tasks much better than those given the second instruction. When the experiment was performed it was found that there was in fact virtually no difference between the two (1.7 as against 1.8).

Finding that these explanations were unsatisfactory, Zeigarnik decided to look elsewhere for an explanation. "The memorial advantage of uncompleted tasks lies not in any experience accompanying the interruption but rather in the forces existing at the time of recall" (p 305). She found her explanation in the psychical difference between completed and uncompleted tasks at the moment of recall, putting forward the explanation noted above, using quasi-needs and tension systems as explanatory concepts, and using, of course, the Lewinian assumptions.

Zeigarnik's explanation takes the following form;  
When the experimenter asks the subjects to recall the tasks  
"a desire or quasi- need" (p 306) is set up to recall all of  
the tasks.

Dynamically expressed, the situation at the time of recall may be described as follows: a quasi-need to report all the tasks has been established by the experimenter's request; in addition however, there are quasi-needs leading to recall of the unfinished but not of the finished tasks. Just how strong the tension favouring recall of unfinished tasks is will depend upon the relationship between these two fundamental factors. If, in accordance with instructions to repeat all tasks, the desire to do this is overweening, the relative advantage enjoyed by unfinished tasks (IR) will be diminished and IR will approximately equal CR. On the other hand, if this desire is not excessively strong, the advantage enjoyed by IR will be determined almost entirely by the unresolved tensions of the interrupted tasks.  
(p 306)

This formulation can explain the more detailed findings Zeigarnik made, that a wide variety of quotients existed, even for the relatively small samples used. The extremes extended from a 500 per cent advantage with one subject to a 25 per cent disadvantage with another (p 301). Of the 32 subjects, 26 remembered interrupted tasks best, 3 remembered them both equally well, and 3 remembered completed tasks better. There were also differences in recall for the tasks themselves. In her summary, Zeigarnik introduces many caveats to the general law offered, that interruption of an activity leads to better recall of that task. The above formulation allows these variations to be explained, as a general law should be able to do.

To recap, these experiments are examples of Lewin's

contribution to the foundations of social psychology, of his provision of the rationale and theoretical foundations for experiments to be performed in the area of motivation and other aspects of social behaviour, which had previously been thought to be immune to them.

The proof of the assertion Jones (1985) makes, that "Lewinian metatheory...was directly instrumental in converting social psychology into a primarily experimental discipline" (p 71), is that the same metatheory underlying Zeigarnik and Ovsiankina's work in the 1920s underlies the experimental work being done today.

To repeat something of the evidence we have already seen about social psychology and experimentation, Manstead and Semin (1988), in Hewstone et. al.'s textbook, agree that experimentation has been the dominant methodology in social psychology for the past three decades (p 64). They say that it is an ideal method for providing unambiguous evidence about causal relationships among variables, and that therefore it is without parallel as a method for testing theories that predict such relationships. They also note that standard guides to research in social psychology, such as Aronson, Brewer, and Carlsmith (1985), in The Handbook of Social Psychology, tend to treat experimentation as the preferred research method in social psychology [9].

Even though the style of experimentation has been tinkered with in some ways since Lewin's day, the "ideology"

remains the same, the conceptualizations of variable and manipulation which make experimentation possible are still intact, as can be seen from an experimental study published in a journal in 1990.

The experimental study which will be discussed was chosen from a respected journal, The Journal Of Experimental Social Psychology, and was published in the most recently available edition, May 1990, for use in the writing of this thesis. This issue, and others, were consulted, and no reason was found to believe that the study is unusual in any way, either in content or methodology, and its aim, of clarifying the results of previous experiments and supporting the use of a particular conceptual approach, suggests that as an example of the current practice of social psychology it is at least no worse than any other which could have been selected. For instance, all of the papers in this issue of the journal deal in one way or another with cognitive processes, or information processing, both of which occur at a level below awareness [10].

The paper in question is written by Patricia Pliner and Shelly Chaiken, is entitled "Eating, Social Motives, and Self-Presentation in Women and Men", and is an attempt to explore the notion that eating behaviour can serve a role in "impression management". Impression management theory is an approach which has in recent years offered an alternative to explanations based on "dissonance" (an important concept in social psychology for many years), and which refers to the behavioural strategies that people use to create the social



images or identities they desire (Hewstone: p 189).

Impression management theory also assumes that "individuals have a social concern for appearing consistent to others, and may feign attitudinal responses to create the impression of consistency" (Hewstone, glossary: p 451).

The study was set up on the basis of various previous findings, which suggested that "in our culture females, in comparison with males, evidence a greater concern with food, eating and body weight" (Pliner et. al.: p 240/241)

Impression management theory, as advanced by Schlenker (1980, 1982, and 1986) posits that, if, in a particular situation, a woman wishes to claim a feminine social identity, she might behave in a way that conveys an impression of femininity. "Managing an impression of femininity through restricting food intake would be one means of doing so. Further, any situational variable that increases the desirability of a feminine social identity - eg., a male (vs female) eating partner - should enhance women's tendencies to restrict their food intake" (p 241).

This is the "deep" background to the experiments; more recently, a study (Mori, Chaiken, and Pliner, 1987: Experiment 1) found that males ate less with a female than a male partner, which was an unexpected result, because it had been assumed that the amount eaten was irrelevant to the male sex role, because it would not be useful for conveying an impression of masculinity for men (p 241). In the study we will be discussing, Pliner and Chaiken performed two experiments which they hoped would help them to understand



the above findings more fully.

Manstead and Semin (1988) define a standard social psychology laboratory experiment in the following way: In an experiment, the researcher deliberately modifies some features of the environment in which the phenomenon under investigation occurs. By controlling the variation in A, resulting changes in B can be discovered. The feature of the environment which is manipulated by the experimenter is called the "independent variable" (Manstead and Semin: p 67). "All other aspects of the experimental scenario are held constant, and the independent variable is changed in some respect with a view to assessing the consequences of this manipulation" (p 67). To assess the impact of the independent variable, some feature of the subject's behaviour or internal state is measured, this measurement being known as the "dependent variable" (p 69).

In Zeigarnik's experiment, the independent variable was the interruption of the tasks, with the dependent variable being the number of tasks that could be recalled, both completed and not completed. In Ovsiankina's experiment, the independent variable was the interruption of an activity, and the dependent variable was whether the activity was resumed. For Pliner and Chaiken there were two independent variables, the desirability of the confederate, and the sex of the confederate. The dependent variable was the number of crackers eaten by the subject.

Experiments are designed to test hypotheses, which are derived from theoretical positions. Zeigarnik and Ovsiankina's were designed to test Lewin's system of explanations involving tensions and quasi-needs, and Pliner and Chaiken's was designed to test the impression management theory.

In their first experiment Pliner and Chaiken used 49 male and 47 female students, who all came to the experiment "food deprived" (p 242) [11]. The experiment required that they eat enough of the snacks provided to become comfortably full, the reason given to them for this necessity being that they were participating in a study designed to assess the effect of hunger on task performance. The students, or "subjects", were paired with a confederate "who was either male or female", and who, in a fabricated interview, had given the impression that they were single and socially attractive.

In order to minimize any spontaneous conversation which might undo the confederate's carefully planned image, while eating the subject and confederate were required to work on a task which permitted very little task irrelevant conversation. The amount eaten by the confederate was always 15 crackers, and the amount eaten by the subject constituted the main dependent variable. (p 242)

The number of crackers that the subjects ate in the different conditions was recorded, and it was found that there were only two statistically significant relationships; that males ate more than females, and that both male and female subjects consumed less in the presence of an opposite-

(vs. same-) sex partner.

In their discussion of the experiment, Pliner and Chaiken related their findings to the sex roles the subjects were performing, the feminine and masculine social identities they were projecting using behavioural techniques, as well as the other aspects of social identity which can be expressed in eating behaviour. What is noticeable is that they used a quasi-technical vocabulary to express their conclusions, which seems, in common with Zeigarnik and Ovsiankina's vocabulary, to be based on what look like ordinary "common sense" explanations. Using eating as a way of appearing a certain way to another person is not something that is entirely unknown in everyday life. An individual may be unaware that they are doing something so complex on a given occasion, but other observers would be able to note something of the sort if they knew that person, and of course, some people would be quite aware that they were doing this. This ordinary knowledge has been translated into a technical terminology and apparently quantified, thus appearing to give evidence of a process of some sort.

Also in common with Zeigarnik and Ovsiankina, an account in terms of sex roles and impression management is able to explain extremes of behaviour, ie., eating less and eating more, by relating the behaviour to the particular sex role or impression that is being projected.

The explanations given here fit the pattern produced by Lewin by being genotypical, systematic, and being

confirmed by experimentation. The rationale for the acceptability of experimentation is exactly the same; all the factors relating to eating behaviours are interrelated, but they are held to be sufficiently separate and identifiable to allow one of them to be manipulated in isolation from the others, the consequences of the manipulation being linked to it in a simple causal manner [12].

From this example of a recently published experimental study we can see that the conceptions Lewin called for are still acting as the philosophical basis for the methods being used in experimental social psychology today. Therefore, we must now turn our attention to assessing the adequacy of Lewin's methods and philosophical reasoning for the task he set himself. This assessment will therefore also allow us to judge the claim of social psychology to give us a "deep" understanding of human social behaviour.

#### CHAPTER SIX: FOOTNOTES

[1] We should perhaps notice the similarity between tension systems and some of Freud's concepts. If tension systems are not dissipated they remain in place as factors in behaviour. Many of Freud's concepts have this property, eg., repressed desires.

[2] Eg. Marrow, Schellenberg, Deutsch and Krauss, Jones, and also Boring, the influential historian of science, who all agree that they were turning points both for Lewin's theory and for psychology as a whole. Marrow (p 41) reports that the studies conducted by Lewin's students can be "rated as among the most distinguished group of empirical inquiries in the history of psychology". Hartmann (1935) says that Zeigarnik's paper was "the first, and in many ways the most impressive" in the series of studies that can be called the work of the Lewin school (p 219), and he also says that Ovsiankina extended and verified these results. Lana says that Zeigarnik demonstrated the validity of some of Lewin's theoretical points (p 87), and Schellenberg, and Deutsch and Krauss, both suggest that Zeigarnik's work is a major achievement in

verifying Lewin's theory. (Hartmann, G.W., Gestalt Psychology: A Survey of Facts and Principles, 1935.

[3] Dynamic Theory of Personality: p 240.

[4] His discussion of these and other experimental works is found in A Dynamic Theory of Personality, chapter VIII, "Survey of the Experimental Investigations". Zeigarnik study originally in Psychologische Forschung, 9, (1927), pp 1-85; Ovsiankina in Psychologische Forschung, 11, (1929), pp 302-379.

[5] (Lewin, 1935, p 243/4)

[6] In Ellis, Source Book of Gestalt Psychology, p 301. This is a partial translation of Zeigarnik's paper. All page references will be to this volume.

[7] Marrow (p 28): "This informal experiment led to Zeigarnik's famous study of the tension system, published in 1927, and completed under Lewin's supervision."

[8] Chomsky argues quite convincingly that Skinner reacts in this way to criticisms of his theory, and that his reaction is inappropriate because it fails to take seriously the objections which are raised about his theory. In "Psychology and Ideology" 1970, eg., pp 322-324.

[9] In the standard work by Aronson, Brewer, and Carlsmith, the authors relate that, in a previous version of the chapter in the 1968 Handbook, two of them, Aronson and Carlsmith, wrote a kind of "cookbook" for social psychology which they hoped would allow almost anyone to perform good experiments (p 441). At the time they wrote the earlier chapter, their preference was for a high impact "scenario" model of experimentation. They say that by emphasizing that style of experiment, they were in effect "codifying some three decades of the Lewinian tradition dating from the classic experiment in which Lewin and his colleagues imposed either democratic, autocratic, or laissez faire adult leadership styles on groups of boys". (They are referring here to the Lewin, Lippitt, and White study of 1939. I see no point in discussing this experiment as I believe it conforms to the general pattern for experimentation which we have already seen Lewin advocate.)

Since they wrote the chapter in 1968, they note that a number of criticisms of experimental social psychology have been advanced. These criticisms have led to three distinct modes of experiment being evident at present: 1) Where the traditional style has been maintained, the experimental treatments have been made weaker, the kinds of deception used becoming more trivial for example; 2) judgement experiments are in use where the subject is asked to recognize, recall, classify, or evaluate stimulus material presented by the experimenter. The emphasis here is on the subject as observer, with little direct impact on them, and with the main concern being controlled and systematic variations in the stimulus environment; 3) field experiments, which have

the advantage of direct impact on subjects in a highly involving situation, but which involve a loss of control over the stimulus environment and over the assignment of subjects to treatment conditions (p 442/443).

Manstead and Semin also suggest that "there are grounds for questioning the extent to which experimental studies provide unambiguous evidence about causation" (p 66), which is of course the stance taken in this thesis.

[10] E.g., Purdue and Gurtman, p 201, in this issue of the journal.

[11] Whatever this means! It was not discussed, and clearly cannot be a controlled variable in the necessary sense for a controlled experiment to be performed.

[12] Pliner and Chaiken's study involved a second experiment, which involved administering questionnaires designed to determine what social motives are important in a social encounter involving eating, and how activation of each of the social motives would affect eating behaviour. The rationale for this experiment is basically the same as in the first, with the independent variable again being the sex of the "eating partner", with reference to whom the subject completed the questionnaire.

## CHAPTER SEVEN

In the preceding chapters it has been shown that social psychologists up to the present day have felt that Kurt Lewin's work in the 1930s and 1940s was of immense importance in the genesis and development of an experimental approach to the subject matter of social psychology. It was also shown that Lewin's importance came from the manner in which he legitimated the use of experiments as part of a system leading to the formulation of general laws. His endorsement of the experimental method was part of his wider concern to place psychology on a scientific basis, which he argued for by championing Galilean as opposed to Aristotelian concepts in science, and by his insistence that by using "topological" concepts in psychology, experimental manipulations are possible, which provide an insight into the genotypical processes that shape our behaviour.

Having shown through ad hoc criticisms that there are reasons to doubt the adequacy of his methodological and theoretical stance towards psychology for the task he has set it, a more thorough argument will now be developed, which will show the errors in Lewin's approach to psychology and which will therefore serve as a criticism of modern social psychology. These errors will be exposed in a discussion of the thoughts of several writers, which will then be related to Lewin primarily, and then by extension, to social psychology.

It is useful to set about developing such an argument by referring to a paper by Daniel N. Robinson, entitled

"Science, Psychology, and Explanation: Synonyms or Antonyms?" [1]. In the course of his discussion, Robinson suggests that, "With all due respect to Kurt Lewin, situations are not akin to magnetic fields" (p 69). The paper was written for the book A Century of Psychology as Science (eds. Koch and Leary, 1985), which was itself written as a celebration of the "hundredth anniversary" of scientific psychology. The editors saw it both as an assessment of history, the history of psychology as a science, and also as a registration of thought in progress in psychology, thought which will itself become part of the history of psychology. Most of the papers included in the book therefore looked both backwards and forwards, assessing psychology's history from their own perspective, and looking to the future, expressing their hopes and wishes for psychology and its development. Robinson's paper certainly has this structure, and although he mentions Lewin only once, in the above manner, his assessment of psychology's past is relevant to an assessment of Lewin's work.

Let us consider one of Robinson's ideas now, as a way of setting the scene for the discussion of Lewin and social psychology to come, and as something that should be kept in mind whilst assessing psychology's claims to a scientific understanding of behaviour. Robinson notes that it is only in the present century, especially in the past few decades, that psychology has been accepted as a natural science. Even in the early years of the Twentieth Century, there were very grave doubts about even the possibility of a scientific psychology (p 60).



The transition from scepticism to confidence has, therefore, been almost sudden. And it is just this sort of shift in perspective that historians of ideas find summoning since it has not arisen from revolutionary discoveries or original and compelling theories. It cannot, for example, be explained the way one would now account for the adoption of Galileo's physics at the expense of Aristotle's or the abandonment of Lamarkian theory in the wake of Mendelian genetics. Unlike these historical swings of allegiance, the newly acquired standing of psychology is largely an extrascientific phenomenon inexpressible in the language of discovery. (p 60/61)

Robinson seems to mean that the acceptance of psychology as a science has been achieved without making any major breakthroughs, which are accepted by most people as being such. There have been no discoveries in psychology which show its emergence as a science by their certainty, nor have psychological theories greatly increased in explanatory power over the years. He hints that psychology's standing is "extrascientific", that although it is seen as a science it has not really achieved, or cannot be shown by logical means to have achieved, that status (whatever "science" is held to be). There are some phenomena that psychology studies that are amenable to a "scientific" treatment, giving the impression that psychology as a whole is a science, whereas in reality, many of the more fundamental psychological phenomena are unsuitable for scientific explanation, even though they have been treated in that way in the past. The "discoveries" that have been made about such phenomena using the scientific method are not cumulative, nor can they be shown to be governed by fundamental laws in the same way as other phenomena amenable to the scientific method are.

However, because of the success of the scientific method in some of the other areas which are identified as psychological, the findings made about these phenomena are afforded scientific status, despite their failure to meet the criteria expected if they were "scientific".

This claim will be examined in more detail later, but we can see immediately that Robinson does not feel that Lewin's optimism about a transformation in the concepts psychology uses, leading to a better, more scientific psychology, is justified. Furthermore, and on the basis of his analysis of the "extrascientific phenomenon" mentioned above, Robinson suggests that his own analysis:

...Discounts the possibility of an ideographic science, at least as this noun is understood within the physical and biological disciplines. What is proposed here is not the means by which some "new" science can be brought to bear upon ideographic topics, but the application of tried and true nonscientific methods of analysis to those psychological problems that are nomothetically inexplicable. (p 73)

Robinson is using the terms "ideographic" and "nomothetic" in the same way as Allport (1937) used them [2] when he contrasted ideographic and nomothetic approaches to psychology. Allport in turn reports (p 22) that the philosopher Windelband proposed to separate the nomothetic from the ideographic disciplines [3]. Nomothetic disciplines only look for general laws and use only those procedures which the natural sciences use. On the other hand, ideographic disciplines, such as history, biography, and literature, try to understand some particular event in nature

or society. Allport suggests that psychology had in the main been "striving to make of itself a completely nomothetic discipline" (p 22), or in other words, a scientific one.

Allport mentions Windelband because he was discussing the proper way in which to study personality, and the person, general laws and the scientific procedure as it has been applied to the individual. Typically, he says, in scientific study a scientist isolates a segment of behaviour from the person being studied, observes the recurrence of this behaviour in many members of a hypothetical class, finds uniformity in the event, and makes a generalization, or law, and then submits this law to repeated tests to establish it by empirical verification (Allport: p 4).

However, Allport was not satisfied with this kind of scientific method for the study of individuality.

The discovery of a law by this kind of procedure is like finding a single thread running from individual nature to individual nature, visible only through the magical spectacles of a special, theoretic attitude. In everyday life, the scientist, like anyone else, deals effectively with his fellow men only by recognizing that their peculiar natures are not adequately represented in his discovery. The single functions which they have in common are deeply overshadowed by the individual use to which they put these functions. The piling of law upon law does not in the slightest degree account for the pattern of individuality which each human being enfolds. The person who is a unique and never-repeated phenomenon evades the traditional scientific approach at every step. In fact, the more science advances, the less do its discoveries resemble the individual life with its patent continuities, mobility, and reciprocal penetration of functions. (p 4/5)

Having said this, Allport does not rule out that "most serviceable technique of the exact sciences" (p 20), the experiment, in studies of personality. He reports that experiments have been satisfactorily completed dealing with complex behaviours such as gait and handwriting, intuitive (non-analytical) judgements, and many more unorthodox subjects (p 20). Even so, there are some aspects of individuality which completely elude the experimental method, such as embarrassment, remorse, falling in love, or religious ecstasy. Therefore, he suggests that psychology is not, and should not be, exclusively experimental in its methodology.

A related question is whether a science treating individuals will be able to evolve laws. Allport suggests that as laws are ordinarily understood, it will not. If a law is a statement of an invariable association common to an entire class of objects, then "individuality is the asymptote of the science that seeks laws" (p 21). He expresses this idea by saying that laws can be seen as the common ground upon which all individual minds meet, but when individuals are considered all laws are modified, making the idea of a general law seem far-fetched.

This is where Windelband's proposal of separating the nomothetic disciplines, studying general principles, and the ideographic, studying the individual case, fits into Allport's discussion. He rejects Windelband's distinction, preferring to regard the two methods as overlapping and contributing to one another (p 21). He argues that in medicine, for example, diagnosis and therapy are ideographic

procedures, even though they rest upon knowledge of the common factors in disease, determined by the nomothetic sciences of bacteriology and biochemistry. Similarly, although biography is ideographic, the best biographies contain a blend of generalizations with individual portraiture. As he says, "a complete study of the individual will embrace both approaches" (p 22).

Robinson, however, goes further than Allport, by discounting the possibility of an ideographic science of psychology altogether. In arguing for this position, Robinson is arguing that general laws of the sort found in the natural sciences are simply not applicable to much of what is supposed to be explained by psychology. By taking this stand we see that he is again at variance with Lewin's views, the "centre of gravity" in most of Lewin's investigations being the discovery of general laws.

The notion that nomothetic explanations are inappropriate for psychology is an important one, and it is the foundation of an argument against the methodology used by social psychology today. In what follows, this notion will be explored, using Robinson's paper as a starting point from which to build an argument drawing upon other philosophers' work. The argument will suggest that the Lewinian assumptions used by social psychology make its claims of acquiring knowledge about human behaviour empty.

Before discussing Robinson's paper more fully, it

seems worthwhile to discuss Lewin's work in terms of the response it drew from his contemporaries. This exercise is valuable because it shows that, firstly, from its very early days, Lewin's work was seen as being logically and methodologically untrustworthy, and also, interestingly, misleading, and secondly, it suggests that the success Lewin's work has enjoyed as the prime mover in social psychology is based in large part on extrascientific factors, in the same way that Robinson contends that the whole of psychology has been based on such factors. It will be shown that the three aspects of Lewin's work that have been emphasized; the transition from Aristotelian to Galilean concepts, involving the use of genotypical rather than phenotypical constructs; the "dynamics" based theory which Lewin generated by this translation; and the experimental evidence for the theory; are all open to severe objections, and have been thoroughly discredited, and indeed were discredited at the time they were published. Finally, an explanation of how such a discredited body of work could be so influential in the genesis of what we see as modern social psychology will be attempted briefly.

Although it is in the nature of the "game" for new theories, or new ideas, books or even movies, to receive mixed "reviews", some enthusiastic and some less so, it is useful to discuss some of the more damning contemporary assessments of Lewin's research, because they give a balance in the first place to the enthusiasm with which his ideas seem to have been taken up by other and subsequent psychologists, and also because the criticisms which were

made at the time seem to be worth rehearsing again, to provide a "fresh angle" of approach to modern social psychology [4]. Finally, if we accept that the criticisms made at the time Lewin was writing were well founded, it is worth conjecturing, along with Robinson, that the success of Lewin's approach, and his later influence, rests partly on "extrascientific" factors. The papers which will be referred to show the main flaws in Lewin's theory, and they did so at a time, the late 1930s and in the 1940s, when Lewin was at the peak of his powers, and at the time when his influence on social psychology was just beginning.

The review of Lewin's Principles of Topological Psychology by C.R. Brolyer, which appeared in 1937 [5], is a good place to start the analysis of Lewin's standing amongst some of his contemporaries, because it is very early, and thus it evaluated the basic principles of Lewin's thinking, which were elaborated on later. Additionally, it reads as though Brolyer went out of his way to be as fair as he could to Lewin. Brolyer tried hard to be positive about the book, saying that he was favourably impressed by the way Lewin drew certain analogies between psychology and topology, and by the keenness of Lewin's insight into human behaviour. He also congratulates Lewin for his emphasis on the necessity of discovering basal laws or principles of psychology, but that was really as far as his praise went. When he began to discuss the detail of the book, the rest of the review served as a dissection of what he saw as being Lewin's mistakes in theorizing and investigation.

Brolyer makes several criticisms, but they are mainly based on two themes; that Lewin has produced interesting analogies but little else; and that having created the analogy he then proceeds to translate familiar explanations into explanations in terms of topology, using the analogy as a justification for making the translation.

Brolyer begins by defining what topology is and does. Topology is:

A highly abstract and practically completely non-metrical branch of mathematics which concerns itself with the kinds of figures that can be drawn on different surfaces, and with those characteristics of the figures that remain unchanged by continuous transformations of the surfaces. (p 257)

Brolyer viewed studying topology as an excellent training for students in procedures of reasoning. He could think of no applications though, other than by analogy, to the metrical disciplines, which are concerned with measurements, and are therefore the antithesis of topology, which deals with the transformations of surfaces, but not however, by measuring them.

On the other hand Brolyer characterizes Lewin's work as plotting behaviour on a life space, which is non-physical, separating this life space into regions according to the psychological characteristics of various situations, and discussing these regions from the point of view of overlap, boundary properties, and the paths from one region to



another. In terms of Lewin's work, this characterization is accurate enough for Brolyer's purpose, as it gives a clear indication of the main sweep of Lewin's theory, and it emphasizes the importance of his theoretical regions in that overall theory.

Having shown what topology is, and what Lewin's work involves, Brolyer then assesses the connection between the two.

[Lewin] has done little more than draw entertaining analogies between the concepts of topology and those of psychology...Such analogies, however, may have decidedly deleterious influences, in that the semi-educated are given a jargon and a set of symbols by which they can fool themselves just as easily as they mystify others. And this, the reviewer thinks, will be the main effect of the book. (p 257) [6]

It might not be clear just how serious Brolyer's criticism could be for Lewin's "topological" theory. After all, to defend himself against this very charge, that his theory was not really topological, Lewin said that the precise requirements of his theory had yet to be met, and that much work needed to be done to show exactly how topology could be used as a set of constructs for psychology to base its theories on.

However, while admitting that topology cannot be applied straightforwardly to the phenomena psychology studies, Lewin stresses that using topology is the way to help psychology progress [7]. Brolyer's criticism though, revolves around the status of topology within Lewin's system.

In effect, his criticism is that there is no connection between topology as it was understood in mathematics, and what Lewin called "topology" in his writings on psychology.

By making this criticism, Brolyer hoped to undermine confidence in the usefulness of Lewin's concepts by showing them to lack support. When Brolyer claims that Lewin has drawn an analogy between topological concepts and psychological concepts, and that this analogy can have deleterious consequences, he is making, amongst others, two further claims; that the analogy between the two is not close enough to support the use Lewin has in mind for it; and that therefore Lewin cannot claim a species of scientific respectability from the use of mathematical concepts. Therefore, when Lewin says that psychology needs topology to help it to progress, Brolyer can reply that even if this is true, Lewin is not really using topology, and that whatever it is that he does use, it is only a re-description of events in a terminology which, through its misleading claim to be based on a mathematical theory, and its appeal as a system of easily applied jargon concepts, is decidedly unreliable as an indication of the rigorous standards required for an inquiry to be scientific.

Brolyer gives an example from Lewin's work to illustrate his argument. A child is playing and does not want to stop and come indoors to wash its hands. Its mother knows that it enjoys wetting the wash cloth in the basin, and asks the child if it wants to wet the cloth, or if it wants her to do it. Because the child wants to wet the cloth it lets

itself be washed without any more trouble. Lewin draws the following conclusion from this interchange:

The question has sufficed to transfer the child from the play situation to the washing situation and it begins to behave according to the requirements of the new region. (Principles: p 98)

Brolyer on the other hand remarks:

The competent nursemaid can distract his attention just as easily as she can transfer him from one topological region to another. The two concepts are equally vague, equally valid or invalid, and equally metaphorical. (p 258)

Essentially what has taken place "seems to be primarily a simple translation from a familiar terminology to a topological one" (Brolyer: p 258). Lewin suggests that a transfer has been made from one region of the life space to another. The nursemaid or the mother would say that they have distracted the child's attention, and that the child is now thinking about something else. Lewin's conception of the life space is of what the individual is perceiving at the time, the different regions being, loosely, different objects or possibilities which are in the person's idea of their life or environment. To say that a change in region has taken place is only to say that the person sees themselves or their environment in a different way. In ordinary terms we might say that what a person does depends upon how they see the things around them. Lewin translates this common sense idea into his terminology in the following manner:

In short, the dynamic conception of a person depends in almost every respect directly upon his position in a certain region. Methodologically therefore in almost every psychological problem one should give first place to the question of the region in which a person is at a given moment, or what change of position is occurring. (Principles: p 99)

In this respect, Lewin's translation of ordinary terms into topological terminology seems to be straightforward enough, even if a little pointless. As we noted earlier in the discussion of experiments testing for tension systems, making this translation is a step which is designed to treat the person and their actions in a different way to that found in everyday life, but it is one which may not add anything to our understanding, simply being a re-statement in different terms, causal and mechanical, of ordinary notions. However, it can be misleading in that the analogical nature of the formulation can all too easily be forgotten, which seems to be Brolyer's point.

It may be that Lewin's enthusiasm for the particular kinds of re-description that he made, for it seems clear that Brolyer is correct in his assessment of Lewin's early terminology as being simply descriptive, combined with his enthusiasm for what he saw as being the correct scientific way to handle the concepts involved in these re-descriptions, led to his belief that he was providing a deeper understanding of behaviour and the motivations for behaviour than any at the disposal of non-psychologists, even though he is certainly not "semi-educated" as Brolyer puts it.

In Brolyer's review this point is made very clearly. Discussing Lewin's famous  $B=f(PE)$  formula, Brolyer points out that Lewin has really said nothing more than that behaviour is the interaction of an individual and an environment, "a fact that has been known for many generations and in many terminologies" (p 258). As Brolyer points out, the symbols used in Lewin's equation "do not constitute a formula except in a highly misleading definition of a formula" (p 258). They do have the appearance of a formula though, and it is this appearance which lends itself so well to the self-deception and mystification which Brolyer sees as being the main consequence of Lewin's work.

The very simplicity of the  $B=f(PE)$  equation is part of its attraction. It seems obvious that Lewin was genuinely impressed by the advances physics had made when using Galilean modes of thought and the aid of experiments, and so it is quite possible that he thought his equation was an equally "scientific" one, along the same lines as those of Einstein or Kepler. That it was not possible to solve it as it stood must have been equally obvious to him, but he thought that by using topology and other methods borrowed from the natural sciences and mathematics it could be done. He must have thought that the analogies upon which he based his assumptions were more than just analogies; that they gave in some way a clearer picture of the reality of the world and of the psychological aspects of that world than that possessed by non-psychologists, but it is this belief that Brolyer and others challenge.

As at least partial evidence that Lewin thought that he was doing more than just presenting analogies, there is a letter which he wrote to Koehler, which can be found in the preface to Principles of Topological Psychology (1936):

I remember the moment when - more than ten years ago - it occurred to me that the figures on the blackboard which were to illustrate some problems for a group in psychology might after all be not merely illustrations but representations of real concepts.  
(p VII)

For the present it is enough to conclude that Brolyer's criticism of Lewin is correct; that Lewin has produced interesting analogies between psychology and topology, but that he has done little else. He has certainly not produced convincing reasons to take the analogy seriously, but he has nevertheless used it to justify a translation of ordinary concepts into a quasi-topological terminology, and has based many of his assumptions on what he sees as the similarity between concepts appropriate in physics, mathematics, and psychology.

This translation produces a topological map of the subject's psychological field, this map lending itself to the idea that the influences on a person's behaviour can be isolated one from the other, and manipulated by experiment to show the causal connections between them. Not only this, it also implies that in similar situations other people's motivations may be similar, and thus the factors which influence behaviour can be generalized across contexts and subjects to provide basic explanations of behaviour, which

can be modified in given circumstances to provide individual explanations. As a consequence of this position, it follows that if a behaviour is desired, it should be possible to achieve it by changing the subject's position from one region to another. Techniques for producing this movement can be discovered experimentally, for example, in the case of the child not wanting to wash its hands.

If, however, there is no proof that Lewin's translation is based on anything more than an analogy, and he gives no real reason to suppose that it is, then what follows from this translation need not be taken as anything more revealing than an entertaining new way of looking at things, and even then, a way which inevitably distorts what it discovers.

If we are asking whether Lewin's conceptual apparatus gives a clear and deep understanding of the psychological world, it seems valuable to spend some time addressing the concepts he used on their own terms so to speak, to discover whether they stand up to critical scrutiny. Since many of the concepts in question were either borrowed or influenced by the natural sciences, one way of assessing them is through a comparison of the uses they were put to in science and in topological psychology. Ivan D. London in his paper "Psychologists' Misuse of the Auxiliary Concepts of Physics and Mathematics" (1944) does just this, arguing that Lewin consistently misuses concepts from other disciplines, with the consequence that "a mathematically unified field-theory for all of psychology along Lewin's pattern is without

significant value and...generally impossible" (p 290).

London suggests that a number of what he calls "auxiliary" concepts are used in every science. For example, in physics, the theorist builds a science from concepts such as the atom, force, energy, and matter. These kinds of concepts can be applied to the description and understanding of observations other than those the concepts were originally constructed to explain. However, as London says, it has often been pointed out that "in a foreign context these concepts may be fatally distorted and lose point" (p 266) [8]. Thus, the value that the concepts originally had does not always transfer to their new use.

London examines Lewin's psychology in detail, a psychology which he believed was "foremost in the use of physical concepts applied to the solution of problems outside of their explicit domain" (p 266), and therefore a suitable place to examine the use of concepts when removed from their natural environment. London discusses the nature of force, Aristotelian and Galilean modes of thought, the nature of field, and aspects of law formation, in what is a long and interesting paper. We will begin by concentrating on what he has to say about topology, and the use Lewin makes in his psychology of the mathematical concepts of topology, with the aim of further assessing Lewin's basic assumptions.

The central premiss in London's discussion of topology is clear; Lewin has "employed an individual variety



of mathematics which has so few points of resemblance to the original that it is not even mathematical, let alone topological" (p 285).

In a discussion of Boolean algebra, London identifies topology as concerning itself with the properties of sets of points. In mathematical topology sets can be multiplied and added, which is apparently very important, yet Lewin does not make use of these properties which is, according to London, "A characteristic indication...of the fragmentary and circumscribed use to which topology is put by Lewin. (p 285) Similarly, a "property of duality" exists which again Lewin makes no use of whatsoever "despite its obvious importance for the derivation of new formulae" (p 286).

Furthermore, London suggests that there are five fundamental concepts in topology, these being derived set, closure, open set, closed set, and convergent sequence. He says though, that it is not really necessary for him to define precisely what these concepts mean, it is enough to note, and to state categorically, that they do not exist in Lewin's formulations. To reinforce his point, London discusses "Abstract Topological Spaces and Deductions", "Topology and the Part-Whole Relationship", and "Coordination of 'Topological Psychology' to Topology", and always comes up with the same answer, that there is a "complete difference between the topology of mathematics and that of Lewin" (p 288).

For example, London reports that Lewin does not

utilize one single theorem of topology. Instead, he makes endless use of a few definitions ripped out of their proper context. He illustrates this point by referring to Lewin's 1940 paper "Formalization and Progress in Psychology", where Lewin makes the following assertion:

If one coordinates certain physical processes to certain geometrical entities one can make certain physical predictions. Such a fruitfulness of coordinating certain physical processes to entities of one rather than another kind of geometry is all that one can mean by saying that a certain type of geometry holds or does not hold for the physical space. Exactly the same procedure is followed if certain psychological processes (such as social locomotion) are coordinated to certain entities of topological or hodological geometry (such as path). There can be no other meaning and no other proof of the applicability of these geometries to psychology than the fruitfulness of predictions based on such coordinations. (p 22)

Using definitions such as this one, without reference to theorems which combine to produce a topology, means that "it is futile to insist that any closely articulated system has been achieved" (p 287). As London says, Newton applied the body of the calculus in evolving his system of mechanics, not just some of the roots of the theory. Using the body allows a system to develop; using some of the roots does not.

Having shown that from this point of view, Lewin's topology is topology in name only, London goes on to ask whether it could have a value divorced from the spurious mathematical basis it claims. For the sake of argument, he disregards the fact that Lewin's topological psychology is,

by these standards, an invention of his own, and that it has merely borrowed the terminology of topology from mathematics, without any of the content, and asks whether it can serve a useful purpose in psychology. His answer is that the utility of "topological" representations in the science of psychology is open to real question.

To illustrate his conclusion, London asks himself what would have happened to a science such as chemistry if it had adopted a topological methodology at a stage in its development comparable to that of psychology. Accordingly, he pictures an example; how the behaviour of inanimate objects in a field threatening combustion might be treated if conceptualized in the same way as the behaviour of human beings subject to the influence of psychological forces.

To provide an example of Lewin's technique, he cites a case Lewin discusses where an adult induces a child to eat an unwanted food by bringing them into the "eating situation" [9].

Lewin argues that if a particular food is not wanted, the otherwise unified action of eating usually breaks up into a series of separate acts such as putting the hand on the table (h), taking the spoon (sp), putting the food on the spoon (f), bringing the spoon halfway to the mouth (hw), bringing it to the mouth (m), taking the food into the mouth (i), chewing (ch), and swallowing (sw). These steps correspond topologically to a series of regions.

If the adult wants the child to eat when the child does not, one way to achieve this is to bring the child (c) step by step through the regions closer to the region of "real eating", in other words, chewing and swallowing. When doing this the child usually resists more, "in accordance with the fact that with approach to the undesired action the repulsive forces (represented by arrows) increase" (p 97).

When the food is actually in its mouth the child is likely to eat it whether it wanted to or not. To explain eating what was originally not wanted, Lewin uses the concepts of regions and forces, as follows:

One can show in detail that this change of behaviour is brought about essentially by the fact that as the child enters the region of "real eating" his position and the direction of the field forces are entirely changed. When the child is in one of the preceding regions, for instance, when he takes the spoon halfway to his mouth, then a region of greater unpleasantness into which the adult tries to push him, still lies ahead. The adult may therefore have to exert great pressure to induce the child to make a locomotion into the disagreeable region. When the child is once within this region of real eating then the region which lies ahead of him is a more pleasant one of relative freedom (fig. 16b). The child therefore often prefers a locomotion in this direction to spitting out, which is a locomotion in the direction of a disagreeable fight with the adult. (p 98)

Lewin suggests that the same process is operative when social groups accept positions they find themselves in which they had previously opposed, and is the reason why the "fait accompli" is so dreaded in politics. When a change in position has actually occurred, "the group will accept its new position without resistance" (p 98) [10].

To illustrate Lewin's technique as it might work in a natural science, London considers the conceptual representation of setting fire to a piece of cardboard whose kindling point is comparatively high. To achieve such a representation, London suggests drawing a figure [11].

In this figure, A, B, F, G, and H are regions representing activities of various kinds; D is a major barrier to be overcome; C and E are force vectors oppositely directed. Given this representation, A could be the flaring up of a struck match, B the application of the match to the cardboard, C the natural tendency of the flame to elevate the temperature of the material in the direction of eventual combustion, D the kindling point of the cardboard which must be reached and overcome, F the chemical activity pre-scientifically identified with the process of combustion, the enclosed sub-regions conceptually being the generation of heat, changes in colour, etc., G the consummation of the series of activities in the direction of the inevitable ashes, and H the cooling residue.

From this conceptualization London draws the conclusion that all that has been achieved is a spatial restatement of what was already known verbally (p 290). Previously recognized facts have been translated into the unfamiliar and complicated language of a pseudo-mathematics. He therefore concludes that chemistry could never have advanced using such methods, presumably because it gives no

insights into the processes at work in combustion, the chemical changes and reaction etc. He suggests that a parallel conclusion is indicated when discussing topological psychology:

A mathematically unified field-theory for all of psychology along Lewin's pattern is without significant value and, furthermore, in view of the sharply divergent stand of modern physics, generally impossible. (p 290)

What is more, he suggests that:

The claim of "topological" psychology to deductive rigour is mathematically spurious and results from an unfortunate misapplication of a very generalized branch of mathematics totally unrelated to the needs of psychological theory. (p 290)

We must now ask ourselves about the usefulness and intelligibility of the analysis London has made of Lewin's concepts for our own, philosophical purposes. We should note firstly that all London has shown is that Lewin has not used his concepts in accordance with the strict uses they are put to in mathematics, or in the way physics uses them. This in itself does not show that they are inapplicable to psychology, despite what London says to the contrary. Unless we know what the needs of psychological theory are, it is impossible to say that just because Lewin has used concepts in a way that would not be acceptable in another discipline, that they are automatically worthless. Showing, as London has, that Lewin's conceptualizing is a bastardization of the concepts he has chosen, could only show either that he was a bad scholar, or that he was being disingenuous in their

usage, and seeking to deceive his readers into thinking that he had substantial mathematical, and therefore logical, backing for his theories and assertions, when in fact he had none, and knew that he had none. A third possibility, that his inventive and speculative nature led him to use an analogical system of explanation which he then took to be revelatory of the actual truth of things, seems to be perhaps the most likely explanation of his theorizing. As we have seen, the idea struck him that the diagrams and concepts he was using could possibly be more than mere analogies. When this thought occurred to him he began studying topology, as it seemed to him that topology was particularly well suited to the specific problems of psychology as he saw them (Preface: VII).

In 1912, when he was a student, he defended the thesis that psychology would be forced to use the concept of time and also of space in order to deal with manifolds of coexisting facts, a thesis which ran counter to a fully accepted philosophical dictum. Using topology was a way of dealing with the spatial concepts he thought that psychology needed.

He found though, that after attempting to use the more complicated concepts of topology, it became "both more sufficient and more fruitful to refer to the most simple topological concepts only" (VII). This might explain why many of the mathematical concepts London mentions are not present in topological psychology.

On the other hand, even though his criticism has not necessarily shaken the foundations of Lewinian theory in quite the way that he had hoped it would, London shows quite clearly that Lewin's assertion that Galilean concepts in physics had replaced Aristotelian is simply false.

The division of the concepts of physics into these two categories...is arbitrary, confusing, and misleading, because the present situation in physics in no way suggests a sharp antithesis between the two, but rather implies that the two are essentially complementary. (p 284)

Physics appeared at that time to work in both Galilean and Aristotelian ways. For instance, quantum theory is Aristotelian in that it deals with statistical measures, but it is Galilean in that it is concerned with constructs and not entities, in other words, with genotypes and not phenotypes. London repeats that the picture Lewin sets up of physics as having undergone a transformation falsifies the reality of what was actually going on in physics.

London implicitly acknowledges the reason for Lewin's advocacy of the "transition" theory in physics though, when he says that the importance of Galileo to modern science lies mainly in his insistence on experiment. It was this insistence, he says, that laid the foundations of a new methodology (p 284). Accepting this new methodology gave the physical sciences a fresh impetus after the sterility of what had passed before as science. In the Galilean scheme, experimentation was to be the sole arbiter of facts, and



hypotheses were to be modified in the light of the facts, but London suggests that physics has moved past the reliance on this method, and has advanced in a seemingly Aristotelian manner. He quotes Eddington [12] as suggesting that there is nothing in the whole system of the laws of physics that could not be deduced unambiguously from epistemological considerations alone, without any recourse to physical hypotheses as such (p 284) [13]. Such a system is "from a certain standpoint unadulterated Aristotelianism" (p 284), and London emphasizes again that developments of this sort cannot be adequately described in terms of Lewin's dichotomy of scientific conceptions.

This assertion raises issues we have considered earlier. On the one hand, it reminds us that Lewin has only used the analogy of the Aristotelian/Galilean transformation to show that the same transformation should take place in psychology; on the other hand, the assertion is not a death blow to Lewin's theorizing because showing that Lewin has either been mistaken or disingenuous about the transition in the first place, does not necessarily entail that the conceptualization he has been arguing for has no place in psychology, or is especially inappropriate to it. Such a proof has to be obtained independently of making the charge that he has misapplied and distorted concepts. However, the charges London makes serve to highlight the fact that Lewin's work rests heavily on the use of analogy, both in its metatheoretical, and in its more detailed, and lower level, theoretical assumptions.

At this level, London amplifies the criticism that Lewin's theorizing is just a re-statement of readily known facts in a technical sounding jargon. The other criticisms he makes, which relate to most of Lewin's concepts, not just his use of topology, illustrate how Lewin's contribution to psychology provoked a fierce reaction from some quarters, which nonetheless, it was able to withstand. In London's case, this was probably due to his failure to engage with any of the real challenges Lewin's work offers. To say that he misuses concepts is rather like "name-calling" if no reason is given to suggest how the modified concepts are inappropriate for the new task they have been set. A more telling criticism would have to show how the concepts Lewin uses are inappropriate for the study of people's behaviour, and this is what will now be attempted.

Lewin appears to have used a series of assumptions in the creation of his conceptual framework. He asserts that in a transformation analogous to that which took place in physics, psychology should adopt Galilean, and therefore experimental, type principles. Similarly, psychology should be unified in terms of one fundamental concept, the dynamic principle of explanation, which was an essential part of the Galilean conception of science, and also of the Gestalt psychology which had influenced him so much. Given that his assumptions utilized dynamic explanations, the next step was to conceptualize these dynamic concepts in terms of fields of forces, which are differentiated and variable, and which again are used in an analogous fashion in physics.

What is interesting is the way in which these assumptions and analogies have seemingly followed on naturally from one another. When Lewin first attempted to provide a scientific basis for the study of behaviour, all of these particular analogies did not strike him at once, but having started on his investigation the number of assumptions that had to be employed to keep the system consistent seems to have grown, until the position noted above is reached, where most of the concepts he uses have been drawn from other sciences and applied to the study of human beings. This seemingly inevitable progression can lead to situations in the theoretical landscape where the assumptions used can seem less than appropriate to the new area of study, and it seems possible that in Lewin's case one area is that of causality, where the pertinence of the assumptions he has used seems to grate on the reader's sense of appropriateness.

#### CHAPTER SEVEN: FOOTNOTES

[1] Robinson is a well respected historian of psychology, whose books include An Intellectual History of Psychology (1976), Systems of Modern Psychology (1979), and as editor, the prestigious twenty eight volume Significant Contributions to the History of Psychology (1977-1978).

[2] G. W. Allport, Personality: A Psychological Interpretation. New York: Holt 1937.

[3] W. Windelband, Geschichte und Naturwissenschaft. 3rd edn., 1904.

[4] "There were unfavourable reviews by Brolyer in 1936-1937, Heidbreder in 1937 and Garrett in 1939. Additional reviews - some favourable, others unfavourable - appeared more frequently in the 1940s. But during the same period many of Lewin's concepts - such as vector, valence, life space, field theory, and tension system - became indispensable parts of psychology" (Marrow: p 80).

[5] C.R. Brolyer, "Review of Lewin's Principles of

Psychology". Character and Personality, 5, 1936-37, pp 257-258.

[6] Compare this with Wittgenstein's remark about Freud: "Freud has many intelligent reasons for saying what he says, great imagination and colossal prejudice, and prejudice which is likely to mislead people." Lectures and Conversations: p 26.

[7] Garrett (1939) notes that in Principles of Topological Psychology and also in A Dynamic Theory of Personality, Lewin contends that topology provides the basis for a truly scientific psychology (p 518).

Garrett, and also London, takes Lewin's statement of intent very seriously, and sets about showing at more length than Brolyer how Lewin is not using topology as it is known in mathematics, and also that the topology Lewin does use is no more than a re-description of everyday events in a different terminology.

[8] This is of course a consideration well understood in "ordinary language" philosophy, as Wittgenstein and Austin point out, and it is a phenomenon which Cavell, in The Claim of Reason (1979) calls non-natural projection.

[9] This example is found immediately before the example Brolyer discusses, of the child washing its hands, in Principles: p 97/98.

[10] This example also illustrates Lewin's contention that his theory is appropriate for all scales of groups, the genotypical process underlying behaviour being more important than the size of group to which it is applied. Once we see the connection between a child being fed something it dislikes, and a group acquiescing to a decision which is forced upon it, both events can be explained in the same way. Thus, three of the difficulties he envisaged in his 1939 paper, "Field Theory and Experiment in Social Psychology", which we have previously mentioned, namely handling problems relating to groups as well as to individuals, and the handling of all sizes of objects and patterns, as well as finding a way to bring large scale patterns into a framework small enough for the technical possibilities of experimentation, can be overcome simply by arguing that that the same process is going on in all cases, and that therefore understanding the process in one area will allow understanding in the others.

[11] Figure 4, p 290, in London, "Psychologist's Misuse of The Auxiliary Concepts of Physics and Mathematics" (1944). See appendix 1.

[12] Sir Arthur Eddington (1882-1944) was an eminent theoretical astronomer and "an original thinker of the first rank" (Stebbing, Philosophy and the Physicists: p 6) who was also a talented popularizer of science.

[13] A.S. Eddington, Relativity Theory of Protons and Electrons (1936), and The Philosophy of Physical Science (1939).

## CHAPTER EIGHT

As noted earlier, Lewin's conception of science meant that explanations were mainly to be given in terms of "systematic" rather than "historic" constructs, which is to say that he was interested in the determinants of behaviour at a given time, and only at that time, rather than in why the determinants existed. As we have seen Jones (1985) note, this conception does not emphasize the person as a collection of propensities developing over time, and in altering its emphasis to the forces operating at a given moment, it seems to neglect part of what we would ordinarily say was important in a consideration of human behaviour, namely who the people are, and where they have been. Emphasizing that people are the product of a long developmental history highlights the distinctiveness and uniqueness of their responses to a common environment, and thus would require explanations to be given with reference to these histories. The consequence of Lewin's contrary explanatory stance is a confused and distorting account of causation as applied to people.

Discussing causation is a way of suggesting that as a whole, Lewin's system is inadequate, because it leads to distortions when dealing with people, and not just re-descriptions. As has already been shown, much of Lewin's use of topology and various other concepts can be seen as a re-statement of ordinary explanations and notions in terms of a quasi-technical terminology. In itself, this re-statement is not necessarily worthy of criticism. In fact, there might even be a gain in precision from using these new concepts,

which could be valuable, and which could help to resolve some of the puzzles of human social behaviour [1].

However, Lewin and modern social psychologists would claim that they are not just re-stating what is already known, but that they are providing a deeper understanding of human behaviour, which is shown by their discovery of causal relationships in social behaviour. Therefore, although the topological reformulation of concepts is not a problem in itself, it provides a rationale for the division of a situation into variables, which in conjunction with the Galilean conception of science, appears to allow experimentation, and therefore the delineation of causal relationships, to take place. The use of causal concepts is an essential part of both Lewin's and social psychology's explanatory systems, and of course of science as a whole. There seem to be problems in using causal concepts to explain human behaviour though, and if these problems are serious, they can serve to show that Lewin's analysis of behaviour is inadequate, and inappropriate.

A useful way of addressing this issue is to consider the work of Hart and Honore, in their book Causation In The Law (1959), where they discuss the notion of "causation" from the lawyer's point of view, a view which they say is, and should be, based on the common sense notion of cause, as opposed to the scientific notions used by psychology for example.

They assert that "the plain man's causal notions function as species of basic model in the light of which the courts see the issues before them" (p 1). As part of their attempt to clarify the use of causal language in the law they made a preliminary analysis of the causal concepts which pervade ordinary thought, identifying their analysis as a philosophical one. Calling their investigation philosophical is clearly an implicit reference to the "ordinary language" school of philosophy which was considered pre-eminent at the time. Their work is clearly influenced by thinkers such as J.L. Austin, and Wittgenstein, although they do not acknowledge the ancestry of their approach explicitly. They do however, distinguish their methods from previous philosophical discussions of causation, which they say seemed totally irrelevant to the lawyer's problems. It is their treatment of these philosophical doctrines that is relevant to our purposes in this thesis, as well as their analysis of common sense notions of causation.

To show what the common sense notion of causation entails, they first consider a philosophical view which had been influential, and then contrast this view with what they believe to be the common use and understanding of terms such as "cause", "effect", "consequence", and so on. They argue that the philosophical approach to causation, which has traditionally been concerned with general causal statements, has been largely unhelpful to the lawyer, or to the historian, who deals with particular causal statements, which are based on common sense notions, and we will discuss this philosophical approach shortly.



To set the scene for their argument, they point out that European philosophy since Hume has been dominated by the doctrine that generalizations or laws constitute the essence of causation (p 4), and they link this fact with the enterprise of the physical sciences, whose job it is to discover these laws. On this account, even singular causal statements, apparently confined to the connection between two particular occurrences, are in fact general, in as much as their causal character is derivative and lies wholly in the fact that these particular events actually exemplify some generalization asserting that the kinds or classes of events are invariably connected. According to this Humean view we can only say with any certainty that something had a cause, and to show that cause, on the assumption that this connection between events was an example of a more general law.

Hart and Honore note that this analysis creates difficulties for the lawyer and historian, whose business it is to make particular causal statements. When generalizations are used to identify the cause of a particular event on a particular occasion, it is not clear whether on this particular occasion something can be said to be the cause of something else, or only a "mere condition" or "part of the circumstances" in which it occurs (p 10). They cite as an example of this difficulty the case of a fire breaking out where the conditions are such that it appears the presence of oxygen is, in one case, a mere condition, and in another

case, the cause (p 10).

Oxygen can be a mere condition of a fire when someone is walking in the woods and drops a lighted cigarette, in which case the cigarette would be the cause of the fire and the oxygen a mere condition. We can imagine a case where oxygen would be the cause of a fire though; for example, in a chemical reaction where it has been deliberately excluded. If it accidentally enters, oxygen can be said to be the cause of the resulting fire. [2]

As Hart and Honore point out, although there are certainly general laws operating in both examples which we may need to use to demonstrate causal connexion, these laws do not tell us that in one case oxygen can be sensibly considered a cause, and in the other not. Our choice of cause responds to the varying contexts of the particular occasion in question, which is a complication not easily dealt with by the scientific notions of cause.

In the ordinary use of causal concepts on the other hand, Hart and Honore claim (p 2) that three general concepts are latent, suggesting that the language the lawyer and historian uses very frequently draws its force and meaning from these concepts. The central concept "is that of a contingency, usually a human intervention, which initiates a series of physical changes, which exemplify general connexions between types of events", for example, when someone manipulates things in order to change a given state of affairs. Secondly, they suggest that in ordinary causal

language there is a conception of someone providing another person with a reason for doing something, however this is done, which can count as a cause; and thirdly, there is the idea of "the provision of an opportunity, commonly exploited for good or ill" which is often seen as the cause of an event. These concepts are important for the lawyer and the historian, as they are both concerned with situations where interventions change the course of events, where people persuade others to do things in one way or another, and where people's acts or negligence provide an opportunity for others to do something otherwise impossible.

Hart and Honore argue that philosophers have contributed little to understanding what is meant by causation in the above cases, even though philosophy has helped understand causation in the natural sciences. They argue that this is because the lawyer and the historian are concerned to make causal statements about particulars, "to establish that on some particular occasion some particular occurrence was the effect or consequence of some other particular occurrence" (p 8/9). On the other hand:

In the experimental sciences, by which so much of the philosophical discussion of causation has been influenced, the focus of attention is the discovery of generalizations and the construction of theories.  
(p 9)

Because science and philosophy have this characteristic interest in the concept of causation, they have been of little use to the lawyer and the historian,

whose interests are so different. For instance, the lawyer is most often interested in the causes of events such as a man's death. "The man's death was caused by this blow" is a singular statement about a causal relationship. Although the death is a particular event, the causal statement uses a generalization, namely, that blows can cause death, applying it to that particular event. Hart and Honore suggest that in this and other respects, the causal statements of the lawyer and the ordinary person are very similar, in that they both identify particular events as causes of other events within the complexity of a real situation, often using generalizations which are "already known or accepted as true and even platitudinous" (p 9) to allow them to do so. These generalizations are of a different kind to the generalizations made by science though, as we will see.

However, when Lewin was attempting to place his psychology on a scientific basis, it was obvious that he should choose a scientific account of causality to explain events in human behaviour. His preference for "Galilean" concepts and his emphasis on experimentation in psychology, means that to remain consistent he must use notions of causality which are similar to those used in the natural sciences, whose concepts and attitudes he was hoping to transplant to the study of human behaviour. His choice of causal concepts also follows from the kind of questions he asks. As we have seen, he was mainly interested in finding general laws as part of his acceptance of the scientific method, and given this aim it is obvious that he would use concepts of causality suited to his ambition.

To show the similarity between Lewin's methods and those of the philosopher or scientist, we will consider what Hart and Honore say about philosophical notions of cause and common sense notions in more detail. It is sufficient for our purposes to be able to show this similarity because it will indicate that Lewin's conception of causality is a consequence of the system he uses, is an essential part of it, and that therefore if the notions he uses are found to be wanting in any way the system cannot be modified to accommodate improvements, but must be deemed to be inadequate in its entirety.

To exemplify the philosophical treatment of causal concepts Hart and Honore chose to discuss J.S. Mill's ideas because he is one of the few philosophers to whom legal writers constantly refer in explaining the idea of causation (p 11). By showing the dissimilarity between his work and ordinary concepts of causation, they attempted to prove their assertion that philosophical ideas had little relevance to lawyers.

They suggest that Mill's analysis of causation as it relates to physical events has four main aspects (p 20) [3]. The central notion involved in Mill's analysis is of course the invariable sequence of events in nature, with the corollary we have already seen, that particular causal statements must be defended by proof of the relevant generalizations asserting such invariable sequence. Although

this conception is similar to Hume's insistence that constant conjunction or regular sequence between events is the essence of the notion of causation, Mill's statement is logical in form rather than psychological. Hart and Honore say that the notions Hume insisted on, the constant conjunction and then the determination of the mind to pass from one object to its usual attendant, have been replaced in Mill's analysis by:

The doctrine that every singular causal statement implies, by its very meaning, a general proposition asserting a universal connexion between kinds of events: To make such a singular causal statement is therefore to claim that the events which it relates are instances of such a universal connexion between types of events. (p 13/14)

It is apparent that Lewin's conception of individual events and their relation to laws or generalizations, as expressed in "Aristotelian and Galilean Modes of Thought", follows Mill's analysis. The conception provides, as we have seen, a justification for using experiments to determine underlying genotypical tendencies relating events by logically connecting the singular with the general.

Secondly, according to Hart and Honore, Mill's analysis shows that causal sequences in nature are not simple chains of events, but that "the antecedents of such invariable sequences are complex sets of conditions which may include not only events but persistent states and negative conditions" (p 20). When we investigate nature, we do not find simple events followed by other simple events. Nature is too complex, containing as it does conditions other than simple events.

Connected with this is Mill's third theme, the distinction between a "philosophical" or "scientific" notion of cause, and the "common sense" notion. According to science, only the whole set of conditions jointly sufficient for the production of an effect is the cause, whereas common sense characteristically selects or singles out one of these conditions as "the" cause. Again, Lewin's adherence to the scientific conception of causation is evident. This conception asserts the importance of the whole situation to understanding an event, and as we have seen, Lewin used a type of topology to represent situations, and thereby explain behaviour, thus emphasizing the importance of knowing the "reality" of a situation rather than relying on "appearances".

Finally, Mill recognizes that both scientific and common sense notions agree that the same event can have different causes on different occasions, since there is the possibility that there may be several independent sets of sufficient conditions for an event of a given type.

Hart and Honore contrast Mill's analysis with what they give as a common sense notion of causation, to show the difficulties it presents as a tool for lawyers and historians. They argue first that Mill's definition of cause in terms of invariable sequence simply does not correspond to the way causal concepts are actually used (p 20), and secondly, that Mill's analysis is inadequate to cope with cases where:

The lawyer, the historian and the ordinary man use the language of "cause" and "consequence" to designate certain relationships between human actions. For the analysis of this use of causal language an altogether different model from that presented by Mill is required. (p 21)

This second contrast that Hart and Honore make will prove to be very important in assessing the adequacy of Lewin's conceptions of causality, relating as it does to human actions.

Hart and Honore show the dissimilarity between ordinary and philosophical notions of cause by firstly pointing out that Mill's definition of cause does not correspond to actual usage because the relevant generalizations which would have to be made to justify a causal account in a particular case are not, and cannot in practice be, statements identifying conditions invariably followed by events of a given kind. This requires too precise a definition of events and consequences, and the relations between them. In common sense causal statements the generalizations given are broader, less specific, and sometimes "quite platitudinous" (p 21), being based on people's experiences of events and consequences. Hart and Honore argue that this difference is a result of the nature of common sense enquiry. In ordinary life the word "cause" is used in cases where "effects" are produced by human actions, but it is also used whenever an explanation is sought of an occurrence about which we are puzzled because we do not know why it occurred. In such "explanatory causal statements",



common sense usually distinguishes between "causes" and "conditions" even though there are reasons to suppose, as Mill has suggested, that strictly speaking, cause and conditions are equally necessary if the effect is to follow. A person's dropped cigarette would be deemed to be the cause of a fire, this being an example of an effect caused by a human action. Similarly, the sun's rays can be the cause of a fire when there is a drought, this case being an example of an explanatory causal statement.

The second important causal concept which Hart and Honore draw from ordinary thought, they call the domain of "interpersonal transactions" (p 48). This "domain" is concerned with cases where one person is said to have caused another to act in a particular way, or where someone induces another person to do something, and they argue that it needs to be analyzed using a different model from the one used by Mill. The most salient feature of causal relationships of this kind is that they do not rely on "regular connexion" or sequence in the way that causal relationships between physical events do. Thus, generalizations are less important in supporting statements of cause.

Hart and Honore argue that there are four common features of this kind of relationship (p 49/50); 1) The second actor knows of and understands the significance of what the first actor has said or done; 2) the first actor's words are part of the second actor's reasons for acting; 3) the second actor forms the intention to do the act in

question only after the first actor's intervention; 4) except where the first actor has merely advised the second actor, they intend the second actor to perform the act in question.

Although it can be quite acceptable language to say that in such cases one person "caused" the other to act in a certain way, it must be remembered that to say a person performed an action because of someone else "carries no implication or covert assertion that if the circumstances were repeated the same action would follow" (p 52). To defend this type of statement by saying for example, that generally a person complies with requests when being threatened, does not show a necessary causal connexion between a threat and an action. To say that the person was "forced" to act in a certain way is not equivalent to saying that they were in a force field, or a magnetic field, and were, like a ball bearing in such a field, forced to move in a certain direction. If the person were in a similar situation again the possibility exists for them to act differently. This possibility does not exist for the ball bearing.

For example, they discuss a situation where a person is asked to show that there was a connection between threats they had received and their actions.

It would be absurd to call upon him to show that there really was this connexion between the threats and his actions, by showing that generally he or other persons complied when threats were made. This general statement might be quite untrue and yet his statement of his reasons might be true. (p 52)

Where no dimension of reasons is involved in the performance of an action, evidence from experience, and thus from generalizations, is normally enough for an explanation. If we say a blow has caused a bruise, no statement of reasons for acting is involved, and therefore "evidence of what experience has shown to be generally the case would be required to show that this was a case of *propter hoc* not *post hoc*" (p 52). Where the agent's reasons are involved in the action however, which it is assumed that in interpersonal transactions they are, general knowledge does not have the same explanatory power.

The agent's own declarations about their reasons for acting have a special primacy of importance, since they are the only people who can have first-hand knowledge of these reasons. However, generalizations can be used as evidence when discussing interpersonal transactions, to suggest that a person was not speaking the truth about their reasons, either because it was "out of character", or rare for anyone to act for those reasons. Generalizations of this sort can be formed on the basis of experience, but the experience upon which they are based is of many cases where it is known that an individual had reasons for acting in the way that they did. This knowledge is independent of any generalizations, Hart and Honore suggest, being based on individual cases or reasons. Singular causal statements about physical events rely on generalizations in a different way. Instances from which causal generalizations are constructed are not recognized apart from such generalizations as cases of causal connexion, but only as cases of succession between events.

Thus, to say that two physical events are in a causal relationship is to assert that such a generalization does exist, whereas generalizations in the case of people's reasons are later than, and independent of, the assertion that the individual has a reason for acting.

However, Hart and Honore suggest that we usually recognize as a reason for an action something which is relevant to the promotion of some purpose known to be pursued by human beings. Therefore, the concept of reasons implies that people respond to certain situations within recognizable limits, such as fleeing from danger, conforming to social rules etc. (p 53).

What they seem to mean here is that we must understand how the action promotes some known good, to be able to say that a person acted for a reason. The presupposition of this broad similarity in human behaviour, without which they say we could not have the concept of a reason for action, does not mean that if we assert that a person acted for a reason, the statement will entail that they will act the same way again. In understanding actions, all that is required is that we must understand why the person acted as they did, ie., if the action was voluntary it would normally be to promote some objective analogous in some way to those which human beings are known to pursue by actions.

They append a warning to this principle though

(p 55), that a question requiring reasons in its answer is not always the best way to approach a puzzling situation involving interpersonal transactions. There are many behaviours where it would be inappropriate to speak of the actor as having a reason for doing them. If a person suddenly appears, making another jump in surprise, the sudden appearance of this person would properly be mentioned in reply to the question "Why did you jump?" as well as to the question "What made you jump?" even though it is not a reason for jumping in the same way as that involved in interpersonal transactions. "In this non-purposive aspect these cases resemble causal connexions between two physical events." (p 55) There are of course behaviours which it is difficult to categorize as being one way or the other; as being done for reasons that we can share, promoting some objective analogous to those which people are known to pursue; or as being caused. If someone were eating a textbook in order to understand what was written in it, we might not accept what they gave as their reason for eating the book as a "proper" reason. Although these strange examples exist, they do not show that the ordinary distinction between reasoned actions and caused actions is invalid. They show rather that further distinctions may need to be made, which require thought and empathy in their application, and which show the rich variety of human behaviour.

From this brief discussion of Hart and Honore's analysis of causal concepts in ordinary usage we can begin to see why Lewin's conception of the causes of human behaviour seems intuitively to be so skewed or inappropriate. As Hart

and Honore have shown, our ordinary concepts of causation are at odds with what science and philosophy tell us are the correct ways to employ these concepts. Firstly we single out aspects of the situation leading up to an event and award them the title "cause", when in fact, according to philosophy and science, we should consider the entire situation, or at least all of its relevant aspects, as being the cause. Lewin sides with the scientific and philosophical approach by stressing the importance of the entire situation in his explanation of events, and this is one of the first areas where he parts company with what ordinary language users would consider important, and therefore where what he says about human behaviour seems strangely unsuitable to his subject matter.

Secondly, ordinary people are often interested in explanatory causal statements, relating to particular events, whereas philosophy and science have been concerned with more general causal investigations. Thus, to be told that someone died because of oxygen deprivation can be rather annoying when you really want to know whether they had been shot or stabbed. Similarly, to be told that somebody completed a task because of the strength of an intra-psychic tension system can be quite irritating, as can being told that someone ate ten crackers because of an intra-psychic impression management system. Thirdly, and this is related to the second inappropriateness, people generally think that other people did things because they had reasons for doing so, however bad or commendable or changeable those reasons might be. As Hart

and Honore point out, it is acceptable to say that some things people do have causes rather than reasons, but still, in most cases the majority of people would say that where actions were voluntary, such as finishing jobs or eating snacks, people have reasons for what they do, and that suggestions to the contrary are somewhat out of place. In showing these distinctions, Hart and Honore highlight the contrast with the single, causal, explanation of behaviour that Lewin is attempting.

The above discussion offers some suggestions as to why Lewin's analysis of the backgrounds and the causes of people's actions seems so peculiar when compared with the ordinary way of thinking about these behaviours. We now need to discuss whether or not this peculiarity has any bearing on an analysis of Lewin's system as a whole.

Firstly, one of the points that Hart and Honore make should be emphasized. In their discussion of what can be considered normal and abnormal conditions relating to events taking place, they note that the context of an enquiry will have an effect on the way in which causes are differentiated from mere conditions in a given case. When there are different points of view about an event, there can be different views of its cause. For example, they mention that the cause of a famine could be identified in more than one way. A peasant might recognize the cause as being a drought, whereas the World Food Authority might call the Government of the country's failure to build up sufficient reserves of food

the cause of the famine, and the drought a mere condition (p 33). The suggestion being made is that the differences in the identification of cause stem from the different outlooks and assumptions each viewpoint brings to the situation, and the questions each ask. Thus, a particular effect may be seen to be caused in different ways by different people depending on their assumptions.

If this is the case, then the scientific explanation Lewin gives of the causes of behaviour could just be a different way of viewing the same action from that given by an ordinary explanation, and as suggested earlier, it could be one which sheds valuable light onto the subject.

An enthusiast of Lewin's position, when faced with the charge that Lewin's theories cannot be correct because they are so obviously "scientific", and therefore run counter to what most people would see as being relevant explanations of behaviour, might therefore refer to a position which is discussed in Ryle's Dilemmas (1954). In "The World of Science and Everyday Life" Ryle tries to show that the conclusions of physicists about matter do not conflict with our ordinary judgements about matter; rather, the physicist takes a different interest in matter from the ordinary person.

Ryle shows this by the use of an analogy, using the story of an undergraduate who discusses the accounts of a college with an auditor. The auditor tells the undergraduate that all aspects of the life of the college are covered in



the accounts. The undergraduate feels however that something has been left out of this analysis of the college, even though all the activities of college life have been covered in the accounts.

Ryle points out that we must be careful about the claim that the accounts cover the whole life of the college. They undeniably do cover it in terms of the monetary transactions that are associated with it, but when for example, some books are purchased, the accounts are "constitutionally speechless about the literary and scholarly qualities of books which are just what interest the student" (p 78).

This being the case, the student's feeling that the accounts miss something of the life of the college is well founded, but it is not a defect of the accounts, because the accounts are just a way of giving information about the life of the college. The student's way of giving information about the college would differ from the auditor's, but the two are not in competition, one with the other. The description the accounts give is not a description, the correctness of which involves the incorrectness of the student's description. Thus, the two can exist simultaneously, in the same way as a physicist's and a joiner's description of a chair can exist simultaneously.

In the same vein, Ryle talks about a science which went through its adolescence relatively recently; economics.

Economics gave an account of people as being actuated only considerations of gain and loss.

The conduct of his life, or at least of his rational life, was governed by the principles of Supply and Demand, Diminishing Returns, Gresham's Law, and a few others. But Man as thus depicted seemed to be disastrously different from Man as depicted by the preacher, the biographer, the wife or the man himself. Which, then, was the real man and which the dummy-man, the Economic Man or the Everyday Man? (p 69)

There seemed at the time to be a deadly rivalry between what economists said about the motives and policies of people, and what the people said about themselves. Ryle argues that we no longer feel this rivalry with economics, because we realize that there is no incompatibility between an ordinary view of our motives and the view economics takes. He uses the figure of his brother to illustrate this point. The economist is not offering an account or a description of the activities of Ryle's brother, but is doing something quite different. The economist:

...Is offering an account of certain marketing-tendencies, which applies to or covers my brother in so far as he concerns himself in marketing matters. But it does not say that he must or does often or does ever concern himself in such matters. In fact it does not mention him at all. (p 70)

When we realize that Ryle's brother is not "a well-camouflaged Economic Man" (p 70), but that even so, the economist talks about Ryle's brother, because they talk about anyone who makes purchases, invests savings and so on, the conflict between economics and real life ceases to bother us.

Similarly, the apparent conflict between science and the everyday world should not trouble us, because science does not produce descriptions which rival ordinary explanations of the world.

Whatever merits this argument has for the distinction between science and the everyday world or economics and ordinary descriptions, they do not apply to psychology, or at least to social psychology. Psychology does not provide an account of the world around us in different terms from those we would ordinarily use; it provides an account of us in terms other than we would normally use. It tries to explain those aspects of our behaviour which we ordinarily explain, but it explains them in a different way. Social psychology tries to account for the behaviour of Ryle's brother, whatever he is doing, in a way which competes with what Ryle's brother would say about his own behaviour, and also which competes with what we might say about Ryle's brother.

If psychology and social psychology were to claim that the account they give of people's behaviour were simply an alternative view, then there would not necessarily be a problem with this competition. After all, there are often competing explanations of behaviour in ordinary life. People disagree with each other about their own reasons for doing things [4]. Psychology however, claims to use a scientific method to give a deeper, or a more clear, understanding of human behaviour than that given in ordinary explanations. By seeking to replace ordinary explanations, social psychology

is in conflict with those explanations. Social psychology however, with its philosophical and methodological foundations deeply rooted in Lewinian theory, is in no position to win this conflict, because there are flaws in its structure which make its point of view untenable. These flaws are in large part the result of Lewin's overextended use of the scientific mode of enquiry, leading to a great deal of what is important about human behaviour being neglected.

As we have seen, Lewin's choice of the scientific, "Galilean" type of enquiry affected the way in which he accepted a causal explanation as being adequate. His conception meant that he needed to show that "the whole situation" was the cause of a behaviour, to conform to scientific practice, and to make it possible for experimentation to take place. Having accepted this need, his next requisite was to find a way of representing the entire situation, and given the scientific nature of that need, it must have seemed obvious to him that that the way to do this was to use a field of dynamic, interrelated concepts, which was drawn in part from physics, and in part from mathematics. This methodological assumption served two purposes, in that it allowed the situation to be represented, and if conceptualized properly, allowed generalizations and laws to be postulated by its assertion of the general configuration of these concepts. If evidence was found of one of the concepts being evident in a particular case, then it must be applicable to other cases, even if in an altered state due to the variation in forces in operation at a given moment.

The next step was for Lewin to visualize how these concepts affected the individual, and again the analogy with concepts used in the natural sciences seems inevitable. To speak of the different concepts or variables as having "forces" which act on the individual, and moreover, which can be quantified, is just a consequence of the rest of the assumptions Lewin has used. Galileo's analysis of dynamics, and of course other work in physics, expressed in laws such as  $S = 1/2gt^2$ , all rely on the use of forces in their conceptual schemes. What could be more natural then, than for Lewin to apply this concept, and the others related to it such as valence, to his explanations of human behaviour?

Once we have seen that Lewin's system is based around the assumptions he made about psychology as a result of the analogies he saw between psychology, the natural sciences and mathematics, it is easy to see that the first analogy he drew, between the change in physics created by Galileo, and the change which should take place in psychology, influenced his approach to psychology to such an extent that his psychology can be said to be permeated throughout by the concepts flowing from this analogy. As we have seen, social psychology appears to be permeated to a similar extent by Lewin's own theoretical and methodological assumptions.

Of course the use of analogies in intellectual enquiry is not new, and it is often a source of very fruitful insights. Arthur Koestler, in The Sleepwalkers (1959) [5],

discusses Johannes Kepler, one of the most influential figures in the development of astronomy. One day Kepler was drawing a figure on the blackboard as an illustration for his class "when an idea struck him with such force that he felt he was holding the key to the secret of creation in his hand" (p 249). This discovery determined the course of his life, and remained his main inspiration throughout it.

His discovery was about the universe, or at least the solar system as we now know it. While a student, he had heard of Copernicus' revolutionary idea, that the Sun was at the centre of the universe, and not the Earth, as had previously been taught. Having decided that this solar centred (heliocentric) conception of the universe truly represented reality, Kepler began to wonder why there were just six planets (that were known at the time) and not a hundred or any other number, and also why they were spaced in the way they were, travelling with the velocities that they did. Thus, says Koestler, he started his quest for the laws of planetary motion.

At first his labours bore no fruit, but on the fateful day, 9 July, 1595 (he noted the date), when he was teaching a class, the truth struck him. He had drawn, roughly, a triangle fitted between two circles, the outer circle circumscribed around the triangle, the inner circle inscribed into it.

As he looked at the two circles, it suddenly struck him that their ratios were the same as those of the orbits of Saturn and Jupiter. The rest of the

inspiration came in a flash. Saturn and Jupiter are the "first" (ie. the two outermost) planets, and "the triangle is the first figure in geometry. Immediately I tried to inscribe into the next interval between Jupiter and Mars a sphere, between Mars and Earth a pentagon". (Koestler: p 251)

Although encouraged by the results he obtained, this insight did not quite work out. There were just too many inconsistencies between the model he used and the data he was trying to fit into it. However, eventually realizing that space is three and not two dimensional, he tried a different approach. It is possible to construct just five regular solids in three dimensional space. These "perfect" solids (also called "Pythagorean" or "Platonic" solids) are symmetrical and have faces which are all identical. Being symmetrical means that they can be inscribed into a sphere, so that all of the corners of the solid lie on the surface of the sphere, and they can also be circumscribed around a sphere, so that the sphere touches each face in its centre.

To Kepler's way of thinking, there existed only five perfect solids, and five intervals between the planets, and as Koestler says, it was impossible for him to believe that this should occur by chance, and not by divine arrangement (p 252). It provided the complete answer to the question "Why should there be just six planets and not a hundred?" as well as answering why the distances between the planets were as they were. They had to be that way so that the five solids could be fitted into the intervals, like an invisible skeleton or frame.

This was a prodigious explanation. It showed why the universe was as it was, and it did so in a mathematical way, which was important given that the mathematization of nature was just beginning. Koestler says that:

That unforgettable moment before the blackboard carried the same inner conviction as Archimedes' Eureka or Newton's flash of insight about the falling apple. (p 254)

In fact, the only problem with Kepler's insight was that it was completely wrong.

The reasoning behind his assumption, such as it was, was that since God could only create a perfect world and since only five perfect symmetrical solids exist, "they are obviously meant to be placed between the six planetary orbits 'where they fit in perfectly'" (Koestler: p 256). There are only two problems with this deduction. The first is that the solids do not really fit, and the second is that there are at least nine planets (not including the asteroid belt) and not six as Kepler thought. The other three planets were not discovered until after Kepler's death, so he could be excused that mistake, but he could not really be excused of knowing that the solids could not be made to fit the orbits of the planets.

Although he had confidently asserted in the preface to his book Mysterium Cosmographicum (1596) that the perfect solids could be made to fit the orbits of the planets, Kepler had trouble with the orbits of Mercury and Jupiter. To



circumvent these problems he did two things. Regarding Jupiter, he said that it was no wonder that it could not be made to fit, considering the great distance it was away from the Earth. As for Mercury, Koestler says quite simply that he cheated, by inscribing Mercury's sphere not into the faces of the octohedron, as ought to be done, but into the square formed by the four median edges (Koestler: p 586).

What is even more interesting about Kepler's ideas of astronomy is that even though they were completely and utterly wrong, they formed the basis of his important and correct work. They did so in ways which were accidental, and Koestler notes that Kepler himself saw this. Kepler said later in his life:

The roads by which men arrive at their insights into celestial matters seem to me almost as worthy of wonder as those matters in themselves. (Astronomia Nova, summary of Cap. 45, cited in Koestler: p 263)

Even with his appreciation of the paradoxical nature of his discoveries, Kepler still maintained his belief in the five perfect solids. Koestler says that the belief showed "all the symptoms of a paranoid delusion, and yet it functioned as the vigor motrix, the spur of his immortal achievements" (p 254) which include his three laws of planetary motion, "the pillars on which Newton built the modern universe" (p 260).

Social psychology however, has no such pillars upon which to build a conception of the human. Even though there

is some agreement as to the starting point for social psychology, and even this agreement is by no means general, no one has been able to build an inspiring edifice upon the foundations which Lewin is meant to have provided.

#### CHAPTER EIGHT: FOOTNOTES

[1] See for example, Bertrand Russell's The Problems of Philosophy (1912), for the idea that what is taken for granted in ordinary life can often be misleading.

[2] Similarly, when a child is being fed by an adult, as in Lewin's example, it could be said that there are occasions when the adult is a mere condition of the child eating, when it is a normal day, and the child is hungry and likes the food on offer, and also occasions when the adult causes the child to eat the food, when it does not want to.

[3] They cite J.S. Mill, A System of Logic Ratiocinative and Inductive (8th edn), London, 1886 as their reference for these ideas.

[4] For example, Cioffi, in "Intention and Interpretation in Criticism" (1964), suggests that people can even be quite correct in disagreeing with authors about the intentions underlying their work.

[5] The Sleepwalkers is a discussion of the division which gradually became established between the sciences and the humanities in intellectual pursuits.

## CHAPTER NINE

This failure to build a satisfactory social psychology expresses itself in the use psychologists make of an argument which could be called the "youth excuse". Robinson, in the paper mentioned earlier, points out that psychology as a whole has taken the form of an experimental science, but that this has not led to it making any of the impressive findings which were hoped for by the founders of scientific psychology.

Twentieth century psychology continues to search for a paradigm. It seems that the old insecurities have been retained by the discipline throughout these recent decades of wide and enthusiastic approval from society and from even the scientific community. As it tells the world it is a science, it tells itself it is not, at least not now - soon perhaps, but not yet. (p 62)

As has been noted earlier, there has been much debate both within the whole of psychology, and also within social psychology, as to whether there is a general paradigm which serves to define activities within the field. It has been argued previously that whether or not there is a clearly defined paradigm, there is certainly a set of prescriptions operating in a large part of social psychology, and that these prescriptions are basically Lewinian in that they stress experimentation, and a "scientific" method.

Even with such prescriptions though, there seems to be the feeling, as Robinson has pointed out, that psychology has not lived up to its promise, or to its promises, yet. This is seen clearly in the psychologists' cry, as above,

that psychology is only a young science, even though it presents itself as a fully fledged science to the outside world. Whilst posturing as an established science, it still maintains a stance towards itself which is somewhat equivocal. Evidence of this uncertainty can be found in the recommendations of historical understanding which have been taken very seriously in social psychology. Graumann (1988) points out:

It has almost become a ritual to refer to the development of what today is called psychology with a quotation from Ebbinghaus (1908, p 1): "Psychology has a long past but only a brief history." Social psychologists have repeatedly applied this to their own discipline. (p 5)

Social psychologists usually make this claim for two reasons. Firstly, they justify their interest in the forms of behaviour they study, by showing that people have always been interested in the questions they are asking, showing that "psychology" has a long past. They generally cite philosophers in this part of their claim; Plato, Aristotle, Hobbes and Marx are favourites. Secondly, they use psychology's short history to excuse the poor progress they have made in solving the problems they see themselves as addressing.

The ritual of referring to psychology's brief history is evident in many types of psychology and social psychology. Rather than quoting numerous examples to prove this point, it is more informative to note a few of the more interesting examples of the youth excuse. For instance, Koehler, in

Gestalt Psychology (1929) is an early example of a psychologist excusing the findings psychology had made, devoting an entire chapter to the subject of the status of "Psychology as a Young Science". Incidentally, it is this chapter that Wittgenstein refers to when he makes his remark about psychology as a young science (Philosophical Investigations: p 232). He says that:

The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problems and method pass one another by. [1]

The idea that method and problem pass one another by will be discussed in more detail later, as it is an important way of illustrating what is wrong with Lewinian and social psychological thinking.

Koehler was writing in the early part of the century, not long after psychology had begun to develop as a field of inquiry distinct from philosophy, so it is easy to excuse his caution in trying to suggest that psychology as a discipline was not fully developed. In 1965 though, Deutsch and Krauss also stress social psychology's youth. In Theories in Social Psychology, they suggest that, at that time, there were four main theoretical perspectives in social psychology. The "Gestalt" orientation, the "Reinforcement" paradigm, the "Role" orientation, and the "Psychoanalytic" orientation [2].

Deutsch and Krauss stress that these orientations are

not theories in the strict sense of being deductive systems from which clear, testable hypotheses can be derived. Instead they express general perspectives towards social psychology and they suggest types of variable which ought to be taken into account by it.

The major theoretical approaches in social psychology are the products of its infancy. It is characteristic of a science in its early stages to develop theories that are ambitiously inclusive and vague in respect of their details. (p 5)

In the concluding comment to their first chapter "The role of Theory in Social Psychology" they repeat and re-emphasize their claim:

Social psychology in its infancy. It has only begun to identify a distinctive subject matter relating to human interaction. Being in its infancy, it is still largely dominated by theoretical approaches that are based on implicit conceptions of the nature of man. None of these is sufficiently explicit in its psychological assumption, in its mode of logical inference, nor in its empirical referents to permit unambiguous testing of its implications. In short, none of these orientations is a "theory" in the sense of theories in the physical sciences. (p 12/13)

Deutsch and Krauss therefore recognize that although social psychology was trying to cast itself in the mould of the physical sciences, it had not been able to do so. Their implicit assertion though, is that when social psychology develops past its infancy (and into its adolescence?) it will have identified its distinctive subject matter, and so will be able to forge ahead in the same way as the physical sciences it tries to emulate.

Theories in Social Psychology was written a quarter of a century ago, but there does not seem to have been much progress towards a universally acceptable social psychology since then. For example, Cartwright (1979) [3] notes that:

In view of the inherently complex nature of our subject matter and the youthfulness of our discipline, I find it remarkable that so much progress has been made. I do not, of course, believe that all is well or that we can be content with what has been accomplished. For social psychology, as we know it today, does have deficiencies and does face some very difficult problems. (p 87)

Social psychology is in an early stage of its development and has not had time to solve all of its problems. Such defects as the susceptibility to fads and fashions, the obsession with technique, the reliance on a single method of research, and the disproportionate emphasis on cognition and other temporally proximate determinants of behaviour are, I believe, symptoms of immaturity and can be expected to be remedied with the passage of time. (p 92)

Cartwright's paper was written just over a decade ago, but it still shows the two faces of social psychology's self-analysis, a realization that all is not well, combined with an assertion that all will be well if it is allowed to develop through its growing pains.

Even more recently, Graumann makes the point that there is a difference of opinion amongst social psychologists about how to bring about the discipline's "coming of age" (p 17). Some feel that society should be brought into the laboratory (eg. Jaspars, 1980: p 426) while others think that it should venture "into the field of social forces outside the laboratory" (p 17). He concludes that perhaps since

originally social psychology had a wider scope and agenda but narrowed down its task to gain scientific acceptance, by employing experimental methods, the best thing that it could do would be to address, or to re-address, real social issues, to gain acceptance as a social science (p 18).

This is an interesting assertion in more ways than one. Firstly, Graumann obviously feels that social psychology has not yet "come of age" and that a change is needed if it is to do so. Also, in noting that one possibility is for social psychology to venture into the "field of social forces" outside the laboratory, he shows how ingrained Lewin's terminology has become in discussions of social behaviour, and therefore how easily taken for granted. Finally, the idea that social psychology should move away from the experimental method, and move towards addressing real social issues is one which Robinson would be quick to agree with, as we will see.

It is worth stressing that the dissatisfactions that have been voiced about social psychology above have all come from the mouths of social psychologists (except Koehler, whose interest was most famously in perception and insight). These criticisms have not been made by outsiders, but by workers in the field who are concerned about the progress social psychology is making. Nor are these criticisms unusual. They seem to be part of the continual criticism which social psychology reserves for itself, this criticism being expressed most obviously in the crisis of confidence in the late 1960s and 1970s, and also at present in the work of



people such as Harre [4], and Parker [5], who aim to replace social psychology as they see it presently operating, with alternative conceptions of the study of the interaction between individual and society.

Such sustained self-criticism could be indicative of a discipline which lacks a firm belief in its own worth. In their protestations of youth, and their critical analyses of their discipline, social psychologists seem to be expressing their awareness of the paradoxical nature of their methods of inquiry, an inquiry which promises more than it can deliver, using the methods of the exact sciences to study phenomena which appear to resist quantification and measurement.

It was of course Lewin who provided a large part of the rationale for the use of scientific methods in social psychology. It is interesting to note the parallels which seem to exist between Lewin's psychology, and Kepler's astronomy.

Lewin too had an "idee fixe", which came in part from a "flash" of inspiration while drawing on a blackboard. He too created a theory which was later recognised to be too far fetched to be of any practical use, but which led to important consequences, in Lewin's case, the use of experiments in social psychology. As we have seen, Brown has said that the details of Lewin's theory are of no concern, since it has attracted little systematic research, but that it has had an enormous impact on the field. As has been

contended, this impact was in providing a rationale for experimentation, and so it could be argued that Lewin's work, like that of Kepler, outgrew the absurdity of its genesis. However, this is not the case in social psychology, rather, experimentation is linked so inextricably with Lewinian theory that it must stand or fall with it. What has in fact happened is that social psychology has accepted the possibility of being an experimental science, without first exploring the implications of this possibility [6].

Robinson shows a line of argument against systems such as Lewin's, and it is worth exploring his position to find how destructive it is to Lewin and therefore to social psychology. We should bear in mind what has been discussed already, that Lewin's method involved taking a scientific stance towards social psychology, involving the use of experimentation, that this stance involves a notion of causation which disagrees with common sense notions of the roots of behaviour, and is a variety of rival explanation, not just an alternative way of giving information about behaviour. Also, Lewin's technical terms seem to be re-descriptions of behaviour, rather than expressions of genotypical processes. These observations alone could lead one to think that no gain in explanatory power is made by employing Lewin's methodology, and that therefore social psychology's claims, of giving a deeper understanding of behaviour, are unfounded, being based on a mere re-description of everyday knowledge which gives the impression of allowing causal, and genotypical, mechanisms to be uncovered. However, this position needs to be supported

from a different angle. Robinson's paper provides a clue as to what this fresh angle of attack should be.

Robinson's suggestion, that the transition from scepticism to confidence about the possibility of a science of psychology was based largely on extra-scientific phenomena, centred around the work of various philosophers who had doubted psychology's adequacy as a science of human behaviour. For example, J.S. Mill rejected the attempt to produce a scientific psychology by the use of introspection by noting that:

The current mental life of an observer was dominated by notions built up over a course of years, and that neither those notions nor the processes by which they were instilled could be unearthed "introspectively". (Robinson: p 61)

Thus, even though Mill was a patron of scientific psychology in his other writings, his empirical associationism, which took it for granted that a person's mental life was built up over the years, demanded more of psychology than psychophysical research and studies of learning or memory could provide.

Robinson locates the cause of the dissatisfaction with even the possibility of a scientific psychology in the widespread philosophical belief that the determinants of psychological processes and outcomes were too numerous and varied to "admit of systematic experimental manipulation" (p 61). Even the great "founder" of scientific psychology,

Wilhelm Wundt, divorced his social psychology from his scientific, experimental psychology, on the grounds that social and transpersonal phenomena could not be deduced in any way from supposed laws of thought.

In the midst of all these doubting philosophers, Kant included, Robinson asks how psychology achieved its scientific status. He suggests that it did so by:

Jettisoning the issues and problems that had traditionally excluded psychology from the pantheon of sciences, while at the same time embracing a different class of problems suited to the methods of the developed sciences. (p 62)

The strategy, or ploy as Robinson hints that it could be called, implying a degree of disingenuity, involves adopting the tools of experimental science, locating problems appropriate to these tools, and then eventually declaring the problems and issues bequeathed by history to be kinds of non-problems, because if they were proper problems the official methods would have been able to solve them (p 62). This analysis certainly seems to fit the procedures of Lewinian theory, although Robinson does not mention Lewin explicitly. In adopting his "scientific" methodology, Lewin suggested that problems relating to the past development of people were no longer a part of what psychology should be interested in, because there was no way that character-forming episodes, for example, could be conceptually related to present behaviour. Having then apparently "successfully" found solutions to the problems psychology can deal with, the interaction of the situation with the individual, Lewin

suggested that there was in fact no need to deal with the issues bequeathed by the individual's life history because past history simply had no bearing on behaviour, except as a minimal contingency leading to the formation of the situation as it is found at any time. As only the present time and the present situation were thought to be important, the past history of the individual was no longer a problem. Therefore, the common sense notion of behaviour being related to past episodes or to future desires was downgraded in importance, and in a sense neglected altogether.

Taking on the form of an experimental science does not solve psychology's problem though, as we have seen from the discussion of the "youth excuse". To provide an insight into the confusions he feels psychology, and social psychology, are troubled by, Robinson addresses the question of how one would know that psychology had become a science, if it succeeded in becoming one. What he hopes to gain from asking this question is an understanding of the kind of inquiry psychology would be if it were to achieve a scientific status [7].

His discussion leads him to argue that psychology should move "back to Aristotle and that most fundamental distinction between scientific and metaphysical explanations: the distinction between causes and reasons" (p 69). This thesis is obviously very challenging to the line Lewin has taken, because Lewin so comprehensively rejects "Aristotelianism" in psychology.

There is, Robinson says, no final word on just what makes a body of facts and methods a science. Alchemy, astrology, and phrenology were all concerned with controlled observations and measurements, as well as with predictions. It is hardly worth pointing out that these disciplines are no longer called sciences, which teaches us, Robinson believes, that quantitative rigour is insufficient to ensure scientific knowledge, and that even fairly reliable correlations, such as those that Gall and Spurzheim turned up in their study of phrenology, are not enough to save a discipline from the conceptual assaults of later and clearer reckonings (p 63).

One of the earliest attempts to "categorize the various modes of human understanding and to establish the nature of scientific understanding within a larger metaphysical context" (p 63), was presented by Aristotle. At the centre of his interest was the concept of causation, which was of course one of the areas in which Galileo disagreed with him. Aristotle was aware of the complexity of causation, and Robinson suggests that part of this complexity comes from the different questions that may be asked about a cause.

For instance, Robinson observes:

One may account for the choral movement of Beethoven's ninth symphony in terms of (1) the composer's financial needs, (2) Schiller's ode, *An die Freude*, (3) the neuromuscular events necessary for vocalization, (4) the spirit of nationalism and brotherhood in the German states following the humiliation at the hands of Napoleon, (5) the black

marks on scored paper indicating the sequence of notes to be sung and played by musicians. And the list could obviously be expanded greatly. (p 63)

By examining the different means by which events could produce others, Aristotle arrived at a set of four causal determinations, the "formal", "material", "efficient", and "final" causes of things. Robinson gives as an example to show the application of these terms, the production of a statue which comes into being because certain "materials" can be worked ("efficiently") into certain "forms" that qualify as statuary. Importantly, the resulting figure depends also on the original design and purpose (telos) of the sculptor, and this design or "end" constitutes Aristotle's "final" cause (p 63).

What is interesting for our analysis of social psychology is Robinson's contention that Aristotle removed final causes from the realm of the natural sciences. The rational principles standing behind those phenomena which are not merely "natural", can only be explained, he thought, by metaphysical analysis.

Science has its own area of application; it provides causal explanations, through the use of general laws, and its explanations are complete to the extent that the phenomena it explains are formed only by nature, which is to say that they have no rational principles.

The model of explanation propounded by Aristotle is rationalistic in that it requires universal laws by which specific (singular) instances become deducible.

It is also empiricistic in that all such instances amenable to scientific study are drawn from the natural (observable) domain. (Robinson: p 64)

Robinson notes that Hempel's "covering law" model of science (1965) is very similar to the Aristotelian model, in that science is held to consist of explanations of natural phenomena framed by a universal (covering) law, such that, given the law, the phenomena become deductively necessary. He characterizes both these conceptions of science as being "deductive-nomological" (p 65). He also points out though that there are other models open to "an aspiring science" like psychology, one of these being Mach's "empiriocriticism" (p 65).

Mach attempted to rid science once and for all of "metaphysics", a task which Robinson suggests had been attempted by Hume and also by J.S. Mill. Mach's implicit argument was that the proper business of science is observation, and that the theoretical side of science can be no more than a formal and systematic re-description of observables. Science cannot rise above the level of orderly descriptions of experience, nor can it yield laws that go beyond the ensembles of perceptual regularities. It was this "ontological purity, this antimetaphysical bias" (p 66) that the Vienna circle was based around, and it was from logical positivism that some psychological thought originally drew for its philosophical basis. Robinson asserts:

It is primarily in the "social" and the "behavioural" sciences that one can still find believers in the possibility of an exclusively observational mode of



enquiry; the possibility of a discipline that never ventures beyond the safe moorings of "objective description". (p 67)

Contemporary psychology in the Machian tradition is taken to be one or another of the versions of behaviourism, especially that defended by B.F. Skinner. However, Robinson has detected recent trends in psychology that show an increasing awareness by psychologists that a genuine science of psychology would have to "outgrow the stage of Baconian descriptivism" and its avoidance of theory (p 68). He suggests that the drift towards cognitivism for example, and away from quasi-Machian approaches reflects a "positive motive to come closer to the universally acknowledged facts of mental life" (p 69), rather than just an impatience with pure description.

The tendency to drift towards cognitivism Robinson mentions is in general psychology. Obviously a similar process was at work much earlier in social psychology, where explanatory theories were used much more readily, and indeed were much more necessary due to the poverty of pure descriptions of social behaviours as explanations of that behaviour. However, Robinson sees the motivation to come closer to the acknowledged facts of human mental life as being the impulse which should urge psychology back to Aristotle, and the fundamental distinction between scientific and metaphysical explanations; the distinction between reasons and causes.

It was Dray (1957) who challenged the applicability

of the deductive-nomological model of causality to historiography on grounds that are largely Aristotelian. Dray argued that in history, explanations take the form of determining the rationale, objectives, and motives of an actor, and that these do not behave like "efficient causes" or purely material antecedents in the natural sciences. Hart and Honore, who mention Dray's book in passing, would clearly agree with the general emphasis of this analysis of the applicability of general laws to history, and therefore also to the law. Robinson draws the moral from Dray's work that some of the phenomena in the sphere of activity psychology is interested in will admit of scientific explanation, while others will not, and that it is the phenomena which should dictate the method, and not vice versa.

This notion is relevant to a discussion of Lewin's work, because it seems to coincide with the common sense view of actions being performed for reasons and not being caused, as against Lewin's conception of systematic causation, so this position will be explored in a little more depth.

#### CHAPTER NINE: FOOTNOTES

[1] In the preface to Wittgenstein's Remarks on the Philosophy of Psychology, Volume 1, (1980), [known as RPP1] the editors, Anscombe and Von Wright, trace the origins of part II of Philosophical Investigations. Wittgenstein dictated a selection of remarks written between May 1946 and May 1949 to a typist, creating the typescripts numbered 229 and 232, which have been published as Remarks on the Philosophy of Psychology (RPP). Anscombe and Von Wright suggest that it is justified to consider the work of this period as preparatory studies for part II of the Investigations, a view endorsed by the number of remarks found in the Investigations virtually unchanged from the two typescripts. There are 70 such examples of correspondences between TS 229 and part II of PI, 31 of them found in section

XI, which deals with aspect seeing etc. The fact that remark 1039 corresponds to the last section of PI, and has no place in section XI seems surprising, until we consider the editors' acknowledgement in PI that they are responsible for the placement of the final fragment in its present position. Von Wright, in Wittgenstein (1982) reveals that this last section of the printed book was found written on a separate sheet in MS 144 (almost identical in content to part II of Investigations) placed between two sheets with consecutive text on them (p 135). It was placed at the end of part II because it was felt that it might form a link between Wittgenstein's remarks on psychology and those on mathematics. This may be true, but it seems to me that the above considerations show that remark 1039 of RPP1 (p 203 PI) is part of the argument dealing with aspect seeing, and also therefore with Wittgenstein's remarks about Koehler and Gestalt psychology, which in the typescripts are found in discussions of aspect seeing.

[2] Deutsch and Krauss are well respected social psychologists, Deutsch being one of Lewin's graduate students at M.I.T. in the 1940s. Deutsch wrote a chapter on "Field Theory" in the second edition of The Handbook of Social Psychology (1968).

Deutsch and Krauss say that the four main theoretical perspectives in social psychology at that time were the "Gestalt" orientation, assuming that people are constructed so as to be principally concerned with developing an organised and meaningful view of the world; the "Reinforcement" paradigm, suggesting behaviour is primarily determined by its consequences for pain and pleasure; the "Role" orientation, suggesting that what is considered pleasurable, and also meaningful, is determined by people's experiences in their "role" in a given society; and the "Psychoanalytic" orientation, in which behaviour is seen as the outcome of a battle between people's animal natures, and society as represented by their families.

[3] Cartwright is also a very well respected social psychologist, who was one of the first members of staff at the M.I.T. Research Center for Group Dynamics, which Lewin set up and supervised.

[4] For example, Harre, R., Social Being: A Theory for Social Psychology (1979).

[5] Parker, I., The Crisis in Social Psychology (1989).

[6] One could even say that social psychology's mistake was "heroic", if it is judged that the Trojan's action in taking in the gift of a great wooden horse without checking its contents can be seen as heroic.

[7] As he says, would a scientific psychology be "Geisteswissenschaft", rather than "Naturwissenschaft", or would it be a member of the community occupied by physics, chemistry, and biology? (p 62)

## CHAPTER TEN

Dray's argument in Laws and Explanations in History concerns the "covering law" model of explanation. The model claims that when an explanation is made, it can only be a proper explanation if what is to be explained can be subsumed under a general law (p 1). Thus, as Galileo, and Lewin, argue, an object's falling to the ground is explained by subsuming the particular event under the general law of gravitation. The law covers all aspects of falling, how fast the object falls, with what trajectory, and so on, covering all instances of all other objects falling too. It is because cases are brought under a law, or covered by a law, that it is called the covering law model. Dray argues that whether or not the covering law model has a use in other fields:

It is a dangerous model for the philosophy of history. For it commonly leads its advocates into talking about explanation in history in ways which are either radically incorrect or misleading in important respects. (p 1)

The covering law model is inappropriate for historical use because history deals with people, and Dray's basic argument is that people are not just very complex forms of essentially similar objects, rather, they are unlike objects, in that each person is unique and has reasons for their actions on many occasions, and thus an explanation which works for one of their actions will not necessarily work for anyone else's actions. This assertion allows Dray to contend that:

When a human action is explained by reference to the principle which it applies, the force of the explanation does not depend upon the truth of the assertion that all men, or even any sub-class of them, apply such a principle in such circumstances. (p 158)

Dray has drawn attention to the difference between explanations which apply empirical laws, and those which apply "principles of action". He does not deny that "reasons for acting" have a kind of generality or universality, as Hart and Honore pointed out, and thus can be likened to empirically validated laws, which are also general or universal. It is quite in order to say that:

If Y is a good reason for A to do X, then Y would be a good reason for anyone sufficiently like A to do X under sufficiently similar circumstances. (p 132)

If Y is "generally" a good reason for doing X, when this generalization is offered as a rational explanation, in Dray's terms, it does not mean that if an instance is found where someone like A did not do X, the general statement that Y is a good reason to do X will be falsified. In cases of this kind the general statement is better seen as a "principle of action". This principle of action can be expressed in the form "when in a situation of type  $C_1 \dots C_n$ , the thing to do is X", without implying that in a situation of that sort X will necessarily be done (p 132). When explaining actions, this is a perfectly reasonable position to take. General explanations in history are not of the same logical type as general explanations in the natural sciences. As Robinson says, the actor's motives etc. do not behave like

"efficient causes" or the purely material antecedents in the realm of natural science (p 69). Therefore, saying that Y is a reason for doing X is logically different from saying that Y is a cause of X.

Hempel, who is one of the covering law theorists Dray discusses at length, responded to Dray's position by suggesting that motives and reasons can be treated as nomologically similar to natural causes if an explanation is made in terms of rational beings doing a certain thing in situations of a certain type. For instance, if person A is in a situation of type C, and A is rational, then to explain A's behaviour it is sufficient to show that in situations of type C, rational beings do X (Robinson: p 69). Of course, this is the same argument which Dray sets aside as being inapplicable to history. There is no necessity involved in situations to allow this type of generalisation to function as an efficient cause.

Robinson raises additional objections to Hempel's suggestion, with the aim of showing how the covering law model is inappropriate for psychology. Firstly he points out that the situations which Hempel mentions can only be explained in rational-historical terms, meaning that situations of a "type" C are only of this type because they are similar to each other in some way. The similarity is essentially defined in terms of the people in them. Within this similarity there can be enormous differences in situations, which nevertheless can be said to be of the same type. Buying something can be said to be a "type" of

situation, but there are any number of ways in which to buy something. The "eating" situation is another example. In fact, talk of situations here is very like talk of regions in Lewin's theory, a kind of "catch-all" category which has no definite meaning.

What is more, talking of situations at all implies that people are involved in them, and that these people have reasons and motives which operate in that situation. This is the juncture at which Robinson suggests that situations are not akin to magnetic fields, and cannot be substituted for those initial conditions that serve as minor premises in Hempel's scheme, meaning that they are not constant, nor objectifiable, because they can have a different meaning for all the people in them, who will therefore react differently to them, a point which will be discussed in due course.

Robinson's second objection is that "many significant psychological states do not have public empirical referents" (p 69). This is an important point because it is part of his contention that only by returning to what we know to be true about people, that they have hopes and feelings which may not necessarily be accompanied by any physical behaviour, can any progress be made in psychology. The example he gives is of the "hope" that it will rain on Friday. If we hope that it will rain, so that an outing is cancelled perhaps, there is not necessarily any behaviour associated with the hope, even though we would recognize any "hoping" behaviour that did occur. Hoping is not necessarily private, but it can be. His

third and final objection is that:

The actual and logical connections between reasons and actions are not the same as those obtaining between causes and effects...there are reasons for actions which never lead to action and some that cannot. (p 69)

He suggests that there is a logical entailment between causes and effects, (how can you have a cause without an effect?) but there is no such logical entailment between reasons and actions. It appears as though I can have reasons for acting, without actually acting. He gives as examples the desire to live forever, "a wish for which even jogging is not an apt behavioural correlate", and the hope that "if there is life on Mars, the laws on Mars must be just" (p 69).

I may have a reason for wanting to live forever, perhaps to be able to read all the books in the library, but this reason can never lead to an action, simply because there is no action which is appropriate to the original desire. Similarly, I may hope that laws on Mars be just because I believe that equality of treatment is a good thing, but this reason for wanting just laws does not lead to action, because again there is no action that is appropriate.

These two cases are examples where reasons cannot lead to actions. There are of course just as many cases where reasons do not lead to actions, even though it is perfectly possible for them to do so. We may have reasons to lose weight or to work harder, but still do neither. So when we say we have a reason for doing something, this does not



necessarily mean that it will be done, but if it is done then we would commonly say that our reason was the cause of what we did. We had a reason to lose weight, or to work harder, so we did [1].

Robinson argues that "Hempel can only assimilate reasons and rational explanations to the deductive-nomological model by taking the behaviourist ontology for granted, and it is just this that Dray rejects" (p 69). He asserts this because he believes that one of the requirements of the deductive-nomological explanation is that all explananda be empirical, and thus "public" in some way. "The only 'public' events routinely taken to be psychological in nature are behavioural (in the wide sense)" (p 69). Robinson disagrees with this position though, suggesting that any behaviour that can be observed is in fact psychologically neutral, because any number of psychological inferences can be drawn about it. He argues that what is genuinely psychological about behaviour is the "cognitive, affective, and volitional antecedents" related to it (p 69). In other words, we are interested in what people are thinking about (in the loose sense) when they are doing something. As such, the proper subject of psychology could be said to be the study of "mental life" [2].

To support his contention that behaviour is psychologically neutral, Robinson suggests that behaviour on its own, as it were, is in some ways like the results of an opinion poll. It is possible to find the most popular opinion

adopted from a range of different opinions, but not the (covert) determinants of this choice of position. Eighty per cent of a sample might oppose homicide, "but from this fact...nothing of explanatory import can be extracted" (p 70). There can be many different reasons why homicide is opposed, and the same can be said about any "piece" of behaviour. On its own, and as such, it has no psychological significance.

To clarify Robinson's position, it is useful to consider a concrete example. Often, he says, what we are interested in when we want to know opinions is why the person holds those opinions, which involves a discussion of reasons in some cases. There are times when we only want to know how many people hold a certain opinion, and this is a perfectly legitimate concern, but equally, there are times when we want to know more than this. On these occasions, when all we have is the opinion, or the behaviour, on its own, it cannot give us the information that we want.

As another example, we could take the Bible story of Abraham, who took his son up to the mountain top to sacrifice him because God told him to do so [3]. As a piece of behaviour, Abraham's movements are psychologically neutral, in as much as they may have little meaning for the observer. They are just movements, which are easily described and which can be described on a superficial level as being Abraham's response to a certain situation ( $B=f(P,E)$ ). If we really want to understand what Abraham is doing though, we must have some idea that he is about to sacrifice the child because God has

told him to do so, which is a very different action from punishing the child or wanting to kill and eat the child, even though the behaviours involved in all three could be identical. In other words, we need to understand the situation as Abraham sees it, not the situation as we see it.

To explain Abraham's actions, we need to understand his reasons for what he is doing. His behaviour can be explained as being based on principles of action, but it does not seem right to say that they can be explained nomologically in the way that Hempel's covering law would attempt to do. In fact the covering law would seem to distort the circumstances in a strange way. If people like Abraham would do the same as he does in the same circumstances, to say that a person did something different means that by definition they are not like Abraham. Even if the law was expressed as "Very religious people will sacrifice their children when asked to do so by God", then if they do not, their actions must mean that they are not very religious, which does not seem to be necessarily true. Nor does it help to understand why they do or do not sacrifice their children. An explanation of that act seems most appropriate in terms of their reasons, not in terms of a general law.

It is clear that Robinson believes psychology, or at least a relevant psychology, should deal with the "cognitive, affective, and volitional" antecedents of behaviour, or rather actions, and that given this agenda, a specifically

"scientific" psychology is unable to handle the job. Not only does a scientific analysis need generalizations appropriate to the deductive-nomological framework it uses, which the concept of reasons inherent in our non-scientific account of behaviour renders inappropriate, but also those reasons and other psychological states have no empirical referents, which also means that a scientific psychology, based on objective observations of behaviour, will be unable to cope with it. It is also clear that in assessing the function of psychology in this way, he is endorsing the received common sense view that people have reasons for the things that they do, which means that they are not to be treated in the same way as a ball bearing in a magnetic field, by having their behaviour explained in those terms.

By rejecting the possibility of a scientific approach to psychology, on grounds which he says share a lot with Aristotle's conception of causes, presumably in terms of focussing attention on final causes (telos) or intentions, Robinson rejects as inappropriate one form of enquiry. This does not mean however, that he is anti-scientific. On the contrary, he stresses that there is a place for science in the study of people. He says that there are places where some of the complex phenomena in any sphere of human activity "will admit of scientific explanation and others will not" (p 72), which is of course the Aristotelian view.

Where complex social interactions are involved, or anywhere in which there is unequivocal evidence of human intentions, volitions and deliberations, it is clear that any coherent explanation will not be of the causal variety but will inevitably require the

postulation of irreducibly psychological dispositions. Recognizing this, the inquirer proceeds to assess the worthiness of those possible methods of investigation which honour the essential character of the phenomena. Depending on the particular event under consideration, the preferred method may be introspective, historical, interrogative, purely conceptual, or experimental. But it is the phenomenon that dictates the method, and not vice versa. (p 72/73) [4]

Robinson's title asked whether the relationship between psychology and science was one of synonymy or antonymy, and his answer to this question is that it can be one or the other, depending on what kind of explanation is offered, which in turn depends upon the phenomenon being studied. He suggests in concluding that those phenomena that lend themselves to a purely causal account, needing no recourse to concepts such as intentionality or rationality, can be given a scientific explanation, but that the discipline that provides such an explanation is not really psychology. On the other hand, those phenomena best understood by positing rationality as part of the behaviour of the participants "must be explained psychologically" (p 73), but the explanations given of these phenomena will generally lack the nomological features of scientific explanation. He argues that there is room for a scientific non-psychology, as well as a non-scientific psychology, which can both help people to understand themselves, but he also stresses that ideographic science can simply be discounted. It seems that we should understand him as meaning that a science which has as its main aim the understanding of individual events, such as, by implication, many of our "rational" or "volitional" actions are held to be, cannot use

explanations of a nomothetic sort, which look for general laws. Human beings are essentially too unique for such an approach to help us to understand ourselves.

This idea can be related back to his observation that a drift towards cognitivism has taken place in psychology, which he sees as being an expression of the desire to come closer to the acknowledged facts of mental life. The realization that forms of psychology like behaviourism produce distorted explanations of behaviour to such an extent that its explanations are actually vacuous has meant that an attempt has been made to deal with what are acknowledged facts about people, that they have thoughts and feelings which affect their behaviour. However, this realization has not been combined with an appropriate methodology. Instead, the covering law and nomological models have been used, along with their attendant causal explanations, which again fail to come to grips with the fundamental reality of human life. Thus, Robinson appears to lend support to the position which is maintained in this thesis, that an experimental, causal social psychology does not give us deeper knowledge about ourselves, because it neglects too many of the fundamental distinctions which have to be made when dealing with people's behaviour. Not only this, Robinson shows how "problem and method" pass one another by, by showing that the "problems" which social psychology are attempting to study are not accessible by the methods it uses, because they are most often related to human wishes, thoughts, and emotions, and therefore not to be explained in general terms.

Robinson asks, finally, whether his recommendations for psychology mean no more than a revival of "armchair" psychology. This brings to mind the paper by Dorwin Cartwright already mentioned, "Contemporary Social Psychology in Historical Perspective" (1979), in which Cartwright suggested that:

The emergence of social psychology as a distinctive field of empirical research around the turn of the century can be viewed, I believe, as a [Kuhnian] generational revolt against the armchair methods of social philosophy. (p 83)

Robinson suggests that to even ask such a question means that the scientific approach to psychology has been accepted as the correct one. The widespread conviction is "that once psychology moves away from its imitation of science it can aspire to no more than opinion or reckless speculation" (Robinson: p 73). As one would expect, Robinson does not accept this criticism, preferring instead the "rich trove of insights and possibilities possessed by folk psychology" to the "far less consistent and far more artificial models in some textbook psychology" (p 73/74). By accepting scientism, or the applicability of the scientific method to all problems, psychology is unable to explain many things that the lay community have no difficulty at all in explaining, while, according to Robinson, the counterclaim, "that laypeople only think they have an explanation but are really deceiving themselves - can only be made by a discipline that has accepted the task of explaining the same phenomenon" (p 74). He thinks that psychology avoids the

relevant facts about human behaviour, and thus fails "in the historic mission facing all serious scholarship - the correction of common errors and the elimination of common prejudice" (p 74), even though this is the avowed aim of social psychology.

The idea of scholarship correcting common errors can be related to a philosophical argument given expression by Russell in The Problems of Philosophy [5], that appearances and reality can be different from each other, and that therefore we can be wrong about what we feel certain of. There seems to be no way of denying this claim, but what is being claimed here is that despite Lewin's conceptual apparatus for distinguishing between appearances and reality, or between phenotypes and genotypes in his terminology, he does not in fact show a way of understanding what the deeper reality of human behaviour is, if indeed there is one there to be discovered at all, which is a separate, and difficult, question.

We have seen that Lewin's work is at heart a re-description of ordinary concepts, and that this re-description leads to a conception of causation which appears to rival the common sense conceptions, whilst systematically downgrading explanations involving reasons because they do not fit the model which is being used. It has been suggested that reasons are at times an essential part of understanding human behaviour, and that without them a psychological inquiry is seriously impoverished. We will now see that Lewin in fact recognized the importance of this



criticism, but in attempting to account for it in his system of psychology he produced an inquiry which has the form of a science, but which is conceptually unable to make predictions or general explanations, unlike the natural sciences which he tried to emulate, further undermining his claims to give a deep understanding of behaviour.

#### CHAPTER TEN: FOOTNOTES

[1] Of course, Hume says that reasoning cannot, of itself, give rise either to passions or to actions (Treatise of Human Nature). There is no necessary connection between reasons and actions.

[2] Although Robinson does not phrase his assertion in this way, it does seem to follow from what he has been saying about the issue.

[3] The story of Abraham is found in The Old Testament, Genesis, Chapter 22.

[4] It is interesting that Koehler also said this, all those years ago in Gestalt Psychology. "The nature and the actual development of our problems should decide about the methods to be followed" (p 50). Even so, his main interest was in "science" and its application to psychology, and so his example may have influenced Lewin more than his recommendations.

[5] Russell (1912), especially chapters one and two.

## CHAPTER ELEVEN

Kenneth W. Spence, in "The Nature of Theory Construction in Contemporary Psychology" (Psychological Review, 51, 1944), "examines the attempts of psychologists to discover general laws of behaviour, particularly the auxiliary theoretical devices they have employed in doing so" (p 48). In the course of the paper he describes the "scientist's" task as the attempt to discover ever more generalised laws, by which the observable events under scrutiny can be brought into interrelation with one another. To do this the scientist:

Develops and refines (mainly in the direction of quantitative representation) his concepts of variables, arranges highly controlled (experimental) conditions of observation and introduces theoretical constructions. (p 47)

Spence suggests that theory construction can fulfil two functions, and that these two functions can be seen in different fields of science, or even in the same science at different stages of its development. One function, which is seen in "present day physics", is to bring empirical laws into functional connection with one another, when they had previously been isolated realms of knowledge. The physicist is able, by experimentation, to discover descriptive, low-level laws, and then uses theories to formulate more abstract principles which try to bring these laws into relationship with each other. An example of one of these "comprehensive" theories would be Newton's principle of gravitation, which provided a theoretical integration of laws

such as Kepler's three laws of planetary motions, Galileo's law of falling bodies, the laws of the tides, and so on (p 48).

On the other hand, in what Spence calls the less highly developed areas of knowledge, "such as the behaviour and social sciences" (p 48), theories are used in a different way. In what could be called the more advanced sciences, such as physics, experiments can be performed in isolated, elementary situations, meaning that there are only a limited number of variables to manipulate. In the more complex fields of the social sciences, Spence suggests that even the simplest experimental situation involves so many variables that it is extremely difficult "if not impossible, to discover the empirical laws relating them" (p 48), a view which is consistent with the philosophical doubts about a scientific psychology attributed to, for example, J.S. Mill by Robinson. Acknowledging this complexity, theories are used as a device to help formulate laws. They consist essentially in the introduction or postulation of hypothetical constructs, which are meant to help bridge the gaps between the experimental variables. Lewin's formulation of "tension systems" is a theoretical construct of this ilk, bridging the gap between giving a waiter an order and his ability to remember that order.

In his general discussion of theoretical constructs in psychology Spence outlines the problem facing the psychologist as scientist. In common with all scientists, the psychologist, according to Spence, wants to establish the

interrelations between a set of experimental variables. In other words, the psychologist wants to discover empirical laws. To this end, Spence reports that psychologists study two main categories of variable:

(1) R-Variables, which are measurements of the behaviour of organisms, and which, for example, can be complex achievements (actions), or generalised response characteristics (traits, abilities, etc). These R-Variables are often referred to as the dependent variables [1].

(2) S-Variables: measurements of the physical and social environmental factors and conditions which organisms respond to. S-Variables are also called independent, or manipulative variables (p 48/49).

The scientist attempts to find the relationship between R and S, normally in numerical terms. Thus

$$R = f(S). \text{ (p 49)}$$

Spence points out what is involved in such an attempt:

The problem here is two-fold: (1) to discover what the relevant S variables are, and (2) to ascertain the nature of the functional relations holding between the two groups of variables. (p 49)

Spence suggests that theoretical constructs are used in psychology to make a "guess" at what variables, other than

the ones the experimenter can control, are determining the observed responses. Such constructs are necessary because of the nature of the phenomena psychology studies.

In Robinson's terms, this observation is prompted by the fact that behaviour "as such" is psychologically neutral, and by the other, related fact, that what is psychologically interesting is the volitional antecedents, and so on, of behaviour, which as independent variables are unobservable. Spence gives Robinson's ideas in a different twist though, suggesting that if particular environmental conditions always produced the same response, then there would be no need for theory, because given this invariable sequence, the response could always be predicted. Even if responses varied systematically with differing environmental situations, it would be possible to state a law relating them. Unfortunately, things are not this simple in psychology.

As Spence says:

On a second occasion of the presentation of condition X, the subject is very likely to exhibit a different magnitude of response, or in the second example there may be no simple curve discernible between the two sets of experimental values. (p 51)

Thus, hypothetical constructs are used to show that the response variable is determined in part by X, and in part by some additional intervening variables or factors. Constructs of this kind have been used in four main ways by psychologists [2]: Spence identifies Lewin's theory as

involving constructs defined primarily in terms of the R-response variables. This observation emphasizes that Lewin was not much concerned with the objective environment in his theorising. Lewin had noted, as we have already seen, that the behaviour of what Spence terms as organisms, varies, even though the objective environmental conditions remain unchanged (Spence: p 53). This observation led Lewin (and others) to assert that behaviour must be accounted for in terms of the psychological situation, or the situation as the subject sees or perceives it, in terms of what it means to them, an approach which Robinson would clearly approve of, and one which would seem to make sense when related to the Abraham example.

Lewin of course used topology, and also hodology (which is a topology with "directions" or vectors) to represent the relationships between the behaviour of an individual and the two sets of theoretical constructs he was interested in, the person (P) and their psychological environment (E). Spence comments about Lewin's topology, making an observation which is relevant to what has already been said about Lewin's system, and also to the main theme of this thesis.

He begins by pointing out, as other philosophers and psychologists (and mathematicians) have noted, that "there is considerable reason to doubt whether Lewin does much more than take over the terms of topology, making little if any use of postulates (implicit definitions) of this formal system". He continues by saying that the formal system of

dynamics, which Lewin has mentioned "remains thus far closeted....in Lewin's mind". Spence then comes to his conclusion:

Like so many of these field theorists, Lewin sets up a most attractive program for theory. Taken in conjunction with his interesting experiments the illusion is nicely created that there is some connection between them. (note 3 to p 54)

This is a very frustrating assertion, both because of its brevity, and because Spence gives no evidence to support his accusation, other than his belief that the theoretical edifice Lewin raises is not based on anything substantial in the way of a strict mathematical analysis, or on a formal logical system with which to describe his thoughts on dynamic processes. However, this assertion does give a novel twist to the idea that problems and methods pass one another by in experimental psychology. Here the assertion is that method and theory are not connected, and that even if they do not pass each other, they are certainly not moving in the same direction, even though from a distance, it looks as though they could be. Looking further at Spence's argument should help to explain what he meant by this rather nasty remark, and also help to understand some of the problems inherent in Lewin's thinking.

A clue to the fundamental defect of Lewin's system comes in Spence's discussion of the functional relationship Lewin is interested in,  $B=f(P.E)$ . As Spence says, this is obviously not a law of the type that psychologists were meant

to be interested in, ie.,  $R=f(S)$ . Laws are supposed to be a statement of the relationship between independently defined variables, but because the objective situation is not emphasized in Lewin's work, it had been suggested that there is only one set of independent variables in his equation, the observed behaviour (p 55).

However, Spence rejects this criticism on Lewin's behalf. He says that Lewin's method of determining the properties of the psychological environment in individual cases depends heavily on phenomenological introspection. If we think of these introspections as a kind of verbal response, in which the subject tries to describe their own particular way of perceiving the objective environment, it could be said that Lewin's theory can provide laws mediating between independent variables, if we take these variables as being two different responses the subject makes, or if we take the laws as mediating between the experimenter's own perceptual responses and the subject's subsequent response. Lewin's laws therefore amount to  $R_1=f(R_2)$ .

Whether this reply answers criticisms of Lewin's formulation of laws or not, it is clear that methodologically he has to rely either on asking the subject to describe how they perceive the situation, or on inferring what the subject's field might be, by making his own introspections.

Spence quotes Snygg (1941) in connection with this observation, and also with the problem of prediction.



...The determining locus of behaviour is the behavior's p.f. [phenomenological field]. This is not open to direct observation by an outside observer. The process of prediction therefore involves two steps: (1) the securing of an understanding of the subject's field by inference or reconstruction, (2) the projection of the future field.

The first operation is of the common "Now why did he do that?" or "Under what circumstances would I have done that?" character. Much of the topological work of Lewin is of this type.... (Snygg: p 413)

This observation is of great importance, as it gives a clue both to the attractiveness of Lewin's system, and also to its major logical flaw, and thus has ramifications for what has been built upon its foundations, namely social psychology.

Spence says that the field approach Lewin used was fruitful and valuable, as evidenced by the experimental contributions it has spawned, although significantly, in Spence's opinion "the theoretical superstructure has played a much less significant role than is sometimes credited to it" (p 57). He then goes on to say that the phenomenological approach has advantages over other methods when dealing with the very complex field of social behaviour of the human adult.

It is obviously much easier to gain some notion as to the relevant variables determining such complex behaviour by asking the individual to verbalize than it is to employ the procedure of trying to hypothesize them from knowledge of past history. (p 57)

The above remarks are part of Spence's description of

what he feels Lewin's method to be; an analysis of behaviour in terms of the individual's psychological field, which is to be found either by simply asking the individual, or by inferring what that field is. However, Spence is both right and wrong about Lewin's method; right in that what he suggests seems logical given the statements Lewin makes about the psychological field, and wrong in that Lewin actually does not do what Spence thinks he does, or ought to do.

If we are to take the determining factor in behaviour to be the situation as the subject sees or perceives it, if we take it to be the psychological environment that is important, then it looks as though we should ask the subject what aspects of the situation prompted them to behave in the ways that they did. For example, if people are asked why they finished what they had been given to do in a psychology experiment, they might say that for them it was important to finish the tasks because they wanted to please the experimenter, because they had been asked to help in an experiment and they did not want to be seen to be going back on their promise to help, because they did not want to look lazy, because everybody else was finishing the tasks, because there was nothing else to do, and so on. If they were to be asked why they remembered unfinished tasks better than completed ones, they might say that it was because they thought they might have to finish them later, and they did not want to appear stupid when they could not, because they thought that they had been interrupted for a reason, to see how much they could remember later, because they thought the experimenter would be happier if they could

finish all the tasks, meaning that they would have to remember how to perform these unfinished ones, and so on.

They could also describe their situation in various other ways, by saying that they felt there was an expectation for them to finish, and thus they felt compelled to do so, because they were carried along by watching others complete tasks, and so on. On the whole, there are two types of explanation that can be offered plausibly in ordinary, common sense discourse; in terms of the person's intentions, and in terms of the situation influencing the person. Lewin's analysis of actions in terms of the interaction between person and situation therefore seems plausible by these standards, and also attractive as a way of explaining people's behaviour when they are not entirely sure why they finished the task or remembered something.

The attractiveness of Lewin's psychology is that it takes what seems to be known all along, that situations influence behaviour, and expresses this knowledge in what looks like a rigorous, scientific way, thereby eliminating the possible bias and inaccuracies of the poet and the biographer, for example.

The different responses people would give to explain their behaviour serve to explain why people behave differently in what is "objectively" the same situation. Lewin acknowledges the fact of divergent behaviour, and therefore tries to account for it with his scientific

terminology of psychological environments, and therefore, phenomenological fields.

As Robinson has pointed out, the same piece of behaviour can have many cognitive (and so on) antecedents. Although these antecedents are not observable, they can be described in ordinary terms by particular individuals. Therefore if we ask someone "Why did you do that?" they can give an answer which can satisfy our curiosity, usually in terms of giving reasons for their actions as above, or even in terms of being "carried along by a situation". These responses are the basic evidence which Lewin has to consider in deciding how to theorise about people's behaviour.

As Spence and Snygg have noted, there are two ways of attempting to ascertain an individual's psychological environment; by asking them (reconstruction), or by inference (deciding what their psychological environment must be like). In many cases it looks as though Lewin takes the second option. For instance, in the child eating the food example, it looks suspiciously as if Lewin has broken down the steps leading to the child eating on the basis of his own thoughts on the matter, or on the basis of his inferring what the relevant steps might be for some average child.

To recap, Lewin suggests that the otherwise unified action of eating usually breaks up into a series of separate acts, such as putting the hand on the table, taking the spoon, putting the food on the spoon, and so on. Although this description looks to be plausible, because after all,

who could deny that these things do take place when we eat something?, it seems unlikely that anyone would actually describe what they are doing, when they are faced with something that they do not want to eat, in this way. The relevant "variables" determining such complex behaviour could be along the lines of not being hungry, being cross with one's parents, disliking the food, and so on. Lewin's interest here is in the effect of social pressure on the behaviour of children during meals, and especially in making the child eat what they do not want to eat. Given this interest it is not surprising that Lewin would see the situation as a number of stages to be overcome. The child however may see the situation in an entirely different way. What is more, different children could see the objectively same situation in different ways, as could Lewin if his interest were centred on a different aspect of the same situation. His analysis of the "causes" of behaviour could take different forms, as Hart and Honore for example have pointed out. Lewin's analysis of adolescence can be seen similarly, as being constructed on the basis of his observations and imagination.

Lewin says that the steps he has identified in the action of eating correspond topologically to a series of regions, which he represents pictorially. This raises the question though, of whose psychological environment the figure represents; the child's, the adult feeding it, or perhaps even nobody's? If what is important in explaining the different behaviours of different individuals is their

different perspectives on the same situation, how can Lewin be sure that the explanation he has given here will apply to a given individual? It may apply to them, and explain their behaviour, but from the main assumption that is made, that people behave differently because of their different psychological environments, it must be impossible to know whether or not anyone has a particular psychological environment until it has been ascertained that they have, and the only way to do this is to ask them.

If Abraham sacrificing his son is taken simply as a piece of behaviour, Abraham's psychological environment cannot be inferred from this behaviour, because the behaviour could be the result of a number of different psychological configurations. We may not believe him when he tells us that he is sacrificing his child, but if the way in which he sees his situation, or his psychological environment to put it the other way, is deemed important, then there might be no way in which this can be inferred satisfactorily to give a better picture of the motives for his behaviour than the one he gives us.

This fact must have further consequences. For example, what might induce one child to eat might not induce another if their psychological environment is different.

There seems therefore to be an inbuilt tension existing between two of Lewin's assumptions. His original assumption is that differences in behaviour can be explained by differences in psychological fields. The psychological

field functions as a singular concept in at least two related ways; firstly, they are given "phenomenologically", which necessarily means that they are singular; and secondly, they have to be individual because the behaviour of organisms varies even though the objective environment remains the same, meaning that there must be individual differences between the organisms to account for this discrepancy in behaviour. There is the suspicion therefore that the general, scientific, type of explanation which Lewin wishes to use does not fit very well with the phenomena that he is attempting to explain, because these phenomena are by their very nature, singular.

Lewin on the other hand, would suggest that this type of thinking and suspicion was old-fashioned, and that for progress to be made, it must be discarded and replaced by an analysis which allows that individual examples are part of a more general law, and that the differences in behaviour which are observed are only the result of the variable strength of the same forces acting upon individuals. All that needs to be shown is that the same forces are arranged differently in different cases. Thus, for remembering tasks, if we can show that the general explanatory concept, the tension system, is weaker in some cases than others, we are able to explain why some people remembered fewer tasks than others. Similarly, we can explain why some people eat less crackers than others, through the hypothesized impression management construct.

The problem though, is that it is impossible to take

a sample of people, put them into a situation, and to suppose that they all see it in the same way, or in other words, that their psychological environments are the same. This is implicitly admitted when experiments are conducted in which differences are noted between children, adults, and adolescents, as in Zeigarnik and Ovsiankina's studies. If there are differences between these groups, why not between people of different ages, from different areas, of different colour, and so on? So then why not admit that there are differences between people which make it impossible to have the kind of general law that Lewin is attempting to find? If we believe that the individual phenomenological field is important in determining behaviour, and therefore accept that people have different phenomenological fields, it seems unlikely that anything is gained by an analysis which extrapolates from the individual to the general, and which then tries to impose this general extrapolation back onto a different individual, whose phenomenological field might be entirely different.

A related point can be raised with respect to Lewin's treatment of causation. A criticism of his ideas on causation was made by R.W. Leeper in 1943, in his book, Lewin's Topological and Vector Psychology: A Digest and a Critique, in which Leeper, as the title suggests, attempts to "summarize accurately and sympathetically Lewin's methodological concepts" (preface: V), and also to make constructive suggestions as to how these concepts can be improved [3].



Leeper is not in the business of trying to ridicule Lewin's system; rather, the reverse is true:

Despite my numerous criticisms, the fact remains that I regard Lewin's concepts as a creative contribution of rare value. Otherwise, of course, I would not have been interested in criticising them so laboriously.  
(preface: XI)

Leeper was deeply impressed by Lewin's contribution to psychology, and he wanted to develop and clarify this contribution. As a "friendly" critic, who had consulted with Lewin on many occasions, Leeper's views are of great interest, since even Lewin himself said that Leeper not only presented vector psychology, but also improved it (foreword: IV).

Leeper notes that many features of Lewin's treatment of the domain of psychology have been influenced by his view that strict scientific concepts of causation must avoid relating present effects to past causes, and "must pertain merely to relationships within the present moment" (p 78). He says though that in actual experimental work Lewin's practice has not matched his principles, often being concerned with relationships between past and present. This opinion seems to confirm what Spence has said, but from a slightly more sympathetic point of view. Even so, Leeper believes that Lewin's views on the nature of causation have exerted a considerable influence on the rest of his system. It is worth reminding ourselves yet again that this conception springs entirely from his belief in the efficacy of science to

produce a relevant and proper psychology.

As we have seen, Lewin's view on causation is that it should be concerned with "systematic", as opposed to "historical", causation. As Leeper puts it:

The aim in studies of systematic causation is to find general laws which would be stated in terms of the interrelation among a number of facts (either observed directly or inferred) that exist at the same time. (p 79)

In Principles of Topological Psychology Lewin said that the Aristotelian way of thinking about causation did not make a clear enough distinction between systematic and historic questions, which led to the practice of taking past or future facts as the causes of present events. In contradistinction to the Aristotelian line, Lewin resolved to "strongly defend the thesis that neither past nor future psychological facts but only the present situation can influence present events" (Lewin: p 34).

This thesis follows from the idea that only what exists "concretely" can have effects, and that since the past and the future do not exist at the present moment, they cannot influence the present. Leeper suggests that when Lewin refers to the "present moment" he must be understood as meaning "parallel or contemporaneous processes over some undetermined (though presumably brief) time interval" (Leeper: p 79) [4].

Leeper raises two possible objections to the principle that causal processes are strictly contemporaneous with their effects. The first objection deals with whether cause and effect can be regarded as strictly contemporaneous in time. According to some scientific discussions of causation which were then current, if measurements are very precise, then cause and effect can be linked so closely as to be effectively contemporaneous. Leeper gives as an example a case where a light "globe" is broken by a hammer (p 80). Strictly speaking, he says, the movement of the hammer before striking the globe has nothing to do with breaking the globe. To refute this sort of argument, Leeper introduces common sense, which he believes asserts that things exist in time, and that what exists is a function of what has existed before it in time.

And so, if the universe is causally governed, and if the universe has in it a causal dimension, then effects are results of preceding causes, not of contemporaneous causes or conditions strictly parallel in time. (p 80)

In recognizing the temporal element in causal relationships, Leeper's objection takes on board the common sense notion of causation, and allows for the possibility of events further back in time being the causes of present events. It also implicitly admits that there can be more than one notion of causation. Strictly speaking, the scientific cause of the globe breaking is its contact with the hammer, this contact, the cause, being contemporaneous with the effect, the breakage. This kind of analysis gives no indication of the wider "cause" of the breakage though. For

example, there could be at least two ways in which the globe can be broken; being struck by the hammer, or being crushed by the weight of the hammer. In the first condition, there must be some idea in the analysis that either the hammer or the globe is moving, or else there would be no impact, which is what has been implicitly asserted all the time. Without this impact there would be no breakage. In the second condition, if the hammer is held in contact with the globe, and then allowed to fall under its own weight, the "contemporaneous contact" does not give an idea of the true situation. In both cases, to give the cause, as an explanation, of why the globe broke would need an extension of the causal process to include in the first case the motion of either the hammer or the globe, and in the second case the change in conditions leading to the hammer being able to exert its weight upon the globe. In both cases the cause would have to include more detail to explain the occurrence, and would have to include part of the context within which the globe was broken.

Such an explanation would be of a peculiar kind though. It might satisfy our curiosity if we were performing an experiment to determine what it is about hammers that causes globes to break. If we find that it is either an impact, or a pressure of one on the other which makes this happen, which we could perform experiments to discover, we would be happy, because we would have eliminated factors such as the colour of the hammer, its shape, where it was made, who is holding it, and so on. Beyond even this extended

scientific causation though, if we asked ordinarily, "How did the light globe get broken?", meaning an individual globe, we would not be interested in an explanation just involving the hammer, we would also want to know other things, such as how the hammer came into contact with the globe, or such as why someone broke the globe, both of which could involve references to the past and to the future.

Leaving this observation aside for one moment, we see that Leeper also agrees that it is not metaphysically justifiable to say that cause and effect are strictly contemporaneous, but he does say that as studies grow more and more refined, it may in the future be possible to state the causes of events in terms of antecedent conditions which precede the events only by the most minute divisions of time, perhaps even beyond measurement. On the basis of this belief, and one statement from Lewin:

In order to determine the contemporaneity for different parts of the field with absolute exactness one ought to take into account in psychology, as is done in modern physics, the velocity of mediating processes. But we can neglect this question for the present. It is much more important to stress the necessity of excluding events which roughly speaking belong to past and future time (Principles: p 35),

Leeper feels that it is safe to assume that by using the word "contemporaneous" what Lewin in fact means is "juxtaposed as closely as possible in time". In fact Lewin has a confused idea of what he means by causation when dealing with people.

Leeper's second objection is that on practical methodological considerations, it is difficult to relate effects only to immediately present causes. He says that we cannot know the structure of the life space for a given person at a given point of time, as Abraham's behaviour on the mountain top shows.

How can we know that the sight of a particular object will arouse a region of positive valence, or that certain tension systems have reached a state of satiation, or that the person is more likely to see one action rather than another as a means to a goal? (p 82)

Constructs such as these are not immediately observable and measurable, as Spence has pointed out. Leeper suggests that it is possible to infer that such factors exist in the present situation as immediately present variables, and therefore to understand the subject's field, only if; (1) we know what the previous events in the person's life have been, and (2) we have laws describing the relationships that exist between such earlier events and subsequent events (p 82).

Leeper puts great stress on the second condition here, emphasizing that such laws are not historical laws, but are of the sort that are properly included within a science. What he means by the distinction between historical laws and scientific laws he does not make clear, but he does say that he has discussed the question personally with Lewin, and we are therefore to suppose that the prescriptions Leeper gives are those which Lewin agrees with. Presumably he means that

scientific laws are of the covering law variety, linking causes directly with effects.

If we have laws that satisfy this second condition, Leeper is "happy to report" (p 82), that Lewin intends that his concept of systematic causation should be understood as recognizing both those laws which relate temporarily adjacent factors, and also those relating causes and effects that are separated by some shorter or longer time span.

At the time Leeper was writing his book, Lewin had not made a published statement on systematic causation which would have agreed with this analysis. Interestingly, in 1943, in the same year as Leeper's book was published, Lewin also published "Defining the Field at a Given Time" (Psychological Review, 1943), which does not mention Leeper by name, but which discusses and endorses the positions Leeper has argued for.

In this paper Lewin gives as one of the basic statements of psychological field theory the principle that any behaviour, or change in a psychological field, depends only upon the psychological field at that time, a formulation we have noted before. He says that in contrast to what many people have thought this statement means, field theorists can be interested in historical problems or the effects of previous experiences. This is possible because behaviour can be related to a past situation or to a future situation given two conditions, firstly, that these situations form a "closed system", and secondly, that the changes in the intermediate

periods can be accounted for by known laws (p 48). Leeper suggests that in his discussions with Lewin, he understood Lewin as meaning that in a closed system "all of the factors operating within the interval in question must be the same in one case after another in all significant respects" (Leeper: p 82).

Lewin's suggestion as to the way field theorists can be interested in historical issues differs slightly from Leeper's, by showing more precisely how laws can be formulated to account for "historical" causation. The two conditions he mentions for formulating such laws are related to each other. If in a closed system only certain factors are operating, and these factors are known, then experiments can be performed to show the particular effects these factors have. If, however, there are different factors at work on every occasion, then no laws can be formulated, simply because there is no regularity present upon which to base such laws, there being too many unknown variables, the determinants of psychological processes and outcomes being too numerous and varied to admit of systematic experimental manipulation, to re-iterate what Robinson noted earlier.

Leeper points out though that "human life is never, strictly, a closed system because no two persons ever have exactly the same events in their lives between one point of time and some later point in time" (p 83). This observation means that if an experiment is performed on memorising for example, and the results are used to help predict the outcome



of learning by other individuals:

...We can be fairly sure that their lives during the interim will not be "closed systems" comparable even to the average interim-experiences of our experimental subjects. Psychology, therefore, seems to be a science that must forever be doomed to make statements in terms of "probably...", or "there will be a tendency for...", etc. We can never be exact because we cannot secure closed systems with which to work. (p 83)

This is an important admission, since in dealing with an activity such as memorising, which can be seen as a "mechanical" activity, and not under as much individual control as other activities, it might be thought that a fairly good relationship could be found between the relevant variables. If this is not the case, then the hope that significant relationships can be found when dealing with motivations is obviously put under great pressure. To argue that the factors involved in people's motivations form a closed system, and that they are the same in one case after another, does not fit the psychological facts as they seem apparent, that the same act can be performed for many different reasons, and that these reasons may never be repeated, either in the individual's life-time, or in any other individual's experience.

When Leeper argues in this way he implicitly acknowledges that what Dray says about explanations involving motivations is true, that to say people in a certain situation will behave in a certain way can only be a principle of action. The behaviour does not follow logically, or necessarily, from the situation. By admitting that

people's lives are not closed systems, and that they cannot be treated as though they are, Leeper and Lewin seem to be admitting a methodological flaw, in that they could argue that given sufficient funds, resources and time, they could in fact find the laws relating behaviour to situations etc. Such laws could in principle be found, but at present, in practice, cannot. Dray on the other hand, would probably argue that there is a conceptual flaw in an argument which tries to produce covering laws dealing with human behaviour. This difference of opinion is of fundamental importance, but if psychology claims to provide a rival explanation of human behaviour superior to that provided by common sense, or ordinary thought, as exemplified by Hart and Honore, then for the moment it is enough to show that social psychology's claims cannot be substantiated, and that its promise to give a deep understanding of behaviour through the use of scientific concepts and causal analyses collapses.

This failure is part of Lewin's acceptance of the scientific method the acceptance of which led him into a confusion when dealing with the motivations, or causes, of behaviour. He accepted the basically common sense view that behaviour changes as a result of situations, and also that the interpersonal differences in the same situation are to be explained by people's different perceptions of that situation. Then however, he sought to systematize his insight using a quasi-technical terminology to characterize the common sense notions he used as his primary data, generalizing these data by suggesting that the same basic

configuration of psychological environments was common to a situation, and explaining differences of behaviour in terms of differing strengths of the same field. Unfortunately this generalization is an unjustified step, because his own insight originally suggested that people's perceptions and psychological environments were particular to themselves. Thus, the conceptions and assumptions he was using led him to conclusions which his own basic premises should not have supported.

A similar confusion existed with his conception of causation, which was based on an understanding of the wide variety of motivations people have, but which he tried hard to force into a structured causal framework, only to find that it did not fit, as his conception of closed systems shows. His rejection of Aristotelian conceptions of cause has a great deal to do with this failure. If he had allowed, with Robinson, that some aspects of human behaviour are amenable to causal, nomological explanation, while others are not, he might not have attempted to fit all human behaviour into a causal system which seems patently inappropriate for it.

Having argued in this way about Lewin, it seems obvious that the same could be true of social psychology. Social psychology also tries to deal with motivations in terms of genotypical causes and mechanisms, which are founded theoretically on the Lewinian method. There is perhaps an initial insight into a behaviour, that people eat less in the presence of a member of the opposite sex, which is then translated into a quasi-technical concept, the impression

management mechanism. Endorsing the idea of a "mechanism" being operative justifies assuming its generality and its capacity to be systematically altered by the manipulation of variables, in accordance with the topological representational assumptions of the psychological environment. This being the case, experiments can be performed to elucidate the causal connections between the variables and the behaviour in question. The only problem is that the whole technique is unsuited for dealing with volitions and motivations, which is why a solid body of theory and facts has not been built upon Lewin's theorizing. Kepler's original ideas about the universe were wrong, but he was using an appropriate technique for the problems he was studying. Lewin's original ideas were wrong too, but the techniques he used could not accommodate what he seemed to know were the facts of human life, this inhospitality being shown in the confusion which ensued in Lewin's topological psychology, and also later, in social psychology.

It is worth discussing briefly how Lewin fell into error when discussing psychology. The reasons for his mistake can perhaps be seen by considering a paper by Frank Cioffi, which will also help to show the conceptual errors into which Lewin fell.

#### CHAPTER ELEVEN: FOOTNOTES

[1] See for example Manstead and Semin (1988) for a discussion of dependent and independent variables.

[2] 1) Animistic conceptions, 2) neurophysiological theories, 3) response inferred theoretical constructs, which Spence concludes Lewin uses, and 4) theoretical constructs

as intervening variables between S and R variables (Spence, 1944). Rather than discussing the merits of each of these approaches, it will be more fruitful just to consider Lewin's particular approach, as a way of understanding it more fully.

[3] Boring (1950: p 734): "The most thorough and extensive exposition of Lewin's thought is by Leeper. The most stormy denunciation of Lewin's use of concepts is by I.D. London."

[4] ...Not some hypothetical entities devoid of any temporal dimension at all, which is what could be read into his prescription, if the present moment is taken to be almost instantaneous, or if the temporal component of the event is eliminated.

## CHAPTER TWELVE

In "When do Empirical Methods Bypass 'The Problems which Trouble Us'?" (1984), Cioffi discusses the problems of knowledge that we as individuals find in the world. There are questions that arise in the course of our lives which we feel we need an answer to, which are important to us, but which can somehow seem to elude our attempts to answer them. He does not claim that these questions are a general feature of our lives; some people are not affected at all by their force, and on many occasions the explanations we have at hand are quite adequate. He does however draw attention to the fact that for some people, some of the time, some particular sorts of solution will not help them answer the questions which trouble them.

The title of the paper is an obvious reference to the Wittgensteinian remark which was mentioned in chapter nine, that the existence of experimental methods makes us think that we have the means of solving "the problems which trouble us; though problems and method pass one another by" (Philosophical Investigations: p 232). Not unexpectedly then, Cioffi deals with what he sees as being Wittgenstein's contribution to assessing the adequacy of explanations in different circumstances. He is critical of Wittgenstein, and these criticisms, along with Wittgenstein's ideas, combine to offer an insight into the confusions into which Lewin and social psychology plunge.

In the first section of the paper Cioffi discusses

Wittgenstein's criticisms of Frazer and Freud, and their handling of certain phenomena. In the second section he discusses disenchantment with biographical knowledge, and in the third and final section he discusses the influence of our remote part on our personal development. His discussions are all related, and are all part of his argument that there can be:

A mode of transaction with the phenomena of our lives other than the empirical - a desire for other than a knowledge of their causes and conditions. (p 171)

Cioffi's contention is obviously very relevant to the discussion of Lewinian social psychology, because his thesis, that there can be a way of viewing ourselves other than in causal terms, runs directly counter to the firmly causal explanatory mode which is reached for by social psychology.

Cioffi begins by discussing Wittgenstein's criticisms of Frazer and Freud, making explicit the problems which he says these criticisms raise. The point at issue in these criticisms is expressed in a feeling we can have about questions and explanations; namely, that although some questions seem to require more information to resolve them, and can therefore be called empirical, such a resolution does not always satisfy us. Cioffi's interest is in how any alternative resolution can be made, and whether it gives us any "knowledge", in the way in which empirical explanations are normally said to.

Wittgenstein's contribution to exposing this feeling

revolves, Cioffi believes, around the idea that:

There are certain phenomena which induce in us a desire for a clearer grasp of the relation in which we stand to them, or, to use Wittgenstein's own expression, a sorting out of our "crush of thoughts" with respect to them. (p 156)

Wittgenstein presents us with an insight into what might be causing the dissatisfaction with empirical information about phenomena. He suggests that in some cases an approach which centres on our own reactions to phenomena can be more appropriate than one which uses empirical or objective data. He suggests further that the distinction between these two approaches to explanation can often be overlooked, and that as a consequence, inappropriate explanations can be offered about phenomena.

The confusion between the use of empirical methods, and the use of a technique to sort out the crush of thoughts, appears to occur in two different ways. The first is to think of certain situations as requiring empirical investigation when what is needed is a self-analysis of some sort. For example, Cioffi says that it is as if we are asking the same kind of question as asking why we are bilious, only about the mind instead of the body. It is the same sort of error as failing to recognize the difference between the two questions "What is it that I am feeling?" and "What is it that I am sitting on?" (p 156).

The second road to confusion is to ignore problems



for which empirical methods are inappropriate and to concentrate on problems which they are appropriate for, neglecting the other problems. This is of course the accusation that Robinson makes about psychology. The difference between these two confusions is very subtle, but Cioffi feels it to be important. On this second view, the mistake being made, as exemplified by Frazer [1], is not that he raises a question and then foolishly begins to investigate it empirically (investigating why the Beltane fires impress us, by trying to trace their origins) but that having learnt about the existence of the festivals he pursued empirical investigations instead of reflecting about his own response to them and what it was that gave this response its particular character (p 157).

Having shown that there is the possibility of making these two different but closely related mistakes, Cioffi then goes on to suggest that, contrary to Wittgenstein's position, Frazer and Freud might not be making mistakes of this kind at all. He points out that it would appear to be a conceptual error to answer questions such as "Why does the Beltane fire festival impress us?" by looking for more information than we already have (p 160), but he also points out that Frazer might not have thought that discovering the origins of the fire festivals was the way to explain why they seem sinister. Cioffi suggests that Frazer's methods did not pass his particular problems by, because they might have given him exactly what he wanted (we cannot know this without asking him); what happens is that they pass our problems by, if we want to know, along with Wittgenstein, what it is that makes

human sacrifice so sinister. Thus, the source of the "confusion" which Wittgenstein accuses Frazer of could lie in the different expectations Frazer and Wittgenstein have for an investigation into the practice of human sacrifice etc.

Similarly, when Cioffi tackles Freud's treatment of dreams (pp 161 ff), he says that Wittgenstein's quarrel with Freud seems to be about Freud's use of causal explanations in general, as opposed to his proffering conceptually inappropriate answers to non-causal questions.

For example, in the dream Wittgenstein discusses (Lectures and Conversations: p 23/24), Freud's patient characterised her dream in a certain way, describing it in a way which is characteristic of it being her own, personal dream. Freud then produced his own interpretation of the dream which contradicted the characterizations the patient had made. Wittgenstein had criticised Freud's work on jokes, saying that the correct "explanation" of the joke is the one that is accepted, and he applied the same criteria to the dream analysis. Cioffi agrees with the joke analysis, but he argues that when discussing dream analysis the same criticism will not suffice, simply because it is so obvious that Freud is not perturbed by the fact that his account of the dream is so different from the dreamer's. This being the case, Cioffi suggests that it seems as though Freud:

...Has given the strongest possible reasons for concluding that he does not see himself as trafficking in further descriptions, or in the elaboration of the ideas with which the dream seemed

pregnant. (p 161/162)

Cioffi suggests that this fact must be obvious to Wittgenstein, and that therefore Wittgenstein's objection to the dream interpretation must lie in his belief that causal questions are irrelevant to dreams. However, Cioffi then asserts that there is no a priori objection that can be made to the causal explanation of a dream, since there is equally no reason why an interpretation should be deemed inappropriate simply because it does not agree with what the dreamer felt about the dream, especially when there are psychopathological reasons for establishing the causal relations into which a phenomenon enters. For example, Cioffi shows that a neurologist would respond to "a patient's account of the aura of imminent revelation that preceded his epileptic fits with talk of epileptic foci and neuronal discharges" (p 163), and says that in this context it is perfectly right and proper that this stance should be taken.

The point Cioffi wants to make in this first section is that Wittgenstein was not necessarily right in his criticisms of Frazer and Freud, and that he might have in fact missed their point. Cioffi says that an hypothesis is an inappropriate response to a request for a further description, which is what is needed in cases where our crush of thoughts, for example, are troubling us, but equally, what Wittgenstein seems to miss, that a request for a further description may itself be inappropriate.

When we are dealing with psychopathological

phenomena, the questions we want answered cannot be settled by reflection, and the questions which can be settled by reflection we ought not to be raising. If the patient complained she felt deprived because our hypotheses lacked the charm of her own elaborations of the meaning of her experience of illness, we should reconcile ourselves to this rather than capitulate to it. (p 163)

Wittgenstein's criticisms of Frazer and Freud have centred on his belief that the methods they use are not appropriate to the problems that trouble them, or to the questions that they are trying to answer. Cioffi however points out that we cannot necessarily know what these problems are, that the questions that other people want answered may be different from the questions that we want answered. The moral to be drawn from the discussion is that there are many differing accounts which can be given of a phenomenon, the interest one has in the phenomenon dictating to a large extent the account which will be accepted as satisfactory. The doctor and the patient can disagree about the adequacy of an explanation, but this is just a fact of life which we must come to terms with.

Cioffi is saying therefore that Wittgenstein's criticisms of Freud and Frazer are not knock-down criticisms, that there can quite properly be differing and conflicting modes of explanation, and that care must be taken when criticising an explanation to allow for these differing interests. As we have seen from thinkers such as Ryle, and Hart and Honore, this is not a new or novel position to take. Cioffi though, is not entirely satisfied with it, and suggests that there are reservations which need to be made to

it, these reservations being concerned with biographical knowledge, and the influence of our past on our personal development.

Cioffi says that we can come to a realization that there are questions, or at any rate yearnings, which are provoked by our encounters with others that no amount of information can assuage (p 163/64). The encounters we have with others can at times produce a kind of "wonder" at the lives of other people, and this wonder seems "naturally" Cioffi says (p 164), to take the form of empirical speculation. The problem is though, that sometimes further information can provide us with what we want (if we learn that someone's ability to overcome their dread of giving a lecture is achieved by thinking of something much worse that they had undergone) but sometimes it cannot, especially where what we are interested in is a kind of "quality of being" as Cioffi puts it.

The "wonder", and the "quality of being" which we are interested in are part of what Sartre calls the capacity of the other to "assault our being", and it is this assault which renders an empirical investigation inappropriate. If we want to know why a historical figure such as Napoleon "assaults our being" then Cioffi suggests that it might be a better idea to inquire into the assaultability of our being by Napoleon. Such an enquiry would explain why we are assaulted by Napoleon and not by Wellington, and it might explain why we are assaulted by Napoleon when our friends are

not. It is also evident, although Cioffi does not say it aloud, that in this case a successful explanation is one that is accepted by the person whose being is assaulted in this way. The success of an explanation cannot be determined by anybody else.

Cioffi gives other interesting examples of cases where additional information will not assuage our curiosity about others, where what we want is to somehow experience what it is like to be somebody else, or where our interest in others is "parasitic on a craving for a perspicuous view of the characterological aspirations and reforms round which our own hopes, efforts and self-reproaches centred" (p 168). What these examples have in common is that our interest can only be assuaged by some sort of self-analysis, by coming to see the relationship in which we stand to the person whose being has captured our interest and wonder [2].

Similarly, in our assessment of the influence of our remote past on our development, Cioffi says:

We tend to confuse the role of past episodes as causal influences with their status as intentional objects of reminiscence and rumination. (p 169)

A person can be bewildered at the kind of person they have turned out to be. This predicament calls for something other, or at least more, than the display of causal relations between their past and present. Cioffi suggests that when we ask ourselves about influences in our past there are occasions when we would not be satisfied by empirical answers

which tell us what difference these influences actually made to us. What is called for is a synoptic view of "the countless reveries" (p 170) into which the thought of a past event might enter, a clearer view of what we thought about that event, or of what we feel we ought to think about it.

Again, this procedure is part of a sorting out of the crush of feelings about an event, or feelings which can be brought forth at the memory of an event, and so what is needed is not causal knowledge about the event, or even a further description of it.

We must remember that throughout the discussion Cioffi has been referring to "our reflections on such matters" (p 170), and "their natural consummation" (p 170), suggesting that there is a mode of explanation which is entirely divorced from the empirical, a mode of transaction with the phenomena of our lives which desires something other than a knowledge of the causes and conditions of these phenomena.

What we want with respect to certain phenomena are not their causes, but their bearings. The lack of closure, the sense of unfinished business that we experience with respect to them is not always a matter of factual ignorance, to be relieved by the discovery of causal relations. (p 171)

Causal relations can enter into the reckoning of the "crush of thoughts" though. Cioffi notes that the synoptic array, the general view of the crush of thoughts that is the aim of the self-analysis, can encompass factors which stand

in a causal relation to the life of the subject. He says that these causal relations do not raise "evidential questions in any acute way" (p 171), because not only do they influence thoughts, feelings, fantasies, and so on, but they also participate in them.

What does Cioffi mean by this rather cryptic comment? He produces it after discussing the way in which Constable accounted for the development of his vocation. Constable said that he loved "the sound of the water escaping from mill dams, willows, old rotten planks, posts and brick work" and that these scenes had made him a painter. Constable knew the source of his impulse to paint, and Cioffi implies that this knowledge is a form of causal knowledge, which indeed it is.

However, in other parts of the paper Cioffi refers to causal knowledge in a rather different way. He mentions it in the discussion of Freud's dream analysis, where he makes reference to the fact that the dreams Freud deals with have been obtained from a patient. The fact that the dreamer is a patient (p 162) and therefore has a problem of some sort seems important, since there is a need to, in some sense, solve their problem.

If the causal relations into which a phenomenon enters, it is a matter of urgency to elicit, as in psychopathology, then, however intrigued by the reverberations of the experience, we would eschew "further descriptions" and "similes" and "words that sum it up" and pursue empirical investigations.  
(p 162)



Similarly, in the course of his discussion of past influences on our development, he says that he can imagine a context in which causal questions are all-important, and synoptic ones frivolous:

...If we were conducting an epidemiological investigation of childhood traumas in the hope of devising prophylactic child rearing regimens.  
(p 169)

Again, Cioffi has shown that although causal questions are often relevant, many of the "problems which trouble us" are not to be solved by an empirical investigation; more information will not help us to straighten out our crush of thoughts. However, as he says, there seems to be a "natural" inclination to indulge in empirical speculation, even when this speculation is not appropriate.

Could Lewin's waiter seem naturally to call for empirical speculation? What happens seems mysterious, the way the memory vanishes when no longer needed seems to call for an investigation. "How does this happen?" "What is going on in his memory?" "Are other people the same?" However, there is no a priori reason why the waiter should elicit an empirical investigation. We could just sort out our crush of thoughts on the subject. The phenomena itself does not seem to call for any particular reaction from us.

Cioffi does not offer a suggestion as to how we can decide what is an appropriate explanation in a given case

where it is not clear one way or the other. What he does show is that there are different ways of explaining others and ourselves in the world, and that these methods can co-exist quite properly along side one another. He suggests also that sometimes empirical solutions to problems are appropriate, and sometimes not, raising the problem of the appropriateness of social psychology and Lewin's empirical, causal answers to the questions which trouble us, or rather, which trouble them.

It has already been suggested that methodologically, Lewinian social psychology has flaws which make its findings untrustworthy. What we need to do now is to discuss whether conceptually it fares any better. Although its use of empirical and causal methods may be "natural" in as much as they could be "called for" in some way, as Cioffi would say, this does not mean that its methods are appropriate to the questions it tries to study. Cioffi raises the spectre that empirical investigations are sometimes otiose. We will now see whether social psychology can justify its style of inquiry, or whether the methods it uses and the problems it studies pass one another by. Cioffi's paper suggest therefore that the possibility exists that social psychology's mode of inquiry might be inappropriate for the subjects it is used to study, and that this mistake occurred because Lewin was trying to answer questions which seem relevant to him, but not to others.

We will see now whether a case can be made which will show the inappropriateness of Lewinian concepts and

methodology when dealing with human social behaviour.

#### CHAPTER TWELVE: FOOTNOTES

[1] Frazer, The Golden Bough, (1890).

[2] In connection with this, it is interesting to note that G.W. Allport, in Personality (1937), already mentioned, reports a study by an experimental psychologist, E. Toulouse, of the mathematician Poincare. (E. Toulouse, Henri Poincare, 1910). In this study, Toulouse undertook an analysis of the psychological functions of a single individual, in terms of deviations from the average. He found that Poincare had a memory span of eleven digits, that his associations with numbers were prolific, that he suffered from insomnia and seemed obsessed with his work, amongst other traits. "Surveying his efforts Toulouse admits that the genius of Poincare was somehow provokingly absent from his 'synthesis'." (Allport: p 12)

## CHAPTER THIRTEEN

We are all social psychologists...What then is the need for a social psychology which, on the face of it, could only restate what we already know by making more explicit the knowledge and accumulated experience shown in our daily behaviour? (p 21)

The above remark was made by Tajfel and Fraser as part of their introductory text to social psychology. The remark is indicative of many of the tensions which centre around social psychology's claim to give a "deeper", or a more sophisticated, understanding of human behaviour than that given in "common sense" explanations. We have discussed how Robinson has argued against a scientific psychology, and we have noted Cioffi's observation that there can be a mode of transaction with the phenomena of our lives other than the purely empirical, factual, and causal investigation which is often offered by science. What will now be done is to use Tajfel and Fraser's argument for the relevance of a scientific social psychology as a means of discussing social psychology's claims in a slightly different way from the above examples.

It will be shown that the way social psychology sets out to provide knowledge about social behaviour is a response to our ordinary explanations of social behaviour. It sees ordinary knowledge as being inadequate in a particular fashion, the way this knowledge fails prompting or inviting a particular response from social psychology, namely a general, scientific one. It will be suggested that the failure in knowledge which social psychology tries to deal with is

analogous to a Cartesian skepticism. Such a skepticism is "exaggerated" though, as various philosophers have pointed out, and likewise, it will be suggested that the skepticism which social psychology takes as its starting point is over-extended, and therefore can be misleading. The methodology social psychology uses is inappropriate, because it is not necessarily invited by the doubt it combats, and it is actually counterproductive, because in its supposed sophistication it over-rides so many distinctions, which are ordinarily made, and which seem vital to a real understanding of behaviour, that the theory which is created is at such a general level that nothing is actually explained about behaviour.

At this point it is worth mentioning that nothing special is being claimed about ordinary explanations here. The everyday, or perhaps non-scientific, explanations which people use about their own and other people's behaviour are not beyond suspicion. Very often we are satisfied by such explanations, but there are times when they are obviously inadequate or wrong. Sometimes we can be at a loss to explain behaviour, and this shortcoming can be remedied only by knowledge which we could not obtain without systematic study.

Being wrong is part of ordinary explanations. We are not always altogether surprised when we find that we were wrong about someone, and sometimes it is essential and right to doubt common sense explanations. Likewise, we are not too worried when a friend's explanation of their behaviour seems

to us to be incorrect. We accept that others, and ourselves, can be mistaken about the motivations for behaviour. When this happens, the particular explanation is discarded, and another one is attempted, which seems in some way better, and which might be accepted as a correct explanation of the behaviour. There are no fast rules for succeeding in this task. What social psychology does however is to reject all ordinary common sense explanations, in what ever way they are presented, in favour of a single method of explanation involving causes, mechanisms, and social forces, which we as individuals are unaware of. Thus common sense explanation as a whole, embodying the many distinctions people make between causes and reasons for behaviour we have seen in Hart and Cioffi, is challenged, and the single model of explanation imposed on behaviour instead. We will now discuss how this rejection comes about, and whether it can be fully justified.

Tajfel and Fraser use the remark noted above as a means of emphasizing the fundamental paradox of social psychology, the paradox of a subject which studies what we all already know. We know why we like and dislike people, we know how we make decisions, and we know how to behave in groups, as well as many other things which the social psychologist studies [1]. We know how, as individuals, we interact with society. Why then do we need a science to tell us what we already know? As we saw earlier, social psychology is not like physics, giving a complementary explanation of everyday things. It explains what we all ordinarily explain,

and claims that its explanations have more truth than everyday ones.

Tajfel and Fraser recognize that most of us seem quite capable of dealing with our social environments, an observation which implies that most of us are at least competent to a degree in the skills and abilities which they discuss. They note too that it is not difficult to find examples of the restatement of the obvious in the research and writings of social psychologists. They say that this restatement is inescapable, given the nature of the questions they ask about social behaviour and experience, "the principal aims of social psychology being to study, as systematically as possible, various aspects of the interaction between individuals and social systems, small or large, of which they form a part" (p 22). Having said that part of social psychology is just a restatement of the obvious, of what was already known as common knowledge, they go on to say that "it may be necessary to point out that much of what superficially appears obvious becomes less so on closer inspection" (p 22), the implication being that when social psychology provides this closer inspection, what was already known and thought obvious turns out to be neither known nor obvious. Thus, these explanations need to be replaced by a deeper, more sophisticated system of explanation.

To support this contention, they take an example from the experimental literature concerning conformity. When faced

with making a decision about some information they have been given, it has been found that people are sometimes influenced by the majority of the members of the group they find themselves in, and sometimes they are not. They suggest two "obvious" reasons why people are not influenced by others. On the one hand, if very clear information is given, then a person should be reasonably sure of their judgement, and so will be less likely to yield to the pressure of the majority opinion. On the other hand, if the information is unclear or ambiguous, then the person might feel that their opinion is just as good as anyone else's, and so would again be disinclined to yield to the majority. Tajfel and Fraser admit, "both the above statements are common sense, and there is no doubt that each of them is valid in some conditions" (p 22). They are not really interested in these conditions however. An individual might say that they stuck to their opinion for a particular reason, but this is of secondary importance for the social psychologist. They express social psychology's particular interest in conformity thus:

The fundamental question concerns the social factors which determine whether an individual will interpret information as being "clear" or not; to what extent, and in what circumstances, can overwhelming social consensus lead to certainties which may replace the search by individuals for their own independent information about various aspects of the world in which they live? In other words, to what extent is the very notion of "clarity" inescapably social? (p 22/23)

This example shows very starkly the kind of investigation social psychology is involved in. It begins with the observation that people conform to the opinions of



others with surprising ease, in the face of sometimes plainly contradictory evidence. It recognizes that, in everyday terms, there may be conflicting explanations of such conformity, and it then attempts to resolve this conflict by empirically investigating the notion of clarity and the effect of social pressures on people's perceptions and decisions. Thus, it goes beyond, or deeper than, ordinary explanations, and tries to find the link between the two answers, the principle which will show that the two answers are not incompatible, but are really part of the same, deeper answer. The obvious analogy here is with Newton's laws of motion, already mentioned. The principle, once discovered, will then apply to all people in any situation one cares to mention. Discovering what it is that makes information clear will enable us to say what it is that makes anyone and everyone maintain their opinion in the face of opposition.

Social psychology's kind of question is of course only one possible approach to conformity. Another line of thinking might be similar to someone like C.L. Stevenson, who would argue that there is no way to prove that values or preferences are rationally right or correct [2]. If there is no rational way to show that a value or preference is right, then the explanation of why we prefer X to Y may be psychological rather than logical, which means, in these terms, that there could be any number of reasons why different individuals prefer one thing to another. Therefore to ask, "Why do people prefer X to Y?" has no simple answer, and nor would "Why do people conform to the opinions of others?" This argument cannot be discussed in detail, but it

is important to be aware that social psychology's approach is not one that is necessarily shared by other inquiries, a fact which highlights the source of its approach in the scientific endeavour.

The kind of "essentialism" social psychology advocates does have the consequence though, of not taking the individual's answer fully seriously. The individual answer is just part of the more general principle, of which the individual may know nothing. The answer given by the individual may be true, but it is not essentially true. The underlying principle or law is what is true, the formula which shows the factors determining whether this individual interprets information as being clear. What the individual tells us is only part of an answer, the true answer applying generally, and being "deeper" than the answer given by any one person. This belief is engrained in social psychological thought, and we will see more examples of it in due course.

It could be argued that there are many good reasons why we should not accept common sense explanations at face value. As Russell pointed out, following Descartes [3], our beliefs about the existence of objects, tables, etc., could be mistaken, as could our beliefs about other humans having thoughts, being hungry, and so on. Philosophers have often attacked our common sense beliefs about the world, saying, for various reasons, that it is possible to doubt them, and that they are confused. The philosopher's desire for certainty prompts these attacks.

Similarly, the social psychologist looks for certainty in the study of human social behaviour, or looks for a better understanding of it than ordinary people have. There seem to be two main reasons which social psychologists use to justify their claim that ordinary, everyday explanations are inadequate. Firstly, they cite the confusion which exists in common sense explanations, saying that such explanations often contradict each other, are inconsistent, and so on, as is shown in the conformity example above. The claim that social psychology then makes, that it can in fact discover basic principles which will undermine these conflicts by providing an all-encompassing singular analysis, is in fact a metaphysical assumption, as we will see later.

The second reason why social psychologists adopt their stance towards ordinary explanations is Lewinian in nature.

For example, Tajfel and Fraser suggest that much of our behaviour can be ascribed to "the exercise of our individual choices in the face of changes continuously presented to us by our environment, and in ourselves" (p 23) [4]. Although they say that they take seriously our feeling that we ourselves determine what these choices should be, they assert that this feeling is not decisive with regard to our choices, because much of our behaviour derives from its social background and context, even if we are very often subjectively unaware of this fact.

And thus, the subjectively "obvious" reasons or causes for doing one thing or another are like the tip of an iceberg; a great deal of what is submerged can only be understood if our individual choices are set against the background of their social causation. (p 23)

Lewin's, and social psychology's, insight, that behaviour can be related to the individual and their environment, is given a twist here, and is used to justify calling ordinary explanations of behaviour inadequate. The point being made is that very often there are aspects of the "psychological field" (in Lewin's terms) which can influence the individual without them being aware of it. Again, this would seem to be a metaphysical claim, in that it is being claimed both that such influences are pervasive and ever-present, and that in some sense, the individual as an individual cannot bring these influences to their own attention.

Lewin, and social psychology, uses the observation that we are very often unaware of the influences being exerted upon us, which is a general observation, to suggest that a general doubt is justified, that behaviour can only be understood with reference to the social forces working on us. The general example is used to explain or justify what could be called a "theoretical" general doubt, because it has not yet been shown conclusively that the inadequacies which do exist in our knowledge should lead necessarily to a general doubt about the adequacy of our explanations of behaviour. A suspicion is raised, but such a suspicion need not produce the consequences Lewin and social psychology argue for, as we

will see.

In order to combat the inadequacies they see as existing in our explanations, Tajfel and Fraser recommend that one should question what is normally taken for granted, and adopt "the stance of a visitor from Mars" (p 22). In ordinary life, the argument goes, people do not do this. They put their faith in, they believe, their feelings about their choices, without acknowledging social factors as causes of their behaviour. We could say that this faith is expressed in the way people accept explanations they are given about other people's behaviour. Ordinarily they would not doubt the truth of an explanation if they had no reason to doubt the sincerity of the person giving it, or if it seemed to them to make sense. If they did doubt it, they would not offer a "deeper" explanation, they would give their own alternative explanation of the behaviour, expressed in much the same terms as the original.

However, the task of a Martian in eventualities such as this would be to observe the entire situation in such a way as to be able to see the social factors which are at work on the individual, and to be able to acknowledge the causal nature of these factors. The Martian, in being able to see what we as people cannot, is an "alter ego" of the social psychologist, who also aspires towards acquiring this ability.

Social psychology, as exemplified by Tajfel and

Fraser, seems to wish to use the Martian analogy to show that scientists, social psychologists, are objective, and that they do not allow their preconceptions and interests to play a part in an explanation. As a reminder that scientists are in this sort of business, the analogy with the Martians holds some water, but if taken too seriously it can cause problems for the social psychologist. For instance, a Martian who is totally objective, who is completely detached from the scene it is observing, will understand nothing of the meaning of behaviour. Behaviour will merely be a series of movements. At the moment I am writing part of this thesis, and I could give reasons and justifications for why I am doing so. An objective, "Martian" account of what I am doing would, or could, consist of noting that I am making marks on a surface. Assuming that the Martians are truly alien, they would not know what I am doing by making such marks. They could describe my behaviour in a sense, but they could not understand it, nor could they understand my motivations for doing it [5]. For example, they would not know, or could not tell, the difference between writing and doodling or scribbling. Therefore, if the model of inquiry which social psychology wishes to use is based on the Martian analogy, it could literally tell us nothing about people's actions, because it will not be able to acknowledge behaviour or action, only movement. Such a strict empiricism would therefore defeat the purpose of using it.

Tajfel and Fraser recognize the danger in advocating this brand of empiricism, noting that a social psychologist cannot do their job properly if they rely on an alienated

"objective" stance, but being wedded to the idea that ordinary explanations are fraught with confusions and doubt, they still maintain that social psychology cannot rely on an analysis of social behaviour consisting only of a re-description and systematization of the accounts given by the participants of their own actions. They characterize this dilemma as follows:

In the first case, we would have an over-deterministic push-button image of social Man; in the second, we would deny ourselves the possibility of searching for explanations which are not accessible to subjective individual experience. (p 23)

The debate over how best to resolve this dilemma was very much at the forefront of Tajfel and Fraser's thoughts [6]. They themselves admit that the social psychologist "cannot miraculously combine objectivity and empathy in a uniquely wise conceptual scheme" (p 24). What they do suggest however is the use of "intellectual crutches", which for the social psychologist are the research methods that are available to be used (p 24).

The research method discussed in this thesis has been the experimental analysis of behaviour, and it has been shown that this analysis has been derived in large part from the Lewinian tradition in psychology and social psychology. Tajfel and Fraser's analysis of the needs of social psychology goes a long way to showing why the Lewinian tradition has been used so enthusiastically in social psychology. Lewin's methodology seems to straddle the horns

of the dilemma faced by social psychology. On the one hand it allows that the individual's perceptions of situations must be taken into account, meaning that an analysis of behaviour in terms which the actor and others understand, in terms of their psychological field, is permissible, while on the other hand, an objective, experimental method is legislated for, meaning that findings can be generalized across situations and individuals to present law-like statements relating to behaviour.

Although we have touched on philosophical reasons for doubting beliefs, and social psychological reasons for doubting explanations of behaviour, it should always be remembered that these sophisticated doubts are formed and expressed over a less sophisticated layer of skepticism, the ordinary doubts we all have on occasion. Part of the motive force of social psychology is the feeling that social behaviour is very often mysterious and hard to understand. The frustration engendered by our ignorance about social behaviour is aroused in many ways, as touched upon earlier. We can wonder why our friends do the things that they do, why they behave towards others in the ways that they do, sourly, defensively, rudely, and so on. We might wonder why they hold such strange opinions, or we might not understand why they hold the opinions that they do, which is a slightly different matter. This observation relates to what Cioffi said about being "assaulted" by other people. The behaviour of people we do not know can be even further from our understanding. Why are so many people prejudiced? Why are they so aggressive? So unresponsive? Why do they do the good/bad things that they



do?

The problem does not reside solely in our understanding of others though. There are times, possibly many times, when we are at a loss to explain our own actions. We can be surprised at our anger when somebody jumps the queue ahead of us, or makes a flippant remark. We can wonder why we do things we resolved never to do again (drink too much, swear at pedestrians when driving a car) and we can be at a loss to explain our feelings about people or things. Noticing that we often appear unable to understand our own behaviour, the behaviour of people close to us, and that of the mass of people at large, can lead to the feeling that this failure of understanding is located within us, and is inherent, that we cannot escape from it. If this is the case then we obviously need help to understand others, and ourselves. What social psychology promises to do is to help us to understand people better. This promise is highlighted in cases where social psychology tries to help us to recognize occasions when people are deliberately deceiving us, in order to enable us to discover what they really want from us, what they really feel etc [7].

Having noted the temptations which exist for considering our ordinary knowledge of behaviour inadequate, it will be suggested that it is not necessary to regard our inadequacies as all pervasive, a position which will have consequences for social psychology's claims. This will be shown by using an analogy with philosophical doubt and

skepticism, which will show that it is possible to acknowledge doubts, but not to succumb to them entirely. To show this, arguments advanced by various philosophers will be discussed. By using their arguments it is not being suggested that they would agree with each other, or with the use their arguments are put to in this thesis, but there is much to be gained from attempting to use what they say as a way of picking a path through the thorny problem of doubt and everyday knowledge. Therefore, the Cartesian model of doubt will be set up, and then some antidotes to this position, which seem to be effective in neutralizing such an omnipresent doubt, considered.

In The Meditations [8] Descartes tries to find what he can know as being "most true and certain" (p 145). To do this, he believes it necessary to destroy all his former opinions and beliefs, and to accept new beliefs only if he can show that they are "certain and indubitable" (p 145). Any beliefs which do not meet these stringent criteria will be discarded, therefore leaving him with unshakeable knowledge of the world, since once established, he will not be able to doubt this knowledge. He uses the device of "some evil genius not less powerful than deceitful, who has employed his whole energies in deceiving me" (p 148), as a means of trying to "avoid giving credence to any false thing" (p 148). He argues that this ploy will help him avoid false beliefs by making sure that he only accepts as true what he is sure that the evil genius has not deceived him about.

Descartes suggests that everything we have learnt has

been learnt either through or from the senses, and because it is obvious that sometimes these senses can deceive us, they are not to be trusted to give us indubitable knowledge. Although he recognizes that this assertion sounds rather odd, and that people who claim equally strange things such as imagining "that they have an earthenware head or are nothing but pumpkins or are made of glass" (p 145) are called insane, nevertheless, because there appears to be no way to distinguish wakefulness from sleep, making it easy to mistake reality for dreams; and also, because it is possible that God (or the evil genius) has created the world in such a way as to make things seem to exist to us when in fact they do not, this means that "there is nothing in all that I formerly believed to be true of which I cannot in some measure doubt" (p 147/8).

It is clear that Descartes is looking for some criterion of knowledge which does not depend either on the sense perceptions that he has of the "external" world, nor (originally) on an appeal to an external agency such as God, since it is possible to doubt both, at least in principle. What he has done is to give reasons to suggest that what we ordinarily believe is open to doubt, the kind of reasons he gives being important, as we will see later.

His argument contains a crucial element. He suggests that because our senses can at times deceive us, we cannot trust these senses, and therefore we can never really be sure of what we know about the external world; nor, more

importantly, can we even be sure that our own bodies exist, since we learn of these through our senses as well, and we cannot trust these senses. To put this another way, the position entails that the hypothetical "evil genius" is able to convince us that these things (objects, our bodies) exist, when in fact they do not. The parallel with social psychology is clear; Descartes rejects all his previous beliefs because they are not to be trusted, in the same way as social psychology suggests that the social causes of our behaviour are hidden beneath the surface, meaning that our "surface" (subjective) reasons for, and explanations of, behaviour, are inadequate, and therefore not to be trusted.

We find that in both Descartes and social psychology this rejection runs very deep. Descartes rejects everything he "knows" about the world and everything in it, even his own body, whilst social psychology rejects what we take to be our knowledge about other people, and about ourselves, our own motives, and the way we get along with other people. Both rejections, if unalleviated, would be crippling in their scope and seriousness.

Having consigned us to unrelieved skepticism, Descartes moves on from what he terms first knowledge, the only real knowledge that I can have, that I exist as a thinking being, to prove that God exists, that He does not deceive us, and that since God is no deceiver, other things actually exist which correspond to my perceptions of them. Having brought God into his argument to relieve an outright skepticism, he admits that even so, perceptions may still be

incorrect or mistaken as originally noted, but he argues that this is just a result of failing to exercise my powers of judgement properly, not because it is impossible to know the world correctly. He allows that there can be occasions when our knowledge claims fail, but now suggests that these failures no longer have widespread ramifications, but are limited in scope, and perhaps can be overcome. Social psychology too claims that our ordinary "knowledge" may be systematically incorrect, making the important further claim that it is social psychology itself that can bolster our powers of judgement sufficiently to enable us to know the world. In both cases two claims are being made; that what we ordinarily take to be beyond doubt may not be so, but that we need not fear this too much, because there is a way by which we can have a true understanding or knowledge of the world and what happens in it.

If we stretch the analogy between Descartes' argument and social psychology further, we can see that social psychology in a sense performs the same task in the scheme of things that God does. In both cases we have beliefs which are not to be trusted. For Descartes they are dubitable because they have been collected through fallible senses, which means that we do not know, or cannot be sure, which of them are correct. In social psychology we arrive at our beliefs on the basis of information which may be inadequate, which we may use incorrectly, or which may just be plain wrong. To prevent us from being systematically deceived, Descartes introduces God, who may make sure that what our senses tell us

corresponds to reality, while social psychology introduces the notion of psychological facts, which relate to people, and which social psychology itself can give us access to, to prevent us from mistaking our ordinary explanations of behaviour for "real" explanations. Or to put it another way, we could say that social psychology is an instrument of God, an instrument which makes sure that we exercise our powers of judgement correctly. In any event, in both cases doubt exists, and a solution is proffered which will assuage this doubt. More importantly, this doubt is general, in that it covers every example we can think of, relating to the senses for Descartes, and to the explanatory generalizations in common usage for social psychology.

O.K. Bouwsma [9] offered an antidote to the extreme skepticism which can seem invited if Descartes is taken seriously. Bouwsma spells out Descartes' blunder thus:

What is crucial in the language of the argument has, by way of Descartes' philosophical misgiving, been cut off from all significance. Until this is understood, it may seem that Descartes' predicament is a predicament of ignorance, that he actually does not know whether he is awake or asleep. (p 322)

Bouwsma suggests that Descartes' predicament is an illusion, created by the method he uses. The origin of Descartes' misgivings means that he defines the question "Am I awake or am I asleep?", which is crucial to his argument, in such a way that it has to be answered without employing the senses at all.

Bouwsma believes the central puzzles Descartes deals with can be expressed in the question "Am I awake or am I asleep?" Descartes finds that he has previously been deceived by dreaming that he was awake, when he was in fact asleep and dreaming. Being struck by the possibility that seeming to be awake might in fact be a dream, he concludes that "there are no certain indications by which we may clearly distinguish wakefulness from sleep" (p 145). Before noticing this possibility, he had felt that there were a number of things about which we could not reasonably have any doubt, "for example, the fact that I am here, seated by the fire, attired in a dressing gown, having this paper in my hands" (p 145). Since his observation that he had been deceived by his dreams in the past however, he felt that he could no longer rule out the possibility of doubting these apparent facts.

Descartes' project was to attempt to find the best possible case of knowledge, and as part of this project he made a distinction between the case of being seated by the fire, about which there could be no reasonable doubt, and which would therefore be an example of what we would call a best possible case of knowledge, and cases where we encounter "things which we would all readily agree are hardly perceptible or very far away" (p 145). In this second type of case, we could all agree that the senses could deceive us, in the sense that we can be mistaken about such things. Examples such as "those towers which from afar appeared to me to be round, more closely observed seemed square" (p 189), fall into this second category [10].

Bouwsma alerts us to a significant aspect of Descartes' argument, related to the distinction he makes. Descartes points out that it is easy to remember making mistakes about distant or very small objects, an argument that carries some weight, since most people would admit to having made such mistakes themselves. Bouwsma notes that Descartes might continue his argument by saying, "so too I remember your flicking your ashes into a bowl of soup which you mistook for an ash tray, and so you cannot trust your eyes to distinguish things near by either" (p 314). Bouwsma makes the following observation about this hypothetical step in Descartes' argument:

But Descartes does not say this. He does, of course, say that concerning the bowl of soup you may have a reasonable doubt, but not because you have ever been deceived about a bowl or an ash tray. (p 314)

When objects are a long way off they are always hard to identify, and we are not unduly surprised when they turn out to be other than we had supposed when we inspect them more closely. On the other hand, objects which are close enough to be touched, or to flick ash into, rarely prompt us to call the accuracy of our senses into question.

By pointing out that Descartes can appear to raise a reasonable doubt about knowing that we have a bowl of soup in front of us, Bouwsma is illustrating the way in which Descartes' argument works. If we take seeing the bowl of soup or the ash tray as another instance of a best possible example of knowledge, this means that we could say that we



would not ordinarily make a mistake about it unless we were in a dark room, or not paying attention to it etc.; we could say that if we take care, we could not mistake the one for the other in good light, and so on. Bouwsma's remark emphasizes the fact that even in a case like this, where an object is easily seen, and not far away, even then Descartes' argument allows that we may have a reasonable doubt about it, though importantly, not about what it is, but about whether it exists at all.

A reasonable doubt in this context is one which it is possible to articulate. Descartes mentions people who seem insane in his discussion, to emphasize the difference between the doubts that they might have, and the doubt he himself is unable to contain. Descartes' doubt can be shared, because he is able to give a reason for the doubt, which in turn enables us to understand what his doubt is, and to enter into it. The insane on the other hand, cannot share their problems in this way, and therefore observers do not accept their doubts as real, or as being reasonable. By giving a reason for doubting sense perceptions, Descartes allows us to share his problem. Crucially though, it also allows us to overturn this problem, because the opportunity exists to show, as Bouwsma tries to show, that the reason for the doubt, the problem, is not a good reason, thereby meaning that the doubt it provokes is not reasonable, and thus, in a sense, "insane".

The difference between a reasonable and an insane doubt often seems quite clear. When we are in a good position

to see what an object is, when we are close to it or can examine it thoroughly, we can normally say minimally that it is an object, even if we do not know what the object is. In the tower example, we could be unsure about what we are seeing from a great distance; it might be a tree, or a cloud formation on the horizon that looks like a tower. When we are closer to it though this doubt evaporates. We know at least that it is a physical object because we can see and touch it, even taste and smell it perhaps. We can doubt its purpose and function, and we might not know its name or what it is (we might not be able to identify it) but we do know that it is there. We can doubt our knowledge about the object, but not our knowledge of its existence. Ordinarily we would say that it exists, whatever it is, and in most cases we would say, "Yes, that bowl of soup, or that ash tray, exists, without a doubt".

One occasion when we are close to an object and we could be said to doubt our senses (at least in a metaphorical sense) is when the object is disguised as something else. A telephone may be disguised as a Mickey Mouse or a hamburger, a box may be disguised as a book, but in these case we would not say that our senses had deceived us, we would only say that we had been wrong about what the object really was. Our senses could tell us the colour, shape, size, weight, texture, etc. of the object, and on a closer examination we might find that there was something about the object which did not fit in with its "pretended" appearance. The hamburger might feel hard and heavy, and may make a ringing noise, suggesting to us that our first visual impression might be

incorrect. Our senses would not deceive us; they might be perfectly accurate, but we could still be mistaken about the object. Such a mistake would not mean that we would doubt our senses, but it might mean that we would be careful around hamburgers for a while. Examples like these do not lead us to doubt all our senses and what they tell us, they just prompt us to be more circumspect, or more thorough, or more careful, so as to avoid making similar mistakes again. We do not subject every member of these classes of perception to suspicion. We might recognize cases of this sort as another example where a reasonable doubt is intelligible, but such a doubt need not apply to the bowl or the ash tray. We can in practice be convinced that what we have before us is indeed a bowl of soup.

If this is the case, we need to ask how Descartes produces a "reasonable" doubt about the bowl of soup and the ash tray, when he would admit that we have never been deceived by them in the past, and how this doubt is distinguished from an insane doubt.

He in fact produces this doubt by referring again to the problem of distinguishing between sleeping and waking. If what could be beyond reasonable doubt, being seated by the fire, was not really beyond doubt if you are dreaming it, then any other perception could be wrong in just the same way. It is important to recognize here that, as Bouwsma says, "the argument does not depend in any way upon any instance of deception" (p 314).

It's as though Descartes asked: And do the senses deceive us? To which the answer is: Never. He goes on: Are the senses then to be trusted? To which he answers: Certainly not. This illustrates the unusual nature of the argument. (p 314)

Thus, what we might call the best possible cases of knowledge, sitting by the fire, having a bowl of soup or an ash tray in our hands or on the table, which we have never been deceived about, are not enough, are not sure enough, to prevent us from being able to doubt what we see, feel, hear, etc. What we know to be the case may not be so, what we know to exist may not, if we are only dreaming it.

Once a doubt of this kind has been raised there is no way to forestall it, and it covers the categories of both reasonable doubt (far away) and no reasonable doubt (sitting by the fire, flicking ash into the wrong receptacle). The question this leads us to ask is, can we really raise such a doubt? Does it make sense to raise a doubt of this kind? As Bouwsma says "We have then to consider what fact it is, about all perceptions whatsoever, that constitutes a basis for reasonable doubt concerning any perception" (p 314), and he outlines Descartes' argument for the existence of such a fact as follows.

Descartes begins with two facts, that he is sitting by the fire, and that he has dreamed that he is sitting by the fire. "Now what strikes Descartes is the likeness of the fact to what he has dreamed" (p 315). Having been struck by this likeness, he loses confidence in his previous judgement,

that he was awake, and he can now only say that he "seems" to be awake. However, if this is the only claim that he can make it does not amount to anything concrete, because we have to remember that in the past he has seemed to be awake when in fact he was asleep. Thus he cannot tell whether he is here by the fire, or asleep and dreaming it all. Therefore it is possible to doubt what we previously could not doubt.

Bouwsma invites us to notice something about Descartes' argument, namely, that the question "Am I awake or am I asleep?" is either a very peculiar question or else it is readily answerable (p 317). We know how to go about finding an answer to the related questions "Is he awake or asleep?" and "Are you awake?" There are ways of finding out from the available evidence (you can tell by the depth of breathing for example, or simply by asking) [11]. We can be mistaken, but this is due to a failure to understand the evidence properly, not due to a failure of the senses generally.

Next Bouwsma considers the question "Am I awake?" and finds that we might ask ourselves this when for example we "could not believe our eyes" (if we were to meet Elvis Presley in a supermarket) or in cases when we are genuinely puzzled:

There are experiences of waking out of a dream where the vivid peril of one's dream has not yet passed and where the safety of the real world is not yet clear, in which one may quite earnestly and fearfully and hopefully ask: Am I awake or dreaming? (p 318)

We would only ask this question when, as Bouwsma says, "there is an awareness both of the strange encounter in the dream, and of something else vaguely distinguished in contrast with that encounter" (p 318), for instance, the pillow under your head and the ticking of the alarm clock.

The motive behind Descartes' question is nothing like the awareness of a strange encounter though. He is not lying down snoozing; he has not been asleep, dreaming, and failed to recognize his bedroom; there is no present fading dream and no present emerging world. What prompts his question is the "awareness of a certain fact, namely, that one may have dreamed something very much like what one now perceives to be the fact" (p 319).

How can we put Descartes' particular question "Am I awake or am I dreaming?" to the test? How can we gather evidence to provide an answer? In the examples Bouwsma provided, the context in which the question arose was that of the real world, and the real world furnished the criteria by which waking and sleeping were determined. Descartes accepts this; he knows that open eyes are an indication that he is awake, but this is no longer enough to satisfy him. His eyes may seem to be open, but he might only be dreaming that they are, and for any test that one can think of, feeling the pillow, hearing the clock tick, listening to breathing, the same will be true. It is possible that he is just dreaming these things.

Bouwsma draws the following conclusion from this discovery about the implications of Descartes' way of arguing:

Clearly Descartes has abandoned the original distinction, the fact that I am here, etc., and the fact that I dreamed that I am here, etc., upon the basis of which he elaborated his argument. In order to find out whether he is awake or not, he must depend upon facts of the sort he began with. He continues however to use language which is significant only in terms of the distinctions which he has abandoned. The questions "Am I awake?" "Am I asleep?" are questions about bodies. Since he has ruled out bodies as within the range of the application of these terms, his language is now meaningless. (p 321)

Although the language Descartes uses is simple and ordinary, he uses it in such a way that you have to answer the question "Am I awake or am I asleep?" without employing the senses. Without the senses it is an impossible question to answer. Descartes' predicament then is not one of ignorance, but of ignoring, or ruling out of bounds, what might give the answer to the question he sets himself. Therefore, the skepticism which can appear to be aroused as a result of his argument is not an appropriate response to the argument. What Bouwsma has shown is that the reasons Descartes has given for believing that it is possible to be deceived by the senses on all occasions are without foundation. If Descartes were asking "Am I awake or asleep?" in an ordinary context, it would be possible to answer him. As things stand though, the peculiar way in which he asks his questions means that no answer can satisfy him. The reasons he gives to support his claim that we can be systematically deceived seem therefore to be inadequate, so it seems safe to

assume that the doubt which he tries to provoke need not be omnipresent. There can indeed be reasonable doubt about our knowledge in some cases, but such doubt cannot properly be extended to cover cases where we would ordinarily say that no such doubt exists. His argument really has more in common with those poor souls who think that their heads are made of glass than he would care to admit.

There are aspects of Bouwsma's arguments which are also found in the work of J.L. Austin. In his paper "Other Minds" [12], Austin discusses what we would say if we were asked "How do you know?" after having made statements claiming knowledge. His interest is in questions such as "How can we know the thoughts, feelings, sensations, and mind?" of another creature, this interest leading him "to consider what sort of thing actually does happen when ordinary people are asked 'How do you know?'" (p 45).

He chooses to discuss the business of "how we know" by using the statement "That is a goldfinch", which, if we make it, exposes us to the question, "How do you know?". "That is a goldfinch" is, he says, "not unlike" the statement "He is angry" (p 45), and we can use it as a stalking-horse to help us understand how we know that he is angry. If we can elucidate the way in which the statement "That is a goldfinch" expresses a claim to knowledge, it might, in Austin's opinion, shed some light on to the statement "He is angry", seen as a knowledge claim. It is however primarily his discussion of knowledge claims in general which will be



of interest here.

As a preliminary exercise, Austin considers the types of answer that may be given to the question "How do you know that there is a bittern (or a goldfinch) at the bottom of the garden?", and finds that in order to be able to say that we know this, there are various things which we need to have done. I must have:

- (1) been trained in an environment where I could become familiar with bitterns
  - (2) had a certain opportunity in the current case
  - (3) learned to recognize or tell bitterns
  - (4) succeeded in recognizing or telling this as a bittern.
- (1) and (2) mean that my experiences must have been of certain kinds, that I must have had certain opportunities: (3) and (4) mean that I must have exerted a certain kind and amount of acumen. (p 48)

When we are asked how we know that the bird in the garden is a bittern, we can cite our past experience of learning what a bittern is, as well as our current sighting of the bird, as showing that we know what a bittern looks like, and that this bird is one because it has those features which we recognize a bittern as having.

Being able to cite past experience, and the characteristics we recognize as being indicative of a bittern, does not mean that our knowledge is infallible though. "It may be that I have not learned the right (customary, popular, official) name to apply to the creature" (p 51), or it may be that I am confused by the number of species of small British birds. Other birds may share the

same characteristic which was used to identify this particular species, meaning that we might not be sure that this particular bird is really a bittern, or a goldfinch.

Austin suggests that asking about how we know that a bird is a bittern or a goldfinch does not involve a challenge to the credentials we give, nor have the facts we have used been disputed (p 54). This means that when we ask "How do you know?" and we receive the reply, "Because I was brought up in a place where goldfinches (or bitterns) were common", we normally accept this as an adequate way of having learnt what bitterns look like. We do not question these "credentials" of knowing. Similarly, when we say that this is a goldfinch because it has a red head, this fact is not normally disputed, although if we were actually wrong about goldfinch colouration, it could be challenged by pointing out that goldfinches do not have red heads.

Where it is agreed that goldfinches have red heads however, claiming that we know this bird is a goldfinch because it in fact has a red head is accepted as establishing a claim to that knowledge. Having established that credentials and facts of this kind validate our claims to knowledge, Austin then notes that a further sort of challenge can be made, which questions the reliability of these alleged credentials and the alleged facts we use. There are two sorts of question that can be asked as part of this challenge:

(1) But do you know that it's a real goldfinch? How do you know that you're not dreaming? Or after all, mightn't it be a stuffed one? And is the head really

red? Couldn't it have been dyed, or isn't there perhaps an odd light reflected on it?

(2) But are you sure it's the right red for a goldfinch? Are you quite sure it isn't too orange? Isn't it perhaps rather too strident a note for a bittern? (p 54)

The first form of challenge seems similar to the challenge made by social psychology to everyday knowledge. Social psychology asks "Do you really know how you came to make that decision?" "How do you know that you weren't influenced by things that you were unaware of?" Austin says that if you ask "How do you know that it is a real goldfinch?", then you are questioning credentials or facts in a special way, in that it is being suggested that either "my current experiencing or the item currently under consideration...may be abnormal, phoney" (p 55). For example, I could be dreaming, or under the influence of drugs, or else the item in question may be stuffed, painted, dummy, artificial, and so on. There can be cases in which it is not clear whether I am to blame or the item is, for instance in mirages, mirror images, or odd lighting effects.

Austin argues that doubts of this sort, about the reality of things, "are all to be allayed by means of recognized procedures...appropriate to the particular type of case" (p 55). As Bouwsma also points out, there are ways of distinguishing between dreaming and waking (if there were not, how should we know how to use and contrast the words, as Austin notes). There are ways of deciding whether a thing is stuffed or living, and so on. The important point which Austin draws from this discussion is that:

The doubt or question "But is it a real one?" has always (must have) a special basis, there must be some "reason for suggesting" that it isn't real, in the sense of some specific way, or limited number of specific ways, in which it is suggested that this experience or item may be phoney. Sometimes (usually) the context makes it clear what the suggestion is: the goldfinch might be stuffed but there's no suggestion that it is a mirage, the oasis might be a mirage but there's no suggestion it might be stuffed. If the context doesn't make it clear, then I am entitled to ask "How do you mean? Do you mean it might be stuffed or what? What are you suggesting?" (p 55)

If the questioner gives us some indication of the doubt that they have regarding the reality of the item under consideration, then Austin seems to think that there are ways and means of assuaging that doubt. If we identify the bird in the garden beyond doubt as a goldfinch, and someone asks "But are you sure it's a real goldfinch?" it is quite in order to ask what they mean by their question. They then might point out that it has not moved for an hour, or that its movements are jerky or very slow, in which case it could be stuffed or mechanical. If so, an examination of the bird will expose this fact.

Austin suggests however, that it is inviting confusion to inquire about the reality of something, when there is no reason for supposing that it is not real. He says:

The wile of the metaphysician consists in asking "Is it a real table?" (a kind of object which has no obvious way of being phoney) and not specifying or limiting what may be wrong with it, so that I feel at a loss "how to prove" it is a real one. (p 55)

His objection seems valid. We can show that a bird is real when contrasted with stuffed, mechanical, etc., but without knowing what "reality" is being contrasted with, it is hard to do so. If asked to show that we are really sitting by the fire, we would want to know why that fact is being doubted. When told the reason for the doubt, the possibility of being asleep, we can try to calm the doubt by saying that I can pinch myself and feel pain, therefore I am not asleep, and therefore I am really here by the fire. There are various ways of proving that I am awake rather than asleep, and these can be employed when we know that we stand accused of being asleep.

Having made this observation, Austin points out that ordinarily, when I say that I know a bird is a goldfinch, knowing that it is a "real" goldfinch is not in question. In the special cases where this knowledge is called into question, there are ways of discovering whether it is real or not. Part of knowing what something is involves reasonable precautions being taken to ensure accuracy in judgements "relative to current interests and purposes" (p 56) in both ordinary and special cases. Even when taking reasonable precautions, two further conditions must be acknowledged:

(a) It is not possible to know always whether it is a goldfinch or not, because it might fly away for example. "This is simple enough; yet some are prone to argue that because I sometimes don't know or can't discover, I never can" (p 56).

(b) "'Being sure it's real' is no more proof against miracles or outrages of nature than anything else is" (p 56). Events in the future can always force us to revise our ideas about goldfinches, or real goldfinches, or anything else.

Austin acknowledges that our claims to knowledge can sometimes be defeated, that we are not always justified in saying that we know something. This possibility is just a fact of life, and there is not necessarily anything that can be done about it, beyond taking reasonable precautions.

The substantial point that he wants to make though, is that this possibility should not infect our ordinary claims to know something. When somebody doubts what we claim to know, if we are to take their doubt seriously, they should be able to show a reason for their doubt. If they have such a reason we may take them seriously, and try to assuage their doubt. If they do not have any reason for doubting our claim, then the implication from Austin's argument is that in a sense we need not take them seriously. If I were to claim that a bull were charging towards me, but my friend said "How do you know? It could be a Greek God in animal form", I would not take his doubt seriously, whereas if he said "Don't you remember our friends said that they were going to a fancy dress party disguised as a bull?" it might make me take a second look at the very least.

On the other hand, Austin's argument shows how we can

defeat a doubt which has been presented to us. A friend might doubt that she will go to the big match because her car has broken down, and she has a theory about how her afternoon will be spent (watching an old black and white movie on the television). To show that this doubt is unrealistic, it is possible to argue against the doubt, by showing why we cannot share it. Since reasons explain the doubt, we could point out that a neighbour is going to the match, and has space in her car, or that a bus runs from the end of the road past the stadium. If our friend's doubt is rational, it follows that it should now disappear, since the reasons for the doubt have been shown not to support it. Also, the idea that she will spend the afternoon in front of the television will vanish too, to be replaced by the idea that she will in fact go to the match. The phantasy of staying at home is dissolved in the same way that the phantasy of goldfinches being unreal can be dissolved if the reasons for the original doubt are examined and found wanting.

The way in which Bouwsma's antidote to Descartes works is to show that Descartes' question is a disingenuous one. Descartes dismisses our claims to knowledge because we might be systematically deceived, but in the paper we have discussed, and in the other papers where he discusses Descartes [13], Bouwsma shows that there is no deception in the ways Descartes suggests, and further, he hints that Descartes must really know this all along. Austin makes the same point, but he extends it to show that challenges to knowledge claims are ordinarily made for reasons which we can acknowledge as being plausible. When plausible reasons are

given for a doubt, we can attempt to overcome these doubts in certain, roughly well known ways. Austin seems to assert that if this is so, then all "real" doubts of this kind can be stilled, but whether this assertion is true or not, he does seem to show convincingly the need for having a reason for a doubt. As he says, the wile of the metaphysician consists in not giving a proper reason for doubt, a reason which we can respect and take as a serious threat to our knowledge, and in this way the metaphysician is like the insane person in Descartes' argument, whose beliefs we cannot share.

It will now be argued that the doubt which social psychology tries to arouse about everyday explanations of behaviour should not be taken fully seriously, and that the reason it has been taken seriously is due to a confusion, one which stresses an essentialism at the expense of a differentiated view of human behaviour. It is not being suggested however, that, like Descartes, social psychology knew all along that its form of doubting was not a real one, but rather that it sees itself as having found a plausible reason for doubting everyday knowledge, when in fact it has not.

Social psychology's belief in the seriousness of the doubt it thinks it is raising can be seen in the topics it chooses to expose this doubt. Tajfel and Fraser for example, expose making decisions and judgements as being influenced by factors which common sense knows nothing about. Making decisions and judgements are ordinarily taken to be examples



of the social person acting freely: they make judgements only on the basis of their knowledge and what they see, they decide on the basis of their interests and their beliefs. If social psychology can show that even these most fundamental expressions of what we take as being constitutive of human behaviour are influenced by external and unnoticed factors, this will serve the same function as the best case of knowledge, knowing that I am sitting by the fire, in Descartes work. If our beliefs about decisions and judgements are shown to be false, then doubt is cast upon all other areas of human life.

The doubt social psychology attempts to raise should not be taken seriously, as can be seen from examining a paper in which this doubt is raised most forcefully. It will be shown that the doubt raised by this paper is not necessarily endemic in the way it must be for social psychology's claims to be substantiated.

#### CHAPTER THIRTEEN: FOOTNOTES

[1] As we have seen, the range of social psychology's interest is vast, but there is no doubt that for most of this range there are common sense explanations of the phenomena the social psychologist studies.

Revealingly, Lewin himself talks of a paradox, in a slightly different context, but one in which he makes it clear that he has recognized what Tajfel and Fraser have also noted. In "Formalization and Progress in Psychology" (1940), he discusses the need for a "theoretical psychology". He remarks that the psychologist, as an empirical scientist, finds himself "in the midst of a rich and vast land full of strange happenings" (p 3). In this land children play, people fall in love and become unhappy, people dream, are loyal to a group, "and so on without end" (p 2). This land is an immense continent full of fascination and power and full of stretches of land where no one has ever set foot. What psychology attempts to do is to conquer this continent, "to find out where its treasures are hidden, to investigate its danger

spots, to master its vast forces, and to utilize its energies" (p 2).

Lewin finds his paradox to lie in the fact that when a new continent is being explored, it must be tested by methods, or machinery, which already exist. Known methods are used to "open up" areas, which are then explored by "more elaborate instruments" (p 3). He is obviously making an argument for applying methods from other fields to the subject matter of psychology, in order to get to the facts behind behaviour, "below the surface" (p 3). The paradox he speaks of is therefore slightly different from Tajfel and Fraser's, but in setting it up he acknowledges that there is a world of appearances, of people falling in love etc., that we must get behind, because we are unable in one way or another to cope with it (being unhappy and not knowing what to do). He accepts that we understand such things, in that we can identify them, and perhaps even sort themselves out ourselves, but at the same time he calls this an immense continent, where no one has ever set foot. He therefore acknowledges Tajfel and Fraser's paradox, but from a different angle. Also, he says that the tools psychology uses, the tools that he proposes to borrow from other disciplines, have a value for psychology "only in so far as they serve as a means to fruitful progress in its subject matter, and they should be applied, as complex tools always should, only when and where they do not hinder progress" (p 4). It is my contention that the tools Lewin proposes, and social psychology uses, do in fact hinder progress, because their use is based on misleading analogies and on a confused metaphysical foundation.

[2] In Ethics and Language (1944).

[3] In The Problems of Philosophy (1912), Chapters One and Two.

[4] They also suggest that one way to view the subject matter of social psychology is as the study of this interaction between individual choice and environmental change.

[5] I take it that Wittgenstein means something along these lines when he says that "If a lion could talk, we could not understand him" (PI: 223).

[6] Tajfel and Fraser mention three books which show the content of this debate: The Social Construction of Reality. Berger and Luckman, 1967; The Explanation of Social Behaviour. Harre and Secord, 1972; and The Context of Social Psychology: A Critical Assessment. Israel and Tajfel, 1972.

[7] See, for example, the discussion of deception in Roger Brown's Social Psychology: The Second Edition, 1986, pp 502-509.

[8] In The Philosophical Works of Descartes, translated by Haldane and Ross, 1931, volume one.

[9] Bouwsma, O.K., "Descartes' Skepticism of the Senses".

Mind, vol. LIV, 1945, pp 313-322.

[10] An "interpersonal" or "psychological" example of this kind might be an experience of meeting someone who seemed to be very honest on brief acquaintance, but who showed themselves to be less honest when known better.

[11] He does not deal here with cases of deliberate deception, because as we have already noted, this sort of deception is not related to the fallibility of the senses as such.

[12] "Other Minds", in Philosophical Papers, 1961.

[13] For instance, in "Descartes' Evil Genius" (Philosophical Essays, pp 85-97) and "Remarks on the Cogito" (Towards a New Sensibility, 1982).

## CHAPTER FOURTEEN

The paper we will discuss is entitled "Telling More Than We Can Know: Verbal Reports on Mental Processes", written by Nisbett and Wilson in 1977 [1]. In this paper Nisbett and Wilson give a classic exposition of social psychology's claim that ordinary, everyday knowledge about cognitive processes, or the processes by which we come to make decisions, make judgements, and so on, is inadequate [2]. Interestingly, Pliner and Chaiken, whose 1990 paper we discussed earlier, mention Nisbett and Wilson's paper approvingly, indicating that the ideas they propound are taken seriously by social psychologists at present.

Nisbett and Wilson reveal the questions they are interested in at the very beginning of their discussion, by identifying their subject matter as relating to questions which we are all asked in our daily lives, such as "Why do you like him?", "How did you solve that problem?", and "Why did you take that job?" (p 231). Having noticed that these are questions that we can all be expected to deal with in the course of our ordinary doings, they identify the particular area they are interested in with regard to these questions.

In our daily lives we answer many such questions about the cognitive processes underlying our choices, evaluations, judgements, and behaviour. (p 231)

As social psychologists, Nisbett and Wilson are interested in the cognitive processes underlying behaviour, an interest which Lewin would recognize immediately as being

identical to his own interest in genotypical processes. The terminology may have altered, but the interest is exactly the same.

Although we may all answer questions about our like or dislike of people, and our motivations for doing certain things, and even though "social psychologists routinely ask the subjects in their experiments why they behaved, chose or evaluated as they did" (p 231), suggesting that there is something to be gained for social psychology by asking such questions; even so, some cognitive psychologists, Nisbett and Wilson report, "have proposed that we may have no direct access to higher order mental processes such as those involved in evaluation, judgement, problem solving, and the initiation of behaviour" (p 232) [3].

These "anti-introspectivist" (p 232) thinkers "doubt people's ability to observe the workings of their own minds" (p 232). Nisbett and Wilson find three problems with the structure of this view however. In the first place, they say, the anti-introspectivist writers did not cite evidence for their claims that people have no access to specifically higher order mental processes. Instead, most of their research was on the basic processes of perception and memory. Nisbett and Wilson are not surprised that people are unaware of perceptual and memorial processes. They say:

It would be absurd...to ask a subject about the extent to which he relied on parallel line convergence when making a judgement of depth or whether he stored the meanings of animal names in a heirarchical tree fashion or in some other

manner. (p 232)

They also note:

If a person is asked, "What is your mother's maiden name?" the answer appears swiftly in consciousness. Then if the person is asked "How did you come up with that?" he is usually reduced to the inarticulate answer, "I don't know, it just came to me. (p 232)

What Nisbett and Wilson suggest however, is that examples like these, involving perception and memory, do not reveal anything about the cognitive processes underlying more complex behaviours, and so they do not carry any weight in that particular argument, the argument concerning our knowledge of the processes underlying our choices and decisions. Their lack of surprise about our unawareness of perceptual and memorial processes is revealing though, as it shows the implications of the social psychologist's argument. To answer the "How do you know?" question here, involves more than being able to cite relevant experience, opportunity, and success, contrary to Austin's suggestion. The social psychologist is correct in assuming that we are unable to answer the questions they pose, but there is no answer possible, in the same way that Descartes' question has no answer, not because we cannot give an answer (we might be able to, we might be able to say how we force ourselves to learn and remember information for example) but because any answers we can give will be seen as not being good enough, because they are not answers about the process which is expected to be found. We will discuss this point later in more detail.

The second problem Nisbett and Wilson point out about the anti-introspectivist position is that it fails to account for the fact that people gladly tell psychologists what their reasons are for behaving the way they do, and so on. Nisbett and Wilson say:

It would seem incumbent on one who takes a position that denies the possibility of introspective access to higher order processes to account for these reports by specifying their source. If it is not direct introspective access to a memory of the processes involved, what is the source of such verbal reports? (p 232)

This criticism deals with the "vacuum" created by the anti-introspectivists. It assumes that the reasons people give for making decisions must come from somewhere, and that their origin must be explained. If there is no direct introspective access, then reasons must come from elsewhere, and a full account of behaviour should, in their opinion, give a clue as to where from.

The third fault they find is the inability of the view to account for accurate reports about higher order mental processes. They arrive at this criticism by suggesting that it seems unlikely that such reports are always inaccurate, even though this is what the anti-introspectivist view would seem to point to. This criticism has important consequences, as we will see later.

Although noting these problems with the viewpoint

they discuss, Nisbett and Wilson do not discard it. Instead, they accept its basic premiss, that there is a process which underlies making decisions and so on, but modify the conclusions which should be drawn about our reports of such processes, by strengthening the research on which the conclusions are based. They extend the range of research to cover higher order processes and extend the range of processes discussed, to avoid possible criticism that the research is too selective to have wide-spread ramifications. They argue for three new main conclusions: (1) that people often cannot report accurately on the effects of stimuli on higher order responses, (2) that people use "implicit, a priori theories about the causal connection between stimulus and response" (p 233), and not a memory of the cognitive process that operated on the stimuli, and (3) that even when correct reports are made, there is no direct introspective awareness. "Instead, they are due to the incidentally correct employment of a priori causal theories" (p 233).

What Nisbett and Wilson have done then, is to produce a more detailed and strengthened version of the anti-introspectivist theme, one which admits that we can sometimes report on the stimuli that affect our behaviour, but that we can never really know the processes which guide our decision making. Their account shows why we readily give reasons for our actions, and also shows why these reasons are unconnected to the true motivations for our behaviour. As Tajfel and Fraser have said, most of what is important goes on under the surface, at a level too deep for us to be aware of. The doubt which is produced about the adequacy of people's evaluations



of their own motivations, or their real motivations, is therefore total, as is the doubt about our perceptions of the external world for Descartes.

The similarity with Descartes does not end with this simple observation however. For Descartes, doubt arises from a failure, to distinguish between sleeping and waking, and for social psychology it arises in this case from a failure to report properly on a process which determines what we do, how we make a judgement, etc. As an example of this failure, Nisbett and Wilson quote a famous book, by Latane and Darley, The Unresponsive Bystander: Why Doesn't He Help? (1970), in which the authors note that the presence of other people inhibits individual's reaction to situations which the experimenter has set up to be perceived as an emergency. When people are alone they are more likely to help, or to try to help, someone who has fallen over for instance, than when other people are present, all other things being equal. The presence of other people lowers the probability of any individual helping.

Latane and Darley noted that subjects in these experiments seemed utterly unaware of the influence of other people on their behaviour, even though they were asked directly, and therefore presumably alerted to the significance of this influence.

We asked this question every way we knew how: subtly, directly, tactfully, bluntly. Always we got the same answer. Subjects persistently claimed that their behaviour was not influenced by the other people

present. This denial occurred in the face of results showing that the presence of others did inhibit helping. (Latane and Darley: p 124)

Does this fact mean that people are always unaware of the influences other people have on their behaviour? As it stands, it does not. If some of the subjects had said that their behaviour was affected by the presence of others, which surely must be quite possible, then this particular failure of knowledge would not be general. Still, the temptation to say that it is general remains. As Austin says, although some failures of knowledge do not lead to a collapse of knowledge as a whole, there are still those who treat examples like this as though they mean exactly that, the implication being that in cases such as this there is something to be gained by maintaining this position. By maintaining this position, social psychology legitimates its search for psychological processes, as a replacement for the inadequate knowledge provided by common sense.

Processes are, as we have seen, a way of attempting to describe what goes on within the person between stimulus and response, or between seeing an emergency and helping someone, between eating a meal with somebody and not eating the same amount with another person, or between being given an order and forgetting/remembering it [4].

Latane and Darley talk of an "intervention process". They say "let us consider the behavioural and cognitive processes that go on in an individual in the vicinity of an emergency" (p 31) and they also describe the "social

influence process" (chapter 5 passim), which is a way of explaining why people do not help in emergency situations. They say that the intervention process consists of a number of stages, a number of decisions. These are; noticing that something is happening, interpreting it as an emergency, deciding on personal responsibility for acting, deciding on the form of assistance appropriate, and on how to implement this action (p 31/32). They say that this is the way in which people decide to intervene in emergencies.

The socially responsible act is one end point to a series of decisions. Only by making the appropriate decision at each of these steps will the bystander intervene. (p 32)

To call this series of steps a process seems misleading however, because it implies a fixed, almost mechanical, way of coming to act. Seen in a different light, these stages are just another way of describing what might happen in cases of this sort, in emergency situations, and arguing that the presence of other people affects behaviour in a situation like this is just a description of one possibility.

For example, suppose a situation of the sort Latane and Darley construct actually happens. A room fills with smoke, or someone in the next room is heard to fall and hurt themselves. If we decide to help, this does not mean that we have gone through the kind of thought process that Latane and Darley suggest is appropriate. We may not have weighed up the pros and cons, or agonized through a reasoned process. We may

have helped intentionally though, that is to say, not by mistake, or unaware of what we were doing, and we would take full responsibility for our actions.

There are two things which should be noted here. If we were asked "Why did you do that?" we might give a number of reasons. "Something was wrong", "I was scared", "Somebody was in trouble", "I saw the smoke and thought that I should tell someone". We might even say "I don't know, I just did it". As implied above, this does not mean that the act was random, or unintentional, merely that no reason can be thought of to explain it. The same is true of answering "Why didn't you help?" Any number of reasons can be given, including, "I don't know". In some cases people would say that they knew why they did what they did, in others not. The temptation however, is to say that there must be some common aspect in the situation which encouraged people to behave in similar ways, or some common feature of the people themselves. This temptation arises from there being no evidence of particularly reasonable or rational thinking going on in the answers the subjects have given, especially when people say that they do not know why they helped. When common sense answers seem inadequate for understanding why we have made a decision, to help or not to help, then it would seem reasonable to look for a further explanation of what everyone thought that they understood.

When this temptation to look for a further explanation exists, the possibility of using a process as an

explanatory concept is attractive, since it should explain what it is about the interaction between people and their environment which makes them behave in the ways that they do. This is the first thing to note. The second thing to note is that this temptation only goes to reinforce the original skepticism about the adequacy of any understanding of behaviour. By suggesting that the process is a kind of entity which operates without us realizing it, as does the heart and the respiratory system, the process is placed beyond our knowledge, even though we can see its operational products. Nisbett and Wilson acknowledge the absurdity of asking us questions about our memorial and perceptual processes, and in doing so they imply that it is impossible for us to know about any other processes either. We cannot know what these processes are by introspection, so it is literally impossible for us to answer questions about them. Likewise, if Descartes' ideas are taken seriously, there is literally no way to distinguish waking from sleeping. By comparing the two it is of course being suggested that there is a mistake in social psychology's methodology of the same magnitude as that pointed out by Austin and Bouwsma in Descartes' method, and we will discuss this point in a moment.

Nisbett and Wilson's third conclusion is a direct result of the above view. If it is impossible to know about the process underlying decision making, or intervening in emergencies, then verbal reports about these activities cannot be about the processes. Therefore, they must just be accidentally true, or good guesses, if people report accurately at times, as Nisbett and Wilson have to admit

sometimes happens. Thus, even when verbal reports are correct, they are still not really correct, because they are not about the process, which provides the real explanation of actions and decisions.

The step of suggesting that a process is behind making decisions means that conflicting accounts can be cleared up, and a proper explanation given. This has the consequence though, that a skepticism as to everyday knowledge is re-emphasized, and made omnipresent.

#### CHAPTER FOURTEEN: FOOTNOTES

[1] "Telling More Than We Can Know: Verbal Reports on Mental Processes". Psychological Review, Vol 84, number 3, May 1977. The main author of this paper, Richard E. Nisbett, is a well respected practitioner in the area of social cognition who has worked with E.E. Jones amongst others, and whose book, Human Inference: Strategies and Shortcomings in Social Judgements (with L. Ross, Englewood Cliffs N.J.: Prentice-Hall, 1980) has been described as "a 'must' for any student of social cognition...With style and striking examples, the authors report where how and why we make so many erroneous inferences in our social judgements" (Leyens and Codol, p 110, in Hewstone et. al., Introduction to Social Psychology 1988).

[2] In Hewstone et al.'s book there is a section devoted to the "Construction of the Social World" (part II, pp 87-195). Leyens and Codol, in the social cognition chapter, suggest that the term "cognition" refers in a general way to all those activities through which a psychic system organizes information into knowledge (p 108), and in the book's glossary the term is defined as "the activity by which information is received, selected, transformed, and organized by human observers so as to construct representations of reality and to build knowledge" (p 446). Thus, cognitive processes are located within the person's psychic system, and deal with its interaction with the external world. Probably the best way to understand what is meant by a "process" in this context is to see it as a continued set of natural actions over which people have little control. Therefore it would seem that we have no control over these cognitive processes.

[3] Nisbett and Wilson report, for example, that Mandler, G., Mind and Emotion, N.Y.: Wiley, 1975, and Neisser, U.,

Cognitive Psychology, N.Y.: Appleton-Century-Crofts, 1967,  
hold this opinion.

[4] It is worth reminding ourselves that Jones argues that it was Lewin's methodological invention which led to social psychology being able to deal with inferred dispositions influencing responses to both external as well as implicit or imagined stimuli. See Chapter Two of this thesis.

## CHAPTER FIFTEEN.

Nisbett and Wilson's paper has been discussed at length to show the logical extreme which talk of psychological processes can lead to. Ordinary knowledge is rendered illusory, in that, even when correct, it is only accidentally so, and it is consigned to a realm of "appearances", or at best, shadowy reflections of the truth or reality of the motivations for behaviour, which are exposed only by social psychology's methodological triumph.

Ordinary explanations of behaviour are not infallible, as we have seen. We can be brought to recognize that an act which we thought we had performed for personal reasons, perhaps one governed by personal preference, was in fact better described as inflicted upon us. There can be factors at work which we are only dimly, if at all, aware of, which influence our choices and decisions. This much is not at issue. What is contentious is social psychology's claim that by talking about processes, of impression management, of intervention processes in emergency situations, of tension systems and quasi-needs, knowledge is always acquired about people which is deeper than, more fundamental than, and subversive of, our ordinary explanations.

It has been suggested that this subversive knowledge which social psychology promises to deliver is held to be called for in large part by a belief that ordinary knowledge and explanations of behaviour are inadequate, and generally, if not universally, inadequate at that. Austin and Bouwsma



have presented responses to a philosophical skepticism about knowledge which seem to have some force, and the consequences of responses such as these will now be explored with respect to the skepticism social psychology encourages about knowledge of motivations of social behaviour.

What Austin and Bouwsma show in their different ways is that doubt about beliefs and knowledge can go too far, in that it becomes theoretical, and thus a nonsense. Bouwsma shows that although it looks plausible to think that we could be deceived about being seated by the fire, in practise, claiming that such deception is a pervasive possibility is a nonsense, because it is unclear what such a claim means. It cannot mean the same as an "ordinary" claim to be confused about waking and sleeping, because in ordinary circumstances there are ways and means of distinguishing between wakefulness and sleep. No other meaning has been given to it by Descartes. Therefore, Bouwsma's suggestion is that the doubt which Descartes tries to raise is theoretical; of course, we could be deceived in this cruel fashion, but in reality the possibility remains well within the realm of fiction, and therefore the force of Descartes' argument diminishes. Its power to unsettle us is suspended.

By focusing on Descartes' argument concerning the dream, Bouwsma explodes his reason for doubting our knowledge of the world. With no reason for believing that we are systematically deceived, our doubts about what we know of the world are dissipated, and we can continue our lives, not completely sure that we can never be wrong about the world

and its objects, but sure that we are never completely deceived.

Austin too can be seen to be concentrating on the reasons for doubt. If you want me to share your doubt, then you must give me reasons for it. If I say that the bird is a goldfinch, and you say that it is not, unless you give me a reason for the doubt which you raise, it can be quite in order for me to ignore your doubt, and to maintain my own position. If you have such a reason then your doubt can be accepted as a real one, and I should take care to assuage it. If I cannot do this, then I too may come to share in your doubt. Alternatively, I may refuse to accept your reasons for doubt as adequate, or I may show you that your reasons for doubt do not stand up to examination. This is the way in which Bouwsma deals with Descartes. He rejects the possibility of being asleep and dreaming as being an adequate basis for an all-consuming doubt, as a good enough reason for a fundamental skepticism because such a deception could not possibly be maintained. It does not make sense to talk of such a deception.

The idea that doubt can be over-extended and expanded can also be drawn from Austin and Bouwsma's differing arguments. Bouwsma points out that Descartes' original position, that our senses sometimes fail us, can be seen as being uncontroversial. They undoubtedly do in some cases. The leap that Descartes then makes to saying that we can never trust our senses is however, taking his original position too

far. There is just no reason to suppose that we are always deceived. Austin too makes a similar point by suggesting that there are cases where a problem about knowledge can arise naturally, for example, "Is that a goldfinch?", and there are also cases where it does not ordinarily arise; for example, we would very rarely ask "Is that a real goldfinch?", and he identifies this second question, as we have seen, as an example of "the wile of the metaphysician". Where there is no reason to suggest the goldfinch is not real, in a sense, no real question has been asked, and where there is a reason, he suggests that there is usually a way of answering it.

Their observations illustrate how some arguments which purport to show that the best examples we have of knowledge are insufficient, are themselves defeasible. Their contributions are therefore important because it seems as though social psychology uses an argument of this sort itself. Social psychology, as shown in the examples previously discussed, notes that there are times when our knowledge about behaviour, and the motivations for behaviour, seems to fail. This failure can be seen regularly in everyday life in various ways. Additionally, experiments are performed in which it is found that people seem unaware of the influence other people have on them. Therefore, a reasonable doubt is produced about people's knowledge. If we ask, "Why didn't you help?" and receive an answer saying that it was not important to help because it was not an emergency, then at least a suspicion is aroused which suggests that there were influences at work the individual was unaware of. The social psychologist can explain the doubt they have about the

adequacy of knowledge here, in a quite ordinary and understandable way. They can give reasons for their doubt, and we can share in the problem their observation highlights in our understanding of behaviour. On the other hand, if someone said that they failed to help because there were other people present etc., then we could say that there was no reason to doubt the adequacy of their knowledge, because we would accept the accuracy of their explanation, knowing as we do that the presence of others does sometimes affect behaviour.

There seems little to object to in the above example. Some people will be more aware than others, but this is just a fact of life. People are different from each other, and even individuals can be observant on some occasions and not on others. However, social psychology's concern is with a larger "problem". Even in the "best case" of knowledge, where a person knows why they acted as they did (because other people were present) social psychology will still doubt the adequacy of their knowledge. This is because we cannot know the mechanism by which we come to our decision to help or not, and as people, that is ordinary, non-scientific, non-social psychologist people, we can never know. It is only through the scientific study of human behaviour that such knowledge can ever be arrived at. Therefore, even in the best cases of knowing our motivations, we can never know them properly [1].

Social psychology arrived at this position by noting,

as we have seen, that our ordinary explanations are often inconsistent, changeable, and sometimes plainly wrong. Having noted this fact, social psychology, like Descartes, then begins to explore whether we can trust any of our common sense explanations of behaviour, and decides that we cannot. The way in which this decision is arrived at is exactly analogous to Descartes' method. Reasonable doubt is noted, and then a device is used to make the best cases of knowledge appear fallible. For Descartes it was the dream, for social psychology it is the process.

There seem then to be two lines of argument open to us with which to examine social psychology. The first is to examine social psychology's claims, if it is accepted that doubt about ordinary explanations is truly pervasive, and the second is to examine them if it is not. It will be argued that in either case, social psychology's claims do not fare well at all.

We should note first that as they stand, the reasons social psychology gives for doubting ordinary explanations of behaviour do not, and cannot, produce the devastating doubt which it is claimed they arouse. These reasons, as we have seen, are that explanations are often inconsistent and contradictory, and also that there are social causes, or social forces, in the world, which affect our behaviour, but about which we know nothing.

Ordinary explanations of behaviour can certainly be contradictory, as can be seen from the readily available

generalizations in use in "common sense" discourse, provided by proverbs and the like. Although they are used to give advice such as "You can't teach an old dog new tricks", they are also explanatory devices, in this case, explaining a failure to acquire a new skill, habit, or social grace perhaps. What often seems contradictory about such received wisdom is that an equally well-known adage offers the opposite advice, "You're never too old to learn" for example.

However, there is only an apparent contradiction here, appearing when the wisdom is applied in the abstract or generally. It makes perfectly good sense to say of someone in particular that they are so stuck in their habits (and so happy being stuck perhaps) that there is no point in trying to teach them anything, while another individual really is never too old to learn. When applied as explanations of individual's behaviour, these adages are not contradictory in social psychology's sense, and therefore the apparently "obvious" explanations of conformity which Tajfel and Fraser find so disconcerting need not be contradictory either, but should be seen rather as complementary. Such explanations may go wrong on occasion, but this does not mean that it is right to doubt them without exception.

Similarly, the idea that social "forces" influence our behaviour does not entail a fundamental lapse of knowledge. We can sometimes quite easily recognize that a friend was influenced by his peers, by fashion, or by an

attractive advertising campaign. Our friend can recognize this about us, and we can also recognize this ourselves sometimes, meaning that whatever else it is, the doubt about "social forces" which social psychology tries to raise is not necessarily of epidemic proportions in the way it needs to be if social psychology's claims are to be rationally based.

There are of course reasonable doubts about explanations and social forces, but these reasons for doubt which social psychology presents to "arouse our suspicions" are not powerful enough to carry the day alone. It seems then that social psychology's main hope of convincing us that common sense explanations of behaviour are always inadequate lies with its use of the idea of process".

The argument that our ordinary explanations are all incorrect in the sense of not being about the processes operating inside us, or about the social forces influencing us from outside, rests on two linked assertions made by social psychology. The first is that such processes exist, and the second is that social psychology can discover what they are, and therefore know that common sense explanations do not match up to them.

If, for the sake of argument, we accept social psychology's reasons for doubting the validity of common sense explanations of behaviour, it is necessary to examine social psychology's claim to give a deeper, more sophisticated knowledge of behaviour and the motivations

underlying it. Social psychology's claim rests on its assertion that it can discover the processes underlying behaviour, as noted above. On closer examination however, this claim seems to be unsubstantiated. As we saw earlier in the discussion of Lewin's conceptual apparatus, the main thrust of his re-characterization of psychology was to explain actions using a quasi-technical terminology, which turned out to be merely a re-description of the behaviour in question. It was suggested that to say tasks are completed because of tension systems and not intentions is not to gain a deeper insight into them, but is only to re-describe and possibly distort them, since this re-description involves, amongst other things, a rather confused account of causality. It was also suggested that Lewinian methodology still influences social psychology today, and that talk of processes is just another way of describing what is noted by keen observers in everyday life. Therefore, to say that social psychology has provided a deeper understanding of human behaviour seems questionable, being based as it is upon a speculative description of behaviour, which is in turn based upon an analogy with the natural sciences, and the various assumptions that followed it, that, as has been pointed out, was never fully justified in the first place.

Therefore it seems safe to conclude that even if social psychology is correct, and we should doubt all our ordinary common sense explanations of behaviour, there is no need to accept the alternative explanations it offers since they are purely speculative and based on what may be a spurious analogy, which mistakes a description for reality.



Let us now turn to the second line of argument, the idea that in some situations the speculation that we do not know why we behave is well-founded, and that explanations involving mechanisms, or causes, are called for. The problem here is that although there may be some situations where such explanations are appropriate, social psychology over-extends the mechanistic model it uses, and ends up by saying nothing of importance at all.

This happens in the following way. Social psychology's explanations are intended to provide a better account of behaviour than that given by common sense explanations, one which shows that underlying a world of appearances, there is a secret reality consisting of genotypes or processes. If we accept that sometimes this could be true, and that there are influences acting on us which we do not ordinarily notice, we could substitute explanations in terms of processes for explanations in terms of intentions where appropriate. An "automatic" or "unthinking" action, such as turning the radio on when coming into the room, even when we do not intend to listen to it could perhaps be described appropriately as the result of a process, whereas, in other situations, turning the radio on would not be explained in terms of a process. We find that this distinction is in fact made in ordinary speech, as can be seen from accounts such as Hart's and Cioffi's.

However, social psychology cannot be content with

adding to our knowledge in this way, because it sees itself as a "science", and is therefore committed to general, mechanistic or causal accounts of behaviour. The explanations it gives are, by their nature, meant to be applied generally, and to replace our ordinary explanation of behaviour, which means that in situations where we do not see a problem, or feel a doubt about, our knowledge, social psychology sees one. This in turn means that, again, social psychology would say that our explanations are wrong, however we choose to explain behaviour. The only way in which we could be right when explaining behaviour would be if we said "We decided this way because of the decision making process" and so on. Saying this though would mean that the distinctions we normally make in our explanations of behaviour would have to be dropped. For example, we distinguish between free and compelled acts in common sense explanations, but on social psychology's account this distinction would have to be dropped because in reality, all acts are the result of processes and social forces.

What we ordinarily mean by an explanation is not what is meant by social psychology. To explain behaviour in terms of processes and social forces or influences is not a way of explanation that we can always accept. In cases where this type of explanation seems called for, it may be useful, as Cioffi and Robinson would point out, but to say that it is useful in all cases cannot be acceptable, because not only can we fail to share in the reasons for its justification, but also it subverts too many distinctions which are valuable to us, and seems to assert that we are like a machine in a

world of social forces, a machine moreover, which is deceived about its own "machineness", its own reality.

A similar argument is given by Cioffi [2]. He discusses the interpretation of works of literature, and finds that the notion of the author's intention is logically tied to the interpretation we give to the work. This would also seem true of the interpretation we give to a person's actions, especially when we are assessing them from a moral point of view. However, the temptation arises, when discussing intention and interpretation in criticism, to put forward a general thesis as a way of explaining the relationship between the two, rather than taking each piece of literature on its own merits.

What any general thesis about the relevance of intention to interpretation overlooks is the heterogeneity of contexts in which questions of interpretation arise. This heterogeneity makes it impossible to give a general answer to the question of what the relevance of intention to interpretation is. (Cioffi: p 89)

Similarly, the heterogeneity of contexts in which decisions for example, are made, even about emergencies, makes it impossible to give a general answer about the relationship between the two. There are times when it could be appropriate to use a causal explanation of such decisions, when it is obvious that a decision was more of a reflex or a reaction than a deliberate choice perhaps (but not always) however that was made. Equally, there are times when it is not appropriate. Social psychology fails to recognize this

distinction [3].

The view of people as being driven by mechanisms and forces is one which has hopefully been shown to be only an alternative, and not a deeper, more accurate, picture of humans than the one we ordinarily use. Thus, a spanner should hopefully have been dropped into the works of the social psychological, mechanical, causal, world, this world being "created" as a result of social psychologists feeling the need to take a scientific approach to explaining social behaviour. This perceived need tempted them into the use of a scientific methodology "borrowed" from physics and the advanced sciences. The "spanner" in social psychology's works is that social psychologists deal with people, making the methodology inappropriate and unresponsive to the reality of the "subjects", or rather people, being studied [4].

Although social psychology produces an alternative view of explaining behaviour in social situations, this alternative is not one which should find much employment. By undercutting ordinary distinctions with a kind of "essentialism", the explanations social psychology offers do not help us to understand people's behaviour as individuals, because the type of "process" answers which are used to explain behaviour are in fact so extended that they explain nothing. Explaining all the different kinds of decisions that can be made when faced with what might be an emergency, by seeing them as variations of a single fundamental process looks to be helpful, but is not. Social psychologists feel that they have made an important discovery when they talk of

finding an explanation for making decisions, because talk of social influences on decision making is so very different from what is ordinarily said about decisions, and can therefore seem like a discovery. However, this discovery seems trivial when it is remembered that people can see the differences in the behaviour of the people around them when other people are present. They too could say that this was the result of a social influence, but they would mean this in a way which is familiar and available to us all, not in a way which involves hidden currents of influence affecting psychic structures which we know nothing about. It looks therefore as though the sense of discovery the social psychologist feels is partly a result of the strange or extended use they make of language we all ordinarily use, such as force and influence.

Overall, in this discussion of the experimental tradition in social psychology, questions have been asked about the adequacy and appropriateness of social psychology for the task it sets itself, of producing a "deep" and sophisticated understanding of human social behaviour. In the main, the assumptions, both methodological and philosophical, underlying social psychology have been criticised. It was contended that the foundations of modern social psychological thought stem from the re-formulation of psychology which Kurt Lewin undertook in the 1930s and 1940s, which advocated a "Galilean", experimental approach at the expense of an "Aristotelian" understanding of science. A scientific psychology was thought important to eliminate the biases of

the "poet or biographer's" understanding of behaviour, and was championed by Lewin as appropriate to the study of humans. The assumptions underlying the conceptualizations Lewin advocated have been shown to be inadequate; for example, his assumptions of interdependence and topological representations are unfounded, and misrepresent the facts of human behaviour.

The "scientific" approach taken by Lewin poses great problems for him, as is shown in his discussions of causality. Although he attempted to provide a "systematic" causal account of behaviour in line with his scientific aspirations, his acknowledgement of certain facts of human life meant that he was unable to provide a coherent account of the motivations of behaviour.

It was contended that such facts about human beings, that they have hopes and wishes, volitions and attitudes, is a vital part of understanding people. Accounts were discussed which emphasized these facts, and which acknowledged the diversity of explanations that can properly be given of behaviour. Acknowledging these facts means acknowledging that behaviour as such has no single mode of explanation appropriate to it, as Robinson amongst others notes. In trying to impose a single, causal account on all aspects of human life and behaviour, social psychology needed to show how this account was superior to the diverse one we already have in common sense or ordinary use. However, as noted above, the methodological assumptions that social psychology made have been shown to be unfounded, but not only this, the

justification which social psychology gives for its particular interest in behaviour has been shown to be a nonsense, in that there is no reason to rationally suppose that we are fundamentally mistaken about the motivations for our behaviour at all times. Nor is there any reason to suppose that the existence of conflicting and contradictory ordinary explanations means that our understanding of behaviour is inadequate.

There are some areas of human behaviour in which social psychology's methodology could be useful, and add to our understanding, but the imposition of a single, causal, mechanistic account of behaviour on us means that many valuable distinctions we ordinarily make about our behaviour are disregarded. This in turn means that the explanations social psychology presents us with are in fact, in most cases, quite vacuous.

It has been shown therefore that social psychology does not give us a deeper, more sophisticated knowledge of ourselves, even though this is its declared aim. We could say in conclusion that we have noticed the spanner in social psychology's works, a spanner which ought to bring its causal explanation "machine" to a halt.

#### CHAPTER FIFTEEN: FOOTNOTES

[1] The other examples we have considered, eating crackers, finishing tasks, remembering restaurant bills, all share this common factor.

[2] Frank Cioffi, "Intention and Interpretation in Criticism". Proceedings of the Aristotelian Society, Vol

LXIV, 1963-64, pp 85-106.

[3] For a similar approach see Stevenson, Ethics and Language (1944), who psychologizes moral arguments, and Cavell, The Claim of Reason (1979), who argues that by doing this, Stevenson has subverted the basis of morality.

[4] For an interesting discussion of people in psychological experiments see for example, Adair, The Human Subject (1973).



## APPENDIX 1

### "LIFE SPACE" DIAGRAMS.

- A: Figure 16, p 136. Lewin, Field Theory in Social Science.
- B: Figure 17, p 138. Lewin, Field Theory in Social Science.
- C: Figure 16, p 97. Lewin, Principles of Topological Psychology.  
Figure 4, p 290. I.D. London, Psychological Review, 51, 1944.

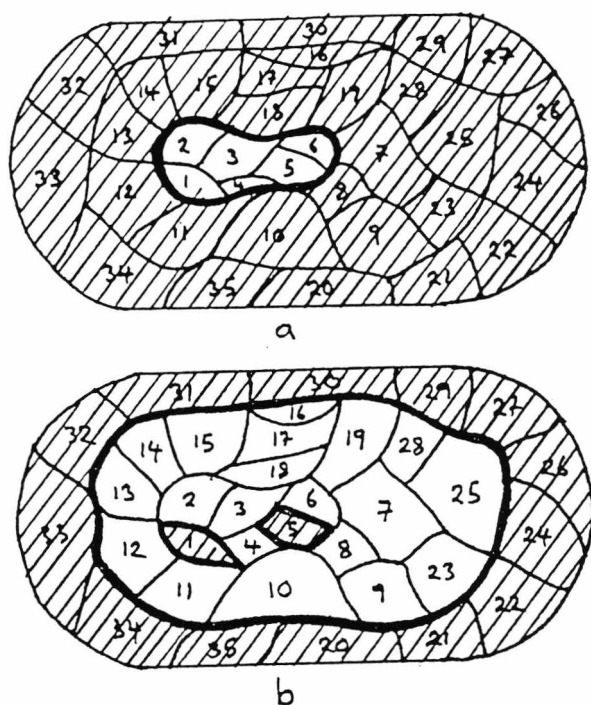


Figure 16, p 136. Lewin, Field Theory in Social Science.

Comparison of the space of free movement of adult and child.

The actual activity regions are represented. The accessible regions are blank; the inaccessible shaded. (a) The space of free movement of the child includes the regions 1-6, representing activities such as getting into the movies at children's rates, belonging to a boy's club, etc. The regions 7-35 are not accessible, representing activities such as driving a car, writing checks for purchases, political activities, performance of adult's occupations etc. (b) The adult space of free movement is considerably wider, although it too is bounded by regions of activities inaccessible to the adult, such as shooting his enemy or entering activities beyond his social or intellectual capacity (represented by regions including 29-35). Some of the regions accessible to the child are not accessible to the adult; for instance, getting into the movies at children's rates, or doing things socially taboo for an adult which are permitted to the child (represented by regions 1 and 5).

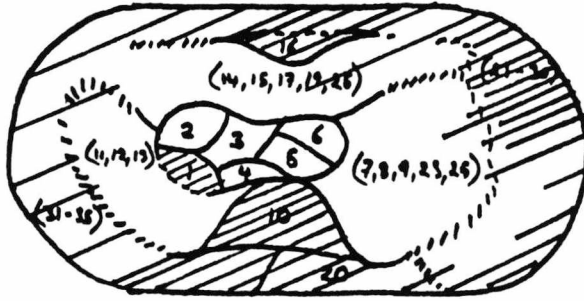


Figure 17, p 138. Lewin, Field Theory in Social Science.

The space of free movement of the adolescent as it appears to him.

The space of free movement is greatly increased, including many regions which previously have not been available to the child such as freedom to smoke, returning home late, driving a car (regions 7-9, 11-13, ...). Certain regions accessible to the adult are clearly not accessible to the adolescent, such as voting (represented by regions 10 and 16). Certain regions accessible to the child have already become inaccessible, such as getting into the movies at children's rates, or behaving on too childish a level (region 1). The boundaries of these newly acquired portions of the space of free movement are only vaguely determined and are in themselves generally less clearly and sharply differentiated than for an adult. In such cases the life space of the adolescent seems to be full of possibilities and at the same time of uncertainties.

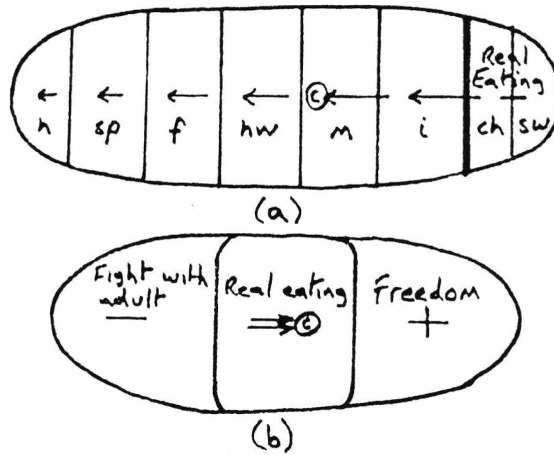


Figure 16, p 97. Lewin, Principles of Topological Psychology.

Situation of child facing disliked food. (a) before entering the region of real eating; (b) after entering the region of real eating. In this and the later figures the following symbols are used:

- force: the direction of the arrow designates the direction of the force; its length, the strength of the force; its point the point of application of the force.
- - - - - locomotion: the point of the arrow designates the place of termination of the locomotion, the other end of the dotted line designates the point of departure.
- + positive valence.
- negative valence.

Figure 4, p 290. I.D. London, Psychological Review, 51, 1944.

The Conceptual Representation of Setting Fire to a Piece of Cardboard Whose Kindling Point is Comparatively High.



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