The Role of Scientific Methods
in Social Work

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ABSTRACT

The thesis discusses the widely held view in social work that practice should be based on intuitive and empathic understanding and that standard scientific procedures are inapplicable. I argue that this anti-science attitude is misguided and that social workers can and should use scientific methods to test theories and develop more effective ways of helping.

There are practical and philosophical reasons for re-examining this dismissal of science. Social workers' statutory powers and duties have increased rapidly but there is also growing concern about their professional competence. Moreover developments in the philosophy of science challenge social workers' assumptions about science.

The first two chapters discuss the importance of overcoming the hostility to science, examining social workers' duties, training, and practice methods. The first objection to science examined is the claim that science studies only observable behaviour not mental phenomena. I argue that this is based on a false idea of science and suggest instead that there is great similarity in the way scientists and social workers theorise. The next chapter discusses the claim that the scientific search for causal explanations conflicts with a belief in free will; I argue that in fact there is no conflict.

The following chapter questions the reliability and scope of fieldworkers' intuitive and empathic judgements and sets out some reasons why they should be supplemented with scientific methods of testing. What counts as empirical evidence and how theories are appraised are the topics of the next two chapters. I argue that the traditional social work view of empiricism is unduly narrow and has hampered social work research. I also address the comparatively new objection to science in social work, namely the relativists' claim that science is not empirical and therefore should not be held up as a model to social workers.

The final chapter considers how scientific methods can be incorporated into practice.

CONTENTS

CHAPTER ONE

INTRODUCTION

When a young child appears with broken bones in a hospital casualty department and staff suspect non-accidental injury, they refer the matter to the social services department. Social workers are then involved in a series of difficult judgements and decisions. They must determine whether this is a case of child abuse, consider who might be responsible, speculate on the possible causes, assess the risk of further abuse, and decide what actions to take.

Social workers, in this example, need to explain why the abuse has occurred, predict the likely consequences of different courses of action, and decide how to intervene. These aims of explanation and prediction resemble those of natural scientists. Whether the similarity can or should extend to social workers using the same methods as natural scientists has long been disputed. As in the social sciences generally, those who argue for a scientific approach (naturalists) are opposed by humanists who claim that understanding human actions requires different methods from those used in studying the natural world. The latter position has received most support in social work, particularly among fieldworkers.

In this thesis, I shall challenge this majority view and argue that social workers can and should use scientific

methods in order to develop the most effective ways of helping the many people in our society who have to rely on them. The thesis examines the philosophical debates underlying the conflicting views on science and argues that the widespread hostility to the use of scientific methods that exists among social workers is based on a false picture of them and hence an inaccurate understanding of how they can be used in social work.

Philosophical debate, though abstract, is of practical importance. Social workers, seeing so much human suffering and bombarded with requests for help, might be inclined to dismiss philosophy as a luxury, an arcane subject which has little to do with their day-to-day work. But social workers inevitably take sides in the philosophical arguments and the position they take influences the help they offer to clients. In assessing the risk of child abuse, for example, they have to decide what evidence is relevant, how reliable it is, and how they can use it to make a prediction about the safety of a particular child. The social worker who favours the humanist approach may base his decision largely on an intuitive appraisal of the parents while his colleague who adopts a scientific approach will give more weight to empirical research evidence on risk factors. Their philosophical differences have practical consequences, affecting decisions about whether or not a child stays with his parents and, in a few cases where the predictions are disastrously wrong, whether he survives.

Moreover, although the debate about using scientific methods in social work is as old as the profession itself,

it has recently received increased attention because of issues in both social work and the philosophy of science.

It is typically in times of trouble that disciplines become interested in their philosophical foundations and to many it appears that social work today is in difficulties. The steady growth in the statutory duties and powers given to the profession illustrates the increasing expectations society has of it to alleviate personal and social problems. But at the same time, doubts are increasing about the competence of social workers. The public have been shocked by a number of cause celebres where children have been severely abused or murdered despite being under the supervision of social workers. Within the profession, unease has developed because, in many areas of practice, scientifically based evaluative research has failed to produce convincing evidence of effectiveness. In this unsettled period, attention turns to questions about what knowledge and skills social workers might have and how they can develop them.

The philosophy of science also provides a motive for re-examining social workers' assumptions about how they might understand and help their clients. There have been developments in thinking about the nature of scientific reasoning which substantially alter the premises of the traditional debate about using scientific methods in studying human conduct. These philosophical changes have led to some re-appraisal of the disputes in social work and two opposing views are emerging. On the one hand, some propose a new argument against science, namely the relativists' claim that criticisms of the former account of

science also invalidate the claim that science produces more reliable theories. Hence, it is said, science should no longer be held up as a model for social workers. On the other hand, it is argued that the more satisfactory empiricist account is not vulnerable to the criticisms humanists have traditionally levelled at science and so bridges the apparent gap between the two sides in the science debate. It is this view which I shall be presenting and defending throughout this thesis.

OUTLINE OF THESIS

My first aim is to show that these debates about knowledge are not just of interest to social workers but are of public significance. Chapter Two reports on the expanding role of social work in modern society and details the profession's increasing duties and powers laid down by parliament. In Chapter Three, I consider how social workers meet their responsibilities. A study of the nature of training programmes for social workers and the main approaches to fieldwork indicates that few use scientific methods in appraising theories or evaluating their work. Empirical research studies though cast serious doubt on the effectiveness of such a style of social work and provide grounds for arguing for a scientific approach.

Most social workers, however, would argue not that they should not use scientific methods but that they cannot employ them when trying to understand and help human beings. Their arguments to support this view are many and

varied, directed at different aspects of science. To review them, I have grouped them in relation to the stages of scientific reasoning. First, I examine claims that we cannot formulate scientific theories about human actions. Secondly, I look at the arguments against using standard scientific methods for testing theories, and thirdly, I review objections to using scientific criteria for evaluating the evidence.

Chapters Four and Five are concerned with claims that, in studying human behaviour, we cannot develop theories akin to those found in the natural sciences. Chapter Four examines the frequently made claim that scientific methods can only be applied to observable phenomena and not to the mental phenomena of thoughts, feelings, and hopes which are the focus of most social workers' interest. This assumption leads many social workers to think, mistakenly as I shall show, that the extreme form of behaviourism which studies only behaviour and not mental processes is the paradigm example of a scientific study of human conduct. Indeed, I shall argue that, far from being different processes, there are in fact strong similarities in the way that social workers and scientists theorise.

Another argument against theory development, examined in Chapter Five, is that the scientific aim of developing causal theories conflicts in some way with the assumption that people have free will.

On the issue of testing theories or conjectures, social workers, in current practice, seem to rely mainly on empathic and intuitive judgements. Although this thesis will not entirely dismiss their value, in Chapter Six, I

shall argue that they have serious limitations and need to be supplemented with more rigorous, scientific methods of testing.

Chapter Seven discusses social workers' objections to using such methods. Their criticisms are based crucially on the belief that only behaviour provides scientifically acceptable evidence and this, they say, is an inadequate test of theories about mental processes. But this is one of the issues on which philosophical thinking has significantly altered. I shall argue for a revised empiricist account of evidence and discuss the implications it has for research methodology in social work. This issue also forms one of the strands in the relativists' claim that science is no more reliable or valid than other forms of reasoning. I examine their argument that there is no empirical evidence which can provide an independent test of a theory.

Evidence does not decisively prove or refute a theory and Chapter Eight considers how theories are appraised in the light of the evidence. This has been a major philosophical issue in recent years but the argument which has received most attention (and support) in the social work literature has been the relativists' claim that decisions to accept or reject theory in science, or in social work, are not determined by empirical evidence but by social and psychological factors. I shall criticise this view and offer instead a Bayesian account of scientific reasoning which considers that scientists reason in accordance with the probability calculus.

This review of social workers' opposition to using scientific methods concludes that their objections are invalid, based on an inaccurate picture of science. Chapter Nine examines the practical implications of the account of science I have presented. I shall discuss how fieldworkers can incorporate scientific methods into their daily work and show the differences this would make in cases such as the abused child mentioned at the beginning of this chapter.

CHAPTER TWO

THE MODERN ROLE OF SOCIAL WORK

> A general haziness and indeterminateness surround the whole concept of social work and social services, so that even now in the late 70s, some fifty years after the introduction of systematic training for social workers, colleagues in related professions — doctors, nurses, teachers — often ask in exasperation: "but what do they do?" (Goldberg and Warburton, 1979, p.6).

This comment still rings true: other professionals and indeed the general public often have only a vague idea about social work. And yet, such ignorance is surprising in view of the power and responsibilities social workers have in modern British society. Over the past two centuries, the population's welfare has become more and more the concern of the State. Alongside the giants of education, health, income maintenance, and housing, social work has progressed from its birth in the slums of nineteenth century London, with a spurt in the post-war creation of the Welfare State, to its current standing as an important and costly public service.

Therefore, as a first step in arguing my case that social workers should use scientific methods, I need to dispel the "haziness" surrounding the concept of social work and outline the context in which the science debate occurs. This chapter provides a brief historical review of the development of social work and lists the main duties and powers of social workers today.

EARLY HISTORY

A central theme in the history of social welfare is the changing balance between individuals' duty to provide for themselves and the community's responsibility to help those in need. In the past hundred years there has been a significant shift towards state services to prevent and ameliorate social problems and, within these services, social work has been given an expanding role.

Before the modern state involvement in welfare, the well-being of individuals was left mainly to themselves and their families with some charitable help from the Church. Since poor law legislation in 1598 and 1601, the state had authorised a minimal, local service for the destitute, offering some help to people in their own homes as well as running workhouses as a final refuge for those in need. This system, funded by local rates, functioned for two hundred years, being implemented in different times and places with varying degrees of generosity to the poor.

At the beginning of the nineteenth century however the poor law system came in for growing criticism as its cost and the number of paupers rose. Byrne and Padfield (1990, p.6) cite three main causes for the call for change: the growth in population; urban development resulting from industrialisation; and war with France which had caused inflation and disrupted the economy.

A Royal Commission on the Poor Laws, set up in 1832, produced a report which formed the basis of the 1834 Poor Law Amendment Act. This Act, which has become notorious for

its harshness, had a strong deterrent element. It was "the great discovery of a commercial age," the historian R. H. Tawney (1948, p.268) reported, "that relief might be so administered as not merely to relieve, but also to deter."

The underlying thinking, reports Pinker was based on the assumption the the poor law system was dealing mainly with able-bodied people who were capable of looking after themselves: "able-bodied pauperism had been diagnosed as the social problem of the eighteen-thirties" (1971, p.61). Their poverty was believed to be their own fault, due to moral weakness and fecklessness. A system of state relief which in any way rewarded such failings and sapped self-reliance was to be condemned. The apparent cruelty of the new Poor Law sprang from concern for the long-term welfare of those in dire straits. It was believed, says Pinker that:

> a seemingly cruel procedure would result in the greatest kindness if able-bodied paupers could be driven back onto the labour market where, according to the laws of political economy, employment and the recovery of personal dignity were awaiting them (Pinker, 1971, p.59).

The new Poor Law ran on the principle of "lesser eligibility": the pauper's lot should be less favourable than those who were self-reliant so that only the truly desperate would turn to the state for help. No help should be given to people at home; admission to a workhouse, with its attendant stigma and reduction in physical comfort, should be obligatory (though in practice many areas did not enforce this rule). Moreover, paupers could be placed in "Correction Houses", and, from 1867 to 1918, men receiving Poor Law relief could in some circumstances lose the right

to vote.

There were of course some poor who were not able-
bodied, the sick and the elderly for example. Although in
principle they should not have received a deterrent
service, difficulties in defining who was not able-bodied
and the expense of running separate services meant that in
practice many suffered from the punitive measures aimed at
the workshy (Pinker, 1971, p.61).

While state help was reduced to a minimum, community
concern for the plight of the destitute grew, expressed in
the form of charity aid. Private philanthropy, since it
was not received by right, was thought to run less risk of
encouraging idleness and dependency. Some philanthropists
were motivated by distress at the conditions of the poor
while others were more worried about the risk of social
unrest or revolution posed by a large, deprived working
class.

Whatever the motivation, charitable works proliferated
in the nineteenth century. A survey in London in 1861 by
Sampson Low (1861) estimated that there were 640 charity
institutions, 279 founded between 1800 and 1850 and 144
between 1850 and 1860. He also reported that the total
income of charities was two and a half million pounds while
the total Poor Law expenditure was only one and a half
million (1861, p26). The charitable scene in London became
chaotic: the numerous organisations were unco-ordinated and
competitive. The most serious defect, to some, was that
they distributed aid indiscriminately, not properly
checking the recipient's circumstances or, in particular,

if he was already receiving help from another charity. The fear grew that, in spite of the good intentions of the philanthropists, charity was creating and encouraging dependency among the poor.

With the aim of co-ordinating charities and assessing need more accurately, the Society for Organising Charitable Relief and Repressing Mendicity was formed in 1870. Soon changing its name to the briefer "Charity Organisation Society", this body had as its central tenet, reports Pinker (1971, p.30) that "overgenerous provision of any kind of statutory or voluntary aid damaged rather than mended the moral fabric of society". To ensure that charity was dispensed in beneficial rather than harmful ways, the C.O.S. created the "caseworker", recognisably the predecessor of today's social worker (Woodroofe, 1962).

The caseworker's task was to interview applicants for alms and assess their needs, circumstances, and character. Most importantly she (it was usually a woman) had to judge whether assistance was likely to be beneficial, helping the person to return to self-sufficiency, or harmful, re-inforcing their laziness or moral depravity. The "deserving" poor could get support and money to assist their efforts but the "undeserving", which included "persons of drunken, immoral or idle habits" (C.O.S., 1890), were turned away, with Poor Law Relief their only source of aid.

Woodroofe , in her history of social work, sums up the thinking at the time:

charity, administered according to certain principles, could encourage independence, strengthen character, and help to preserve the family as the fundamental unit of society (Woodroofe, 1962, p.28).

Case examples illustrate the work of the C.O.S. Successful applicants were those like the single woman, a dressmaker, unemployed for a year because of illness, who was granted a loan of one pound towards the cost of a sewing machine, or a widow given training as an ironer so that she could do laundry work to support herself and her four young children. Case 1,123 was considered undeserving and refused help however. The Brixton Committee of the C.O.S. in 1834 censured this 36 year old man as an illustration of "how inclined many are to run to charity for help, instead of themselves 'putting by for a rainy day'" (C.O.S., 1884, p.149). With an income of 34s a week, rent of 4s 6d, and a wife and child to support, he could not afford to pay for convalescence for his child because he was in debt; this, it was judged, was the result of fecklessness rather than hardship

Both the C.O.S. and the new Poor Law worked on the assumption that the causes of poverty lay mainly within the pauper's control and avoiding destitution was essentially an individual's responsibility. But these beliefs came under increasing attack as the nineteenth century came to an end leading to calls for substantial changes in the state's role in relation to individuals' welfare.

Many factors contributed to the change in attitude to the poor and needy. For example, empirical research studies by Charles Booth (1889) in London and Seebohm Rowntree (1901) in York fuelled public concern by

revealing the extent of poverty. Booth reported that 30.7% of the population of London, that is 1,292,737 people, were living at or below the subsistence level. Rowntree distinguished between "primary poverty", caused by the utter insufficiency of the family income, and "secondary poverty", caused by misspending what could, given a rigidly disciplined pattern of expenditure, be an adequate income for subsistence. He found that 9.91% of York's population were in primary poverty and another 17.93% were in secondary poverty (source: Rose, 1971, p.246). Moreover, analysis of the factors associated with poverty suggested that it was not caused solely or even mainly by individual improvidence but by a lack of jobs, low wages, illness, and old age (Byrne and Padfield, 1990, p.38).

The economic depression of the 1880s which led to widespread industrial unemployment also helped to undermine the belief that the unemployed were mainly workshy (Byrne and Padfield, 1990, p.38).

Furthermore, the case for increased state intervention became more compelling when the Boer and the First World Wars revealed the poor health of the working class, many of whom were unfit for military service. Even if this was deemed their own fault, it was bad for the country and warranted state measures to improve matters.

Another, fundamental, shift in attitudes to the Poor Law arose from changes in political and economic theories. Pinker (1971, Chapter Two) reports that the Poor Law was based on laissez-faire doctrines of competition and self-help; it was "the necessary social complement to the free

play of competitive market forces" (p.84). As it was increasingly criticised however for being unable to solve the social problems of an industrialised society, rival theories were proposed: "between 1900 and 1914 collectivist doctrines of social welfare were gaining popularity and influence, and reached the dimensions of a counter-attack upon the principles of 1834" Pinker (1971, p.85).

For reasons such as these, the Victorian opposition to state intervention in social problems has been gradually replaced by an acceptance that it is necessary in a modern society.

The Poor Law was not completely repealed until 1948. In spite of a Royal Commission (1905-09) into its workings which produced a Majority and a Minority Report, both advocating change, it was not directly altered but "by-passed in the making of social policy" (Pinker, 1971, p.83). Its scope was gradually eroded by a succession of laws dealing with specific social needs, all of which diminished people's reliance on the Poor Law. Thus for example the 1905 Unemployed Workmen Act provided for public works employment and the 1911 National Insurance Act enabled workers to insure against the risk of unemployment or illness, so substantially reducing the potential clients of the Poor Law.

In this century there has been extensive legislation on the major social problems: on employment conditions, income maintenance, housing, health, and education. Within these laws, social workers have been given an

administrative and therapeutic role. The majority of social workers now operate through local authority social services departments and the probation service where their remit is prescribed by Acts of Parliament. A substantial minority (estimated by Byrne and Padfield, 1990, p.403, as about a third) are still employed in voluntary welfare agencies such as the N.S.P.C.C. and the C.O.S.' successor, the Family Welfare Association, but even these are often linked through funding to the state provision of welfare. Therefore the later history of social work is closely linked to changing legislation and is best presented in relation to the major client groups.

THE SICK AND DISABLED.

Nowadays, people who suffer from illness or disability, either physical or mental, form the largest group receiving assistance from the personal social services. Health problems figured frequently in referrals to the C.O.S. but a specifically medical role for social workers can be traced to the "almoners" or medical social workers, the first of whom, Mary Stewart, was appointed in 1895 at the Royal Free Hospital in London. Her task, originally, was to check how much patients could afford to pay for medical treatment but, in assessing their circumstances and needs, she soon became interested in extending her role to dealing with the practical and psychological problems caused by illness. Although her first task became obsolete with the introduction of the

National Health Service in 1948, almoning had by then established itself as a useful social service. In 1951 the Cope Report, reviewing the work of medical auxiliaries which included almoners, judged that "the work of the almoner should be regarded as one of the essential elements of a complete hospital service, and indeed of a complete health service" (1951, para. 115.)

The hospital-based service of the almoner has today been greatly extended by the policy of "community care" for people with chronic mental or physical disabilities. The aim of this policy is to avoid or minimise hospital admissions; people should be helped either to live in their own homes or in hostels/homes within the community. Local authorities are vested with the legal responsibility to provide practical assistance and support services, running day and residential units and employing many staff besides social workers in this major enterprise. The main piece of legislation on this subject until 1990 was the Chronically Sick and Disabled Persons Act 1970 which spelt out the duties of local authorities more precisely than earlier legislation.

Local authority duties are however set to increase substantially under the 1990 National Health Service and Community Care Act. Based on the Griffiths Report (1988) it ends the current division of responsibility between the National Health Service, Social Security, and local authorities because this arrangement has proved to be inefficient; the prime responsibility for providing community care will be given to local authorities. The Act

will be implemented in 1993.

Social workers have particular duties in relation to the mentally ill. About 90% of admissions for psychiatric care are now voluntary but the remaining 10% are admitted against their wishes. Compulsory orders are based on medical recommendations but the application for admission is made by "Approved Social Workers". Their predecessors in the nineteenth century, the Poor Law Receiving Officers, were involved with doctors and magistrates in the process of certifying people as lunatics and arranging for their detention in asylums. While the magistrate's role in compulsory admissions has since disappeared, the Receiving Officer's part has gradually become more important and more clearly identified as the work of a skilled social worker. The "duly authorised officer" of the first half of this century was replaced in 1959 by the "mental welfare officer" who in turn was superseded in 1983 by the Approved Social Worker. The Receiving Officers of the Poor Law had only an administrative function in organising admissions, implementing the decisions of doctors and magistrates. Social workers now are expected to have some independent expertise. The Mental Health Act 1983 specifies that, before making an application, the social worker has a duty to:

> interview the patient in a suitable manner and satisfy himself that detention in hospital is in all the circumstances of the case the most appropriate way of providing the care and medical treatment of which the patient stands in need.

The hospital-based psychiatric social work service has an unusual history. Other social work specialisms

developed in response to the recognition of a social problem. While the needs of the mentally ill had long been seen, psychiatric social work was started because of a new therapeutic method. In the 1920s, American social workers, like many others, had been most impressed by the work of Freud. Psychoanalytic theories seemed to many to provide both a compelling explanation of clients' problems and a therapy for alleviating them. British observers were impressed by the American developments and the Commonwealth Fund provided money to send some social workers to America to learn about the new theories. These social workers subsequently set up the first university-based social work training course at the London School of Economics in 1929. Only after completing the training did the first psychiatric social workers start work in mental hospitals and child guidance clinics. With the current community care policy, their functions have subsequently widened considerably and they now play a significant part in the after-care of patients.

CHILDREN

Children's welfare is the area in which social work gets the most public attention (and criticism). The well-being of children has long been a major concern of social policy makers, the first Children's Act being passed in 1908, the most recent in 1989. The 1948 Children Act was a landmark, setting up local authority Children's Departments, staffed by Child Care Officers, taking over the duties previously divided among several different local

authority departments. These departments have since been incorporated into the general social services departments. They have a duty to monitor children's care, try to prevent family breakdown, and to provide alternative care if necessary. The first concern of social workers according to current legislation is to prevent the break-up of families. The primary duty of local authorities, set out in Section 1 of the Child Care Act of 1980 is to:

> make available advice, guidance and assistance as may promote the welfare of children by diminishing the need to receive children into or keep them in care or to bring them before the Juvenile Court.

If efforts to keep the children safely at home fail, they may be taken into care on either a voluntary or compulsory basis. The local authority has a duty to receive into care any child who is orphaned, without a guardian, abandoned, or whose parents are prevented from caring for him properly and who request his reception into care (section 2 of the Child Care Act 1980).

Local Authorities also have powers to apply to a Magistrate's Court for a "care order", which transfers parental rights to them, when it is considered that a child is "in need of care or control" because of ill-treatment, neglect, delinquency, moral danger, inefficient education, or being beyond the control of his parents (section 1(2) of the Children and Young Person's Act, 1969).

Another major duty is to supervise the welfare of children whom the Juvenile Court has placed on a "Supervision Order". These orders may be made for many reasons, the main ones being because of delinquency or

divorce proceedings.

OFFENDERS

The roots of the Probation Service are found in the nineteenth century practice of some magistrates who, fearing the corrupting effects of prison on young men, preferred to release first-time offenders on the condition that they "kept the peace"; sometimes they attached a condition of supervision by a parent or guardian. Organised supervision began in 1876 when the Church of England Temperance Society appointed a "missionary" to some London police courts. The courts would release the convicted offender on the condition that he would see the missionary who would "advise, assist and befriend" him and help him lead an honest life. The success of this innovation led to the Probation and Offenders Act 1907 which enabled all magistrates' courts to appoint probation officers.

The major duties of today's Probation service, set out in the Probation Rules (H.M.S.O. 1984), are still to supervise offenders and "to advise, assist and befriend" them. Their clients may be placed on a Probation or Supervision Order instead of being given a custodial sentence or they may be referred for supervision as a condition of a release on parole from prison. Some clients are voluntary. For instance, those on bail awaiting trial and people released from prison may ask the probation service for help.

Probation Officers' expertise in social and

interpersonal matters is now more widely used in the legal system. It is generally recognised that the circumstances of the offender are relevant in deciding what sentence will have the most reforming or deterrent effect. The courts may ask the Probation Officer to provide a Social Enquiry Report to help them reach a decision. The Probation Service also plays a major role in divorce and custody proceedings, and provides marriage counselling and conciliation services.

RE-ORGANISATION

The re-organisation of the personal social services in 1968 in Scotland and in 1970 in England and Wales is a major landmark in the expansion of social work, creating large local authority departments with political influence and uniting the various specialisms into a single social work profession.

Social work responsibilities and services developed in a piecemeal way, responding to the needs of particular client groups. This was seen increasingly to lead to problems of duplication, poor co-ordination, and gaps in services. At the same time, there was growing recognition of the similarity in the work of the various helping agencies, irrespective of their client group. Pressure grew for a re-organisation and integration of social services.

In Scotland, the Kilbrandon Report (1964) reviewed the personal social services and recommended the setting up of Social Work Departments which would have responsibility for

all social work, including probation. The Social Work (Scotland) Act of 1968 put these proposals into practice. The Seebohm committee, facing the same task in England and Wales, reported in 1965 and its recommendations were enacted in the Local Authority Social Services Act of 1970. They reached similar conclusions to the Scottish enquiry, recommending the establishment of Social Services Departments which would bring together: the child care service, the welfare services, education welfare and child guidance services, the home help service, mental health social work services, adult training centres, day nurseries, and the supervision of childminders. Unlike the Scottish system, probation remained as a separate service. Medical and psychiatric social work stayed under hospital management until the re-organisation of local government in 1974 when they too came under the management of the local authority.

The Seebohm reforms integrated not only the administration of the different social work services but also the fieldwork of the different specialist social workers. The Seebohm Committee, in line with the developing consensus in the profession, judged that:

> the common elements in the practice of social workers
> in different settings are much more important than the
> elements which distinguish them (1968,
> para.520).

Consequently it thought that the new departments should be staffed not by specialists but by "generic" social workers able to deal with a wide range of problems:

a family or individual in need of social care should, as far as possible, be served by a single worker... with a comprehensive approach to the social problems of his clients" (1968, para. 516).

The new social services departments have expanded rapidly to meet their ever-growing statutory responsibilities. Between 1970 and 1980, the percentage of public expenditure allocated to the personal social services rose from 0.9% to 1.9% (Goldberg and Hatch 1982). During this time, the number of social workers employed by local authority social services departments more than doubled. By 1985, approximately 19,800 social workers were employed in direct work with clients while another 9,500 worked in senior administration and management positions (H.M.S.O. 1987). Although detailed implementation is still only at the planning stage, the increased responsibility for community care given to local authorities by the 1990 National Health Service and Community Care Act means that the number of social workers needed by the statutory services will not decrease in the foreseeable future.

CONCLUSION

The aim of this chapter has been to clarify the modern role of social work. This historical review shows it has expanded dramatically in the past hundred years. Having begun as a mainly voluntary enterprise, it has increasingly become a statutory service with an extensive mandate to help people in difficulty.

Goldberg and Warburton give a vivid illustration of the range of potential clients of a modern social services

department:

> the terrified mother who has hit her nine-month-old
> baby; the boy who has run away from a tight,
> overprotective home; the adolescent drug user who
> lands on his relatives on returning from Morocco; the
> marital complexities in a family where one partner is
> mentally ill; the unpaid bills and eviction order of a
> problem family; the guilt and doubt of parents who are
> at the end of their tether coping with a severely
> mentally handicapped child at home; the wife who
> refuses to have her husband home again after a severe
> stroke; the lonely, old, frail lady whose only son has
> married and who has nothing left to live for in her
> large empty house (1979, p.7).

Moreover, the increasing statutory role has not just
increased social workers' duties but also their power. They
play a significant role in many cases where people's civil
liberties are removed. They may be involved in arranging a
compulsory admission to a psychiatric hospital, or in
removing a child from the care of his parents. Many people
are placed by law in the care of or under the supervision
of social workers. Besides these cases where social
workers have overt authority, their power is in fact more
extensive. Even for most apparently voluntary clients
there is no choice about receiving social work help since
there is no alternative source of assistance. Few parents
of children with learning difficulties, for example, are
rich enough to buy all the social services they need in the
private sector.

Another development shown in the legislation is an
growing belief that social workers have particular skills
in helping. While society may be vague about what social
workers do, it has shown itself ready to give them
increasing responsibility and power. As Richard Titmuss,

a leading authority on social policy, commented during a
lecture in 1965:

> it is an interesting and often overlooked fact that
> during the past twenty years whenever the British
> people have investigated a social problem, there has
> always followed a call for more trained social
> workers" (reported in Randall, 1981, p.222).

This willingness to increase the role of social
workers suggests some trust in their ability to carry out
their duties, possibly even some over-optimism as Butyrm
suggests:

> the granting of such power and responsibility by
> society to social work implies a degree of confidence
> in its knowledge and capabilities which even many
> social workers would consider excessive (1976, p.x).

The growing importance of social work in modern
society strengthens the need to examine its knowledge base.
Philosophical questions about how its knowledge can be
evaluated and its capabilities measured are of urgent
practical and public significance.

CHAPTER THREE

SOCIAL WORK EXPERTISE

INTRODUCTION

Ninety percent of fieldworkers nowadays are professionally qualified. The Central Council for Education and Training in Social Work (CCETSW), the organisation responsible for approving training courses, asserts in a statement on training that social workers "share a common core of knowledge, skills, and values" (1989, p.3). These two facts suggest, misleadingly as I shall argue, a high degree of uniformity within social work in understanding clients' problems and a similar level of unanimity about how to approach them. In fact there are many conflicting views, the one of most interest in this thesis being on the possible role of scientific methods in developing knowledge and expertise in social work.

Social work is seen by some as potentially scientific. The social sciences are considered a source of explanatory theories and therapeutic ideas; social work methods should be evaluated according to the standard procedures of science, and the profession should aim to develop a public and tested set of theories on which to base fieldwork. This point of view is championed mainly by academics and is apparent in the guidelines on social work education produced by CCETSW.

The majority of social workers, particularly of

fieldworkers, on the other hand, are opposed to this view. They consider understanding and helping people depends mainly on the empathic skills and intuitive understanding of the individual worker. Such skills, it is claimed, are essentially personal, refined through experience rather than by formal tuition. The social sciences are seen as a useful source of ideas but such ideas or theories are to be judged by the individual in the light of experience and intuition rather than by scientific methods.

This chapter traces the impact of these rival views on social work education and fieldwork. I begin by examining the history and content of training courses before appraising the dominant, non-scientific, methods of practice.

HISTORY OF SOCIAL WORK EDUCATION

It is not self-evident that social workers need formal training. We all have to solve problems in daily living and most people are called upon to help friends and relatives at times of crisis. Social work is closely linked to these ordinary activities which we manage without training and, in its early days, many considered that social work also needed no special expertise. Social workers, it was thought, needed to be kind, experienced people with common sense and some practical knowledge: nothing else.

This view persisted in some branches of social work until fairly recently and perhaps is still held by some. Before the re-organisation of social services in Britain in

1972, for example, few workers in the local authority welfare and mental health departments held formal qualifications nor did they seem to regret this state of affairs. Rodgers and Dixon (1960), in a study of a county borough in the mid-fifties, found that only 5 of the 72 social workers were professionally trained and they confirmed that this was not seen by the staff as a problem. They reported that workers thought it was more important to be "good with clients", "not allowing people to get away with it", to have a wide practical experience, and to know the statutory regulations. Casework was common sense "requiring no special skill or methods so that any worker feels himself equal to it" (1960, p.158.)

These characteristics are still highly valued in social work today and this is reflected in the fact that training courses prefer older students with more experience of life and its difficulties, and that most social workers start their careers by working as unqualified staff before undertaking any special training. Indeed common sense and practical knowledge are probably sufficient to deal with many of the referrals to a modern social services department. Goldberg (1979, p.125), for example, in her study of such a department, reported that the large majority of new referrals just wanted practical assistance, such as a home help, advice on statutory rights and on available services, or someone to help them deal with another statutory department, particularly the D.H.S.S. or the Housing Department.

Ordinary helping skills are useful in social work but they have come to be seen as insufficient. Formal training

schemes developed from the belief that social workers could offer more than kindness and common sense advice. By turning to the scientific study of human behaviour, it was hoped that social workers could develop a greater understanding and more effective ways of solving people's problems. As the following historical review shows however there has not been consensus on how social workers can be scientific.

Charities offered the first training for social workers, the main pioneer being the Charity Organisation Society (C.O.S.). Their caseworkers received formal tuition and supervised practice — a combination of teaching methods still used in modern courses. The formal teaching covered financial and practical information and record-keeping. In 1917, Mary Richmond, a leading member of the American C.O.S. which taught a similar course, formalised their methods in Social Diagnosis, the first textbook for social workers.

The book advocates an explicitly scientific approach but, for Richmond, this means that social workers should use scientific methods of investigation rather than draw on social science theories. She stresses the importance of a thorough investigation of claimants' social circumstances before "diagnosing" their problems and deciding on "treatments". She discusses the reliability of different types of evidence — direct observation, testimony, and circumstantial — and the validity of different forms of inference. The book also encourages caseworkers to keep clear and consistent records of work so that they can monitor their efforts and formulate general principles. In

1921, Mary Richmond was awarded an honorary master's degree by Smith College for "establishing the scientific basis of a new profession".

Other pioneers of social work education turned to the emerging social sciences for a greater understanding of the social and psychological causes of personal problems, initially concentrating on social and economic theories, rather than psychology. In 1904, Urwick, the first principal of the School of Sociology in London (later incorporated into the London School of Economics), set out the aims of their social work course, stating that social work should be based on the "science of sociology" which:

> finds its place waiting for it as the director of the new social interests and as the interpreter of the complex social life which now for the first time has become an almost universal object of thought.

Simply understanding socio-economic causes of problems however offered little direct therapeutic guidance to those helping individuals and families. Social theories suggested ways of alleviating problems by intervening at the level of social policy. Though some social workers were ready to put their efforts into social reform, for the majority who continued to work with individuals the practical value of these theories was disputed. Richmond, although also advocating a scientific approach, criticised the social science teaching and defended her individual approach in the C.O.S.:

> if I could choose a friend for a family fallen into misfortune and asking for relief, I would rather choose for them one who had this practical resourcefulness than one who had a perfect equipment of advanced social theories.... The former would find the most natural and effective way out... the other would say that the whole social order was wrong and must pay a ransom for its wrongness by generous material help for its victims (Richmond, 1899, p.137).

The "treatment" prescribed in her textbook consists mainly of arranging financial help and encouraging the clients through a friendly relationship: "the tonic influence which an understanding spirit always exerts" Richmond (1917, p.200). Yellolly (1980, p.36) describes the social work of the nineteenth and early twentieth century as characterised by a moral and evangelistic outlook. Caseworkers tried to help the individual or family "through moral influence exercised within an authentic and personal relationship".

Despite Richmond's criticisms, a grounding in the social sciences came to be considered an essential element in social work training but, because of its limited utility in direct work with clients, it gradually came to be seen as a preparatory stage, so that by the 1960s students also took further training of a more vocational and practical kind. In 1959, a Government working party on social work education accepted the view that:

> they (the social science courses) do not adequately equip the student to take a responsible social work post... To be recognised as fully trained, the present day social worker should both have successfully completed a general course in social studies and also have taken a social work training, usually lasting approximately a year, leading to a professional qualification (Younghusband Report, 1959, para. 816).

Psychological rather than socio-economic theories proved to have a more direct influence on work with clients. From around 1910, in what has been termed "the psychiatric deluge" (Woodroofe, 1962), social workers showed a growing interest in psychiatry and psychology. Although initially the interest was not specifically Freudian, psychoanalytic theories soon became dominant,

particularly in America. Like so many others, social workers were greatly impressed not only by the deep understanding these theories seemed to offer of human problems but also by their therapeutic promise. Unlike the social theories, Freud's focus on the individual blended easily with the social work tradition of individual and family work and his theories apparently offered guidance on how to help those in psychological trouble. They were hailed by many as providing a scientifically-based therapeutic method for social work.

Freudian theories became a major component of social work education and had a substantial impact on fieldwork in America where training courses had proliferated. In Britain their influence on fieldwork, and indeed the impact of any type of training, was much less because the British were slower to accept the view that social workers could improve their competence through formal tuition.

Social work began as a set of specialist services for particular client groups and, initially, training courses were also specialised. The value of training was accepted at very different rates in the various specialties. Psychiatric social workers are at one extreme, with a university-based training always having been required since their creation in 1929. Almoners, now called medical social workers, who had existed since 1895, soon followed their example, making qualification a prerequisite for using the title "almoner". Few welfare workers and mental welfare officers however were qualified. Child Care Officers, a category only created by the Children's Act of 1948, and Probation Officers fall between these two

extremes in terms of training, with increasing numbers being professionally qualified.

The 1960s was the time of the fastest expansion in social work education. The following table, taken from Younghusband (1959, p.296), shows the growth in the different specialties.

	1960 % TRAINED	1970 % TRAINED
PSYCHIATRIC SOCIAL WORKERS:	100%	100%
MEDICAL SOCIAL WORKERS:	100%	100%
CHILD CARE OFFICERS:	26%	47%
WELFARE WORKERS:	5%	(approx.)27%
PROBATION OFFICERS:	65%	74%

Since 1970, the figure has grown steadily so that now 90% of all fieldworkers are professionally qualified.

The 1960s however saw not only a proliferation of courses but also of theories taught on them. The dominance of psychoanalytic ideas was reduced by a resurging interest in social theories and several new methods of intervention.

The analytic approach had tended to reduce the emphasis placed on social factors as possible sources of clients' problems. Younghusband reports:

> in the casework writing at the time it was not contested that housing and other environmental factors were important but they were held to be secondary, i.e. that the client would be able to surmount them but for his inner conflicts (1959, p.109).

While Freudian theories were not rejected entirely, there was a growing interest in studying the client's

social context.   Goldberg, a leading academic social

worker, summed up the view:

> having by now securely incorporated into the theory
> and practice of social casework the tenets of dynamic
> psychology, we might usefully re-discover the social
> environment in which our clients move, not as a static
> framework but as a dynamic process continually
> interacting with inner personal forces (1961, p.104).

The practical consequences of this greater interest in

the social circumstances was that more emphasis was put on

providing   practical   help   or   financial   assistance.

Supporting this development,   Younghusband   criticises

analytic casework because it had: "a tendency to denigrate

practical services and to regard 'material needs' as

distracting attention from the 'real problem'" (1959,

p.108).   Improving social circumstances as well as

personal growth were seen as legitimate goals of social

work   (untrained social workers had never lost   this

practical element).

Several new methods of helping were introduced during

the 1960s. Behaviourism, the great rival of psychoanalysis

in psychology, was espoused by Jehu (1967) who argued in

favour of behavioural modification therapies in social

work, and introduced them into the training course at

Leicester University.   Despite a mainly hostile reception,

teaching in behavioural techniques have subsequently been

added to all courses.

Analytic casework was generally considered to need

long-term   involvement but the   brief,   focussed   work

characteristic of behavioural methods was also a feature of

other new approaches, for example task-centred casework and

crisis intervention.

With the expanding input from the social sciences and the number of helping techniques it is not surprising that by the 1970s many social workers were feeling bewildered by the array of competing ideas they were offered. Social Systems Theory looked a possible solution. This offered a comprehensive conceptual framework for the wide range of subjects on a training course — the individual, the family, group dynamics, economic and political systems, and social policies. Pincus and Minahan (1973) and Goldstein (1973) all produced practice theories using systems theory to "integrate" social work theories and methods.

This brief survey of the content of social work training, though not comprehensive, covers the major theoretical changes. The other significant development in social work education is the move from specialist to general or "generic" training. As training courses developed, it was realised that, though each branch of social work had some areas of specialised knowledge, much of the teaching was common to all. Medical social workers for example needed a particularly detailed understanding of illness and Probation Officers required an extensive legal knowledge but, in other respects, their needs were similar. The case for a generic training was first put in 1947 in a Carnegie report (Younghusband, 1947). It was endorsed by the Joint University Council for Social and Public Administration (JUC) in 1951 which argued that the generic course was:

> based on the assumption that the basic skill of the social worker is the same in all situations though it has of course to be adapted to the setting in which it is exercised. The social worker has in fact to be trained first and foremost to understand human beings who are the same people wherever they are.

The first generic course in social work was started at the London School of Economics in 1954 and "genericism" became the norm in the 1960s, though two specialist courses survived into the early 1970s.

CURRENT TEACHING

All training courses for social workers have to be approved by the Central Council for Education and Training in Social Work (CCETSW), a body which was set up by the 1970 Local Authority Social Services Act and incorporated a number of earlier training and advisory services.

At present, there are two qualifications in social work, the Certificate of Qualification in Social Work (C.Q.S.W.), mainly taken by fieldworkers, and the Certificate in Social Service (C.S.S.) usually taken by workers in residential, day-care, and domiciliary services. Because of dissatisfaction with this split, they will be replaced by a single qualification, a Diploma in Social Work, in 1994.

The following discussion deals only with the C.Q.S.W., the qualification most fieldworkers take at the moment. Most courses run for two years though, after a major review, CCETSW (1987) concluded that this was not long enough. It wanted to see courses extended to three years because of the amount of material to be covered, but the government refused to fund this change.

The courses provide both academic teaching and practical work experience. Students spend about half their time in a fieldwork agency working with clients under the

close supervision of a more experienced social worker. The rest of their time is spent in college in academic study.

As my historical review showed, social workers have continually found new areas of study relevant, and rarely discarded any, so that now in considering what social workers need to know, as one commentator said: "the total individual in relation to the whole of his environment may be relevant" (Lee in Bailey and Lee, 1982, p.18). Consequently CCETSW's national guidelines on course content prescribe an immense area, requiring:

> that students are able to demonstrate to the satisfaction of examiners, knowledge and understanding within each of the following areas of study:
>
> (a) social work theories including their relevance to practice in work with individuals, groups, and communities and in field, residential, and day services;
>
> (b) the formulation, processes, functions and purposes of social policies and their manifestations in social services, both public and voluntary; current issues and problems of social policy, social work as a component of social service provision;
>
> (c) the institutions of central and local government, including their political and administrative dimensions; methods of financing local authority services, relationships between local and central government;
>
> (d) the social functions of law and the structure and processes of the courts; the legal context of social work, and the role of social workers in the administration of specific laws;
>
> (e) processes of human development, socialisation and functioning, both normal and deviant, throughout the age cycle, within a multi-cultural society; the nature of moral behaviour; social institutions, systems and organisations and their effects on human functioning and expectations; processes and theories of social change (CCETSW, 1981, para. 5.8).

These guidelines cover vast subject areas. Indeed, the topics are so broad that it is difficult to think of

any part of social science which definitely falls outside them. Selection is essential, but how do teachers decide what to include and what to leave out?

Modern generic training courses have the particular problem of balancing general theories against specialist knowledge relating to particular client groups. The danger is that they will produce social workers who know a little of everything but lack necessary specialist knowledge. Qualified fieldworkers have in fact been severely criticised on this score. For example, in mental health work, it was reported that many social workers involved in compulsory admissions of patients to psychiatric hospitals did not have adequate knowledge and skill. To remedy this, the Mental Health Act of 1983 prescribed extra training for social workers who operate under the Act. Similar criticisms have been made concerning child care. The Beckford Report (1985), enquiring into the death of an abused child under the supervision of the Social Services, censured the fieldworker and her senior social worker for having little understanding of the legislation under which they were acting and practically no knowledge of the relevant literature on child abuse.

Deciding on course content is further complicated by the range of complementary theories of human behaviour. Our actions are generally thought to have a complex causation. Theories citing social pressures, genetic inheritance, economic factors, or psychological causes can all be consistent, each providing a partial explanation of behaviour. Each theory would suggest different ways a social worker could intervene. In this case, social work

teachers can quite consistently accept several theories but, through pressure of time, selection is essential, and in discarding some dimensions, there is a danger of giving a skewed picture of human behaviour. The courses run in the 1960s are now criticised for concentrating on psychological explanations, minimising the importance of social factors. Today's courses pay little attention to genetic or other physical explanations of behaviour, possibly giving students an unbalanced view of the power of social and psychological factors.

The most vital questions though in deciding what theories to teach social workers would seem to be about the truth or probability of the theories and the effectiveness of the various therapeutic methods. The social sciences however present course planners with difficult decisions. The social work teacher is in a very different position from the engineer or technician who looks to the natural sciences for his theoretical knowledge and finds a general consensus about which theories to accept. The social sciences offer a range of conflicting theories, many of which are highly speculative, untested, and controversial.

How are social workers to appraise these theories?

On this issue, the practical consequences of differing attitudes to science become apparent. To some, scientific methods provide the most reliable way of evaluating the many rival ideas and identifying the most productive ways of helping clients. Empirical evidence, derived from standard kinds of scientific tests, is a major factor in the natural scientist's evaluation of a theory. But such

evidence is rare in social work and there is no strong research tradition trying to make good this deficiency. In contrast to those who advocate a scientific appraisal of theories, the majority believe the individual social worker is the best judge of which theories or methods are most appropriate to him or her.

The way theories are usually presented to students shows the prevalence of the latter point of view. Courses generally teach a range of conflicting and complementary theories but do not come down in favour of any particular one (Sheldon, 1978). Students are left to choose the theories that most appeal to them or perhaps to reject them all.

Lack of empirical evidence is one factor which makes it difficult for students themselves to make scientific evaluations, but they are also hampered by a lack of relevant teaching. Cassons made an extensive survey of social work course curricula and criticised the little help given to students about how to judge theories. He complains :

> unless the principles by which knowledge is constructed and tested are made explicit, the student is left with no clear way of comprehending why explanations should conflict and can develop no criteria upon which he can make choices between explanations. A second implication is that the student may attribute a status to knowledge transmitted to him that is not justified by the way it was constructed and tested (Cassons, 1982, p.126).

Students are given a wide choice in their theoretical teaching. Social work methods are usually divided into three main categories: casework (with individuals and families), groupwork, and community work. Each one of these incorporates a number of different theoretical

approaches. The parent who abuses his child may be said to do so because of psychological problems, interpersonal difficulties, material problems of poverty or bad housing, inadequate social support from his local community, or because of the prevailing, oppressive, capitalist system. For each of these causal factors there are several rival theories. For example, in psychology, students are faced first with the major rivalry between psychoanalytic and behavioural approaches and then with the internal disputes of Freud and his many successors.

These various theories suggest different ways for social workers to intervene: to provide counselling, family or marital therapy, to give material aid, to develop social support networks, or to raise clients' political consciousness. This list is by no means comprehensive.

Sheldon describes this type of curriculum as the "supermarket" style of teaching:

> the incoming student takes his "basket" to each of the various subject displays, selects the goods which take his fancy, and obtains his C.Q.S.W. at the check-out: his choice is virtually unconstrained (1978, p.9).

Sheldon goes on to illustrate the choice available on a typical course with a lighthearted but only slightly caricatured account of the student's experience:

> theories are often taught alongside each other, the ultimate choice being left to the student. Here, the failure of an individual to develop an adequately functioning conscience may be discussed on Mondays in terms of his early feeding experiences (Klein), on Tuesdays, as resulting from a failure to resolve a competitive relationship with his father at four (Freud); on Wednesdays a disturbance in the discrete stages of intellectual development which exert influence over the next decade may be indicted (Piaget); and on Thursdays the lifelong process of operant conditioning is emphasized (Skinner). Friday is for fieldwork, of course (Sheldon, 1978, p.10).

Writers commenting on social work training in the U.S.A. have also spoken critically of the way that students are left to make a personal choice from an array of conflicting ideas. For instance, Goldstein, like Sheldon, illustrates the complex range of ideas confronting students, asking us to:

> consider the plight of the typical recent graduate of a social work program whose head is cluttered with this diversity of constructs and theories. Where does one begin in the attempt merely to assess the client? Should the focus be on the client's ego strengths, social role, psychosocial patterns, personality traits, or status in his or her system? Or, should the focus be on the family's interactions, communication patterns, selected external re-inforcements, or what? (Goldstein, 1986, p.354).

Loewenberg is another American social worker who expresses sympathy for the student: "there are those who wonder whether this unrestrained freedom to choose from a large number of different theories does not put too large a burden on the individual social worker" (1984, p.310).

Clients also deserve our sympathy. The help they receive will be dependent on the choices made by their particular fieldworker.

Despite CCETSW's oft-repeated claim that social workers have "a common core of knowledge, skills, and values" (1989, p.3), training provides not a common body of knowledge but a range of conflicting, and for the most part highly speculative theories whose evaluation is generally not based on scientific methods but left to the individual student.

CURRENT FIELDWORK

CCETSW's prescriptions for fieldwork (e.g. CCETSW, 1989) have a strong scientific element, portraying the

social worker acting in a reflective manner, drawing systematically on a body of knowledge, evaluating his own work, and turning to research reports for extra evidence on the effectiveness of different strategies. However, as the following examination shows, few fieldworkers follow this model.

In striking contrast to CCETSW's frequent reference to a _common_ body of knowledge, the strong element of personal choice apparent in training courses continues in later practice as a qualified worker. A review of research studies of fieldwork carried out by the Department of Health and Social Security commented that:

> there is no way of knowing, from present research, what affects the choice of strategy adopted for particular clients, but it does appear to be left largely to the discretion of individual social workers (1981, p.65).

Parsloe and Stevenson, in their extensive study of 31 social service teams, reported a similar finding:

> a feature of all the studies was the wide ranging freedom which social workers had to choose the style and content of their direct work with clients (1978, p.134).

The same degree of personal choice appears to be enjoyed by the Probation service according to Boswell (in an unpublished study reported in Davies, 1982).

CCETSW stresses the importance of reflective practice, of the need for social workers "to analyse, clarify, and conceptualise issues" (1989, p.11). They also specify that: "qualifying social workers must be able to understand the need for reflection on process and outcomes of social work intervention" (1989, p.16). But most fieldworkers do not work like this and, when questioned by researchers,

have great difficulty in articulating what they do or why they are doing it.

Goldberg and Warburton (1979) found this when they started to develop a record and review system for use in social services departments. Their aim was to collect standardised information on clients, the problems being tackled, and the methods and goals of social work intervention. The researchers began by discussing some cases, chosen more or less at random, with each of the 20 social workers involved in the study, hoping to get some general ideas of how the workers saw their jobs in order to to form the basis of a recording system. They reviewed 113 cases in this way but they found that the fieldworkers had great difficulty in describing precisely either their goals or how they hoped to achieve those goals. Aims were expressed in indeterminate terms such as "improving social functioning". When asked their plans, most social workers described the client's present circumstances and difficulties but could not specify what they themselves proposed to do to help.

Parsloe and Stevenson ran into the same difficulties in their study of fieldwork:

> several social workers indicated that they were
> unaccustomed to conceptualising or reflecting upon
> their practice (Parsloe and Stevenson, 1978).

Sainsbury also found that social workers did not have precise goals:

> the casework we studied started with some kind of
> agreed (or at least compatible) task-orientation, but
> then sometimes drifted into a travesty of the
> diagnostic model, in which service continues
> indefinitely and on the assumption that warm
> relationships are all that are required to bring about
> improved social functioning (1980, p.10).

50

Theoretical knowledge is highly valued in social work training and judging students' ability to apply theories in practice is an important element in assessing their competence. But again, fieldworkers do not follow the model set down by CCETSW and taught to them. Studies trying to find out which theories, if any, are used by social workers have consistently found that, while social workers report that theories are influential, they are rarely used in an explicit and systematic way.

Parsloe and Stevenson's extensive study is again a good source of evidence. It concluded that social workers did not, in the main, adopt a specific theoretical approach:

> on the whole, our respondents' descriptions of their work with clients did not suggest that practice was drawn from specific theoretical perspectives. It may be that they had so internalised theory that they put it into practice without being conscious of it or able to talk about it. One experienced worker commented: "If you ask me to state a theory here and now, I wouldn't have a clue but my thinking and approach have been formed by it". There was some evidence that many of the experienced workers were accustomed to working mainly on the intuitive level – in the sense of responding to the immediate situation without conscious reliance on a theoretical framework (1978).

Carew's study supports the finding that theories, if used at all by social workers, are employed in a piecemeal and mainly unconscious way. He employed only a small sample of 20 but the study is interesting in its detail and methodology. His aim was to discover whether social workers applied theoretical reasoning in their work and whether they took account of research results. His methods were not only direct questioning (as used by Parsloe and Stevenson) but also studying the transcripts of interviews between social workers and their clients. The smallness of

Carew's sample is perhaps evidence of social work hostility to research because he had approached another 41 workers in the department but they had refused to join in. Most of them claimed that they refused because they believed that participation would breach confidentiality but Carew suspected that this was not the true reason because he had taken strict precautions to protect privacy. All who took part answered a questionnaire about their use of theories and their views on its importance. Each also provided a transcript of an interview with a client, chosen by themselves, which was then analysed using a modified version of Hollis' Typology of Casework Treatment. This typology classifies the purpose of statements made by social workers. 91% of the social workers' comments showed the use of techniques and procedures which encouraged the client to talk and reflect. 8% of their responses appeared to indicate the use of theory or that they were aware of research findings. Carew then interviewed the participants to check whether these 8% of comments had actually been related to theory or research. After analysing the data, Carew concluded:

> few of the responses reflected the use of theory and research findings. The situation never occurred where respondents clearly indicated that a response was based upon theoretical knowledge or generalizations from research; for example, no-one made such remarks as : "I asked that question because Steele has indicated that a significant proportion of abusive parents were abused by their parents in childhood." They tended to suggest instead that the primary base for their activities was either their own experience or advice from their more experienced colleagues. However when the researcher finally suggested an author, a theory, or a piece of research that might be related to what they had done, then the respondents would sometimes be able to link their activities to a theoretical framework (Carew, 1979).

Besides making little use of the standard theories,

social workers tend not to theorise about their own work. Compton and Galway's (1975) research showed that, in journals, social workers usually discuss only individual cases without generalising from them. In relation to a particular client, they will explain what help they gave and why they think they achieved results, but they refrain from considering the relevance of their experience for other clients.

Carew's finding on social workers' indifference to research is supported by several other studies both in Britain and the U.S.A. While CCETSW asserts that "social workers must be able to apply research findings to practice" (1989, p.11), Rosenblatt (1968) and Kirk et al (1976) both found that few American social workers read research reports, used their results in practice or rated research as helpful.

Similar conclusions were reached in Britain by Davies (1974) and Shaw and Walton (1978). As Davies commented: "it cannot truly be argued that empirical studies have made much positive impact on the traditional pattern of diagnosis and treatment in casework."

Despite the avowed aim of their training, it seems that most fieldworkers do not adopt a scientific approach to their work. Indeed it seems that many are positively hostile to science. Writers with considerable experience of fieldworkers have reached a similar conclusion about the prevalence of anti-scientific attitudes among them. Timms and Timms report:

there is detectable in social work a considerable
scepticism concerning the place of generalisations and
the validity of knowledge that does not proceed from
direct practical experience in helping individual
people (1977, p.118).

Goldberg and Warburton make a comparable judgement

after their extensive study of a social services team:

putting people into categories and quantifying
phenomena, which in part consist of subjective
experiences, is at variance with the social worker's
belief in the uniqueness of each individual experience
and the need to individualise problems in order to
help people in their difficulties. Some social
workers feel that social work is an art based on
intuition and feelings and the ever-changing dynamics
of a therapeutic relationship which are not amenable
to scientific analysis (1979).

Sheldon also found social workers hostile to science,

describing the group as:

increasingly anti-intellectual in its approach to the
problems and issues of social work, suspicious of
outside research and preferring still to rely largely
on personal impression as a way of monitoring its
objectives (1979).

For most social workers, helping is seen as a personal

skill, relying on the empathic skills and intuitive

understanding of the fieldworker. Theoretical knowledge

from the social sciences can be useful in enriching that

intuitive understanding but, where academics talk of "a

body of knowledge", fieldworkers prefer to talk of

"practice wisdom": the insights and understanding each

worker acquires through experience with clients.

Sheldon who has had many years experience as both a

practitioner and teacher of social work describes the

typical style of social work:

(a) time and energy is devoted to providing friendly,
supportive, and confidential relationships with
clients, in which context personal problems can be
discussed freely, and where an attempt is made to
understand behaviour - whatever its shortcomings in
the eyes of the community at large;

(b)   an attempt is made to analyse problems and   their
antecedents,   and clients are encouraged to reflect on
possible lessons and solutions - the medium of   change
is verbal influence;

(c)   there   is   an   intention   that   an   individual's
attitudes   and   behaviour will   change as a   result   of
this process, and that he or she will feel better, and
more able to cope as a result of it;

(d) implicitly or explicitly some use will be made   of
psychological concepts,   both to provide   explanations
of problems,   and to guide the social worker's actions
in trying to overcome them (Sheldon, (1982, p.10).

Carew, whose   study   involved discussing   cases   with
social workers, came to a similar conclusion about the main
method of working:

they   (the social workers) would get their clients   to
state   their problems,   discuss their   feelings   about
them,   reflect   on the causes and on how the   problems
could   be overcome,   and verbalize their   reflections.
If the problem could be overcome through the provision
of resources, the workers would use their knowledge of
the availability of resources,   and the procedures and
legislation   related to these,   to help the client   to
obtain them.   If the problem could not be overcome in
this way,   the client would be encouraged to ventilate
and reflect further (Carew, 1979).

These   accounts of social work practice show a   strong
resemblance to ordinary ways of helping.   For most   social
workers,   there   is   only   a   tenuous   link   between   their
theoretical studies and subsequent work and between   formal
knowledge and practice.   The dominant way of working relies
heavily   on   personal factors,   on the   worker's   intuitive
skills and a body of implicit knowledge derived partly from
experience and partly from training.   Little effort is made
to   formulate   the aims of practice and the   principles   on
which they are working,   or to share the insights they have
gained from their experience.

This   approach   to social work resembles an   art   more
closely   than a science or craft.   The artist is taught   a

55

variety of techniques for painting and studies a number of styles. He then selects the techniques that most appeal to him and develops his own personal style. In a similar way, social workers often describe themselves as selecting "what works for them", or "what suits their personal style of work".

Yet whatever similarities there may be, there is one crucial difference between art and social work. The artist's task is the relatively harmless one of putting paint on canvas or writing poetry or prose; if he produces an unpleasing picture he can clean the canvas or throw away the paper. Social workers on the other hand affect other people's lives, being the only source of help for many. They now have immense responsibilities and powers, and, in some cases, are instrumental in depriving people of their liberty or parents of their children. Clients can choose between Elgar and Eastenders but they are hostages to fortune when they need social work help.

Scientific evaluations of this type of social work, however, throw serious doubt on its effectiveness.

For a time, social workers, especially in America, seem to have been willing to use scientific methods to evaluate their interventions. In the 1950s and 60s, there were several large-scale trials of social work, mainly in the U.S.A. It was a time of great expansion of social work and of a strong belief in the power of social workers to help their clients, fuelled by confidence in the psychoanalytic theories which underpinned their practice. The various trials were not carried out by hostile outsiders who wanted to check on social work but with the

support and co-operation of the fieldworkers who were
evaluated. Mullen and Dumpson describe the mood in which
this research was conducted:

> social work emerged from the 50s with confidence
> concerning its effectiveness. As a profession, it
> sought expanded opportunities and resources to
> demonstrate its competence. The 60s witnessed a
> marked increase in those opportunities, and social
> work set about to demonstrate its relevance. Out of
> this confidence social workers boldly exposed their
> practice to the critical scrutiny of scientific
> evaluation and assumed that such evaluations would
> assist them as they refined their technologies and
> expanded their knowledge. They assumed, too, that
> these evaluations would clearly demonstrate the
> effectiveness of their interventive efforts (1972).

Unfortunately, as Mullen and Dumpson then go on to
show in reviewing these studies,"the researchers, for many
reasons, were rarely able to conclude that a program had
even modest success in achieving its major goals."

These studies have been examined, analysed, and
debated by many authors e.g. Segal (1972), Fischer (1973
and 1976), Wood (1978) and Sheldon (1986). While some have
tried to put the point in a gentler manner, none has
disagreed with the negative conclusion reported above.
Fischer, for instance, concludes that:

> lack of effectiveness appears to be the rule rather
> than the exception across several categories of
> clients, problems, situations, and types of
> casework (1973).

These controlled trials have failed to endorse
fieldworkers' confidence in the value of their help. They
raise acute doubts about the quality of help clients are
receiving. But they have met a mixed reaction in social
work.

Some academics have taken them seriously and argued
strongly for the need to search for more effective methods
of social work. The best-known advocate of this view in

Britain is Brian Sheldon (1979, 1982, 1986) and in the U.S.A. Joel Fischer (1973, 1978, and, with K. Corcoran, 1987).

The majority of fieldworkers however seem to have paid as little attention to these results as to any other, appearing to consider them invalid measures of their work. Sheldon, highly critical of his colleagues' "ostrich-like" response, describes how, at a lecture in 1968, he and the rest of the audience first heard of the negative results of a famous American evaluative study "Girls at Vocational High." He reports that, while he was disturbed by this unexpected failure: "I was much more worried (and still am) by the complacent smiles of colleagues all around me" (1979, p.27).

The majority of social workers' rejection of the results of scientific appraisal of their work applies consistently to positive as well as negative results. Those academics who have looked for helping methods with greater evidence of effectiveness have settled on behavioural therapies (e.g. Fisher 1978 and Sheldon 1982). The behavioural approach in psychology has always had a strong scientific commitment. Empirical research is a central feature in developing the behaviour modification techniques. Though the aims of behavioural therapies are usually more limited than their analytic counterparts, controlled trials have produced impressive evidence of success especially in treating phobias and obsessions (see for example the reviews by Agras et al, 1979, and Rachman and Wilson, 1980). Social work using behavioural methods has also produced positive results (see Reid and Hanrahan

in Goldberg and Connelly, 1981, and Fischer, 1981.)

And yet, most social workers are no more impressed by these successful trials than they are deterred by the reported failure of analytic therapies. As all writers supporting a behavioural approach in social work recognise, they are addressing a very hostile audience.

By ignoring the results of evaluative studies, social workers are demonstrating the strength of their hostility to science and their preference for individual, intuitive appraisal. These results however cannot be simply dismissed as irrelevant or inaccurate and they provide strong grounds for questioning the effectiveness of current styles of social work practice.

CONCLUSIONS

The aim of this chapter has been to describe the kind of expertise used in social work but this has proved to be a difficult task. Knowledge of the law and relevant services are certainly elements in this expertise but there is no uniformity in the remaining part. Rather, it varies from one individual to another, consisting mainly of each person's empathic and intuitive skills, and the ideas from the social sciences which he or she has deemed plausible and useful.

It has long been debated whether this personal style of working could or should be replaced by a more scientific approach.

The pioneers of formal training programmes proceeded

on the assumption that helping in social work could be more than just common sense and practical knowledge. They believed that the developing social sciences offered a more accurate understanding of personal and social problems and so had the potential for providing more effective ways of solving them.

Current training, prescribed by CCETSW, the professional body responsible for validating training courses, still has this commitment to a scientific approach. According to CCETSW's guidelines, students are to be encouraged to work in a reflective, goal-oriented way, drawing on theories from the social sciences to understand their clients, using therapeutic methods in a systematic manner, and turning to research for empirical evidence about the accuracy of theories and the effectiveness of therapies.

CCETSW clearly believes that social work can and should develop empirically supported, publicly available theories to guide fieldwork but its claim that social work already has a "common body of knowledge" seems unfounded. The theoretical knowledge offered to students is vast, conflicting, and little tested. It is neither common nor well enough supported to claim the title of "knowledge", at least according to the cannons of science favoured by CCETSW.

There is however a serious split between this scientific approach and the view of social work held by most fieldworkers. As Sheldon expresses it, "two different sub-cultures are developing within the social work movement" (1978, p.20). Most practitioners adopt a

very different approach from the scientific model, making a
personal selection from the theories they have been taught
and developing an individual and private style of working
which they have trouble articulating. Expertise is thought
to develop more through experience with clients than from
reading social science books. Moreover, many see their
intuitive and empathic approach as directly in conflict
with the scientific tradition they hear about on their
training courses.

Controlled trials cast doubt on the effectiveness of
social work. From the research evidence currently
available we have no grounds for complacency about the
quality of help clients are receiving. Summarising the
impact of the social work research of the the 1950s and
60s, Sheldon comments:

> this body of research, when combined with British
> findings and with similar material from psychotherapy
> and psychiatry (Clare, 1976) marks the end of half a
> century of optimism about the ease with which
> behavioural changes can be induced by verbal
> counselling methods (1982, p.21).

Equally importantly, the results of the empirical
studies question the reliability of fieldworkers' appraisal
of their own work. It appears that American social
workers, over many decades, developed and refined methods
of work which they mistakenly judged to be very effective.
And yet, current practice relies almost entirely on
individual appraisal, both in judging the truth or
credibility of theories and monitoring the service clients
receive.

The debate about science is not just an internal social
work issue. As the previous chapter illustrated, social

work has an important role in our society. The increase in duties and powers is due at least partly to social work's claim to have particular expertise. Social workers have turned to science for theories but science has also provided the means of testing their expertise, producing serious doubts about its quality. Research evidence though appears to have little impact on the dominant non-scientific culture of fieldworkers. They continue to rely on monitoring their own efforts and continue to be optimistic about the power of verbal counselling. Their hostility to science also leads them to belittle the apparently damning evidence about their work. Since their rejection of scientific methods has such far-reaching consequences, their reasons for doing so need to be critically examined.

CHAPTER FOUR

FORMULATING "PRACTICE WISDOM"

INTRODUCTION

My appraisal of social workers' objections to adopting
a scientific approach begins by examining claims that, when
studying human behaviour, one cannot formulate theories
akin to those found in the natural sciences.

Certainly, current social work practices present a
strong contrast with the sciences. In place of the explicit
and publicly testable theories of science, most social
workers rely mainly on their personal skills, of empathy
and intuition, and what is generally called "practice
wisdom", a body of implicit ideas each worker acquires
partly through experience and partly from their formal
social work education. Because of the tacit, vague, and
unarticulated nature of this form of reasoning, the
standard scientific methods cannot be applied. Critics of
science, however, maintain that this state of affairs is
appropriate and unchangeable, claiming that practice wisdom
cannot be articulated and examined in the form of
scientific theories.

A central allegation in their argument is that
scientific methods cannot be applied to the study of the
human mind. "The self cannot be observed ... nor can it be

measured, so it must be ignored" — this Wilkes (1981, p.75) claims is the only possible attitude scientists can take to the human mind. Therefore, she argues, since social workers are very interested in the mental world of individuals seeking help, there is an unbridgeable gap between science and the traditional methods of reasoning in social work. Fieldworkers are concerned with "the self", with people's hopes, thoughts, and feelings. Scientists on the other hand, Wilkes claims, can only apply their methods to observable behaviour.

Many others in social work echo her view. Both Ragg (1977) and Raynor (1984), for example, assume that there can be no scientific explanations of actions in terms of reasons. Jordan (1979) and Goldstein (1986) also depict a scientific approach as necessarily limited to the study of behaviour and contrast this with the typical social work interest in the client's inner experiences.

These writers are assuming a view of science which, I shall argue, is mistaken. As a paradigm example of science, they cite the extreme form of behaviourism associated with J.B. Watson who deemed that psychologists should not study the mind but only observable behaviour. His exclusion of mental phenomena, however, is not demanded by scientific methodology. In the natural sciences scientists do speculate beyond the simply observable and indeed do so very profitably. Contemporary theoretical physics, for example, is very largely concerned with the study of unobservable phenomena such as quarks and electrons. If such strange entities can be the topic of scientific enquiry, our thoughts and feelings cannot be

excluded on the grounds that they are not directly observed.

Moreover, I shall argue that the account of scientific theorising which I report is not only compatible with but also closely resembles social workers' reasoning. There are many similarities in the way that social workers and scientists speculate about the phenomena they are trying to understand, whether it is the delinquent activities of a teenage boy or the movements of the planets. Therefore, I shall contend, formulating practice wisdom, though a difficult task, cannot be said to be impossible.

To reach this conclusion, I first examine the kind of understanding so valued in social work at present - the way we typically explain people's intentional actions in everyday life.

A central issue in the philosophy of the social sciences has been the relationship between this form of understanding and a scientific study of mankind. Humanists and some behaviourists have claimed the two are incompatible. The former then reject a scientific approach and the latter adopt a radically different framework which does, as social workers complain, focus on behaviour not mental phenomena. The third position in this debate however, and the one I defend in this chapter, is that our ordinary understanding can be scientifically developed.

I shall examine the rationale behind the development of behaviourism and show how it uses a more restrictive methodology than most natural sciences. The way theories are developed in the natural sciences will then be

discussed and the ideas applied to social work issues,
first analysing the ways social workers reason and
highlighting their resemblance to scientists, and then
examining studies which have tried to formulate what social
workers do.

INTENTIONAL BEHAVIOUR

If a branch taps at the window, we wonder what caused
it to happen; if a man taps at the window, we ask why he
did it. In everyday life, we think of each other's actions
as intentional, as done for a purpose. Unlike the movement
of a branch, we try to understand human actions in terms
of reasons. We want to know people's thoughts, feelings,
and their goals which, we presume, led them to do a
particular thing. Social sciences have only a recent
history but mankind has, from ancient times, been
interested in understanding his own and others' actions.
The "folk psychology" that is characteristic of ordinary
life is extensive and, for many everyday purposes, very
successful.

The following discussion of this everyday way of
understanding people is intended to clarify the type of
skills and understanding the majority of social workers
consider are needed in helping clients.

First, our explanations purport to tell us what is
going on in someone's mind. We claim we can know their
feelings, their beliefs and their intentions and so we can
understand their reasons for deciding on a particular
action. When judging whether particular reasons provide an

adequate explanation, we assume some kind of rationality principle. Philosophers have found it difficult to formulate this in an uncontentious way but, to use Rosenberg's example, a simple version of it is:

> given any person X, if X wants D and X believes that A is a means to attain D, under the circumstances, then X does A (Rosenberg, 1988, p.25).

The principle is, I think, made clearer if we look at an example of an unsatisfactory explanation. Let us suppose that we are given the explanation that X went to shop B because he wanted to buy a book and he knew it was only stocked at shop A. This is not an adequate explanation of X's action and, indeed, make us more not less curious. On these facts alone, his action is not intelligible but puzzling. We would want more information, perhaps of another goal which X rated more highly and which could be met by going to shop B, before his action seemed intelligible to us.

Within folk psychology, people accumulate a fund of background knowledge about individuals' motivation to help them understand each other. As we grow up, we gradually learn elementary psychology, patterns of behaviour, and social rules. Social workers share this kind of understanding and, in addition, say that they develop "practice wisdom", that is, insights they acquire because of their special contact with people in trouble and their theoretical training. The ideas are however for the most part implicit and the methods of reasoning intuitive; we produce an "interpretation" of an action, an explanation which makes it intelligible to us.

A social worker might, for instance, be initially puzzled by a parent's refusal to accept what seems to him to be much-needed services for his recently handicapped child. Then however, remembering how other clients had reacted, he might speculate that, despite a clear message from the doctors, the parent is not accepting their diagnosis and still expects the child to recover. He might link this to an hypothesis from psychoanalytic theory about the psychological mechanism of "denial", not believing something which seems too painful to accept. On this interpretation of the parent's reasoning, his action now looks "intelligible": if you believe your child is going to recover, services suitable for the long-term handicapped will seem inappropriate.

Much ordinary psychology relies on empathy. To help us work out other people's reasons, we can draw on our own experience and "empathise" with them, that is, imagine what it would be like to be in their circumstances and think how it would feel. So sometimes, when we say we understand someone, we mean that we know what it feels like to be in their position; we, in a sense, share their experience. Equally we may say that we cannot understand somebody; however detailed an account we may be given of Hitler's motivation for example, we may claim that we could never understand, i.e. empathise with, his deciding to kill millions of Jews.

Some social workers argue that empathic understanding is essential in social work not just as a means of understanding the client but as a therapy. Jordan (1979) for example makes this claim:

empathy implies that the helper "feels with" the person in trouble; that by imaginatively entering the other's situation, he engages his own emotions in such a way as to share the other's responses. I am suggesting that this is an essential part of helping.

The majority of social workers at present view helping as predominantly a personal aptitude. Empathic and intuitive skills are highly prized; "understanding" is based on each worker's individual fund of background ideas; testing the accuracy of that understanding is also done mainly by the individual. But in social work, as in the social sciences, there has been a long-standing debate about whether we can improve on the understanding embodied in this folk psychology.

While commonsense understanding may be reasonably successful in everyday circumstances, in areas such as social work where correctness of interpretation affects the life and happiness of others, it is particularly important that understanding should be accurate but, on this score, our commonsense wisdom is limited. Our understanding is often only partial or later seen to be inaccurate. Disagreements are common and difficult to resolve. Our ability to predict is poor. And, importantly for social workers, the implicit nature of our intuitive reasoning makes it difficult to share understanding, to teach others the insights we have gained. The natural sciences, on the other hand, have made impressive progress, producing public theories which enable people to explain and control many aspects of the natural world. Influenced by this contrast, many have argued that the social sciences need to copy the natural sciences to achieve a comparable success in the

social realm. In social work, those who argue for using scientific methods hope that the methods which helped to eradicate smallpox from the world will also help to end the human suffering caused by social and psychological problems.

But what is entailed by adopting the methods of the natural sciences? The relationship between our everyday type of understanding, which is couched in terms of reasons and intentions, and a scientific study of people has long been a key issue in the philosophy of the social sciences. There are three main positions:

The first is that scientific explanations and our ordinary understanding are incompatible. From this it is concluded that scientific methods cannot be used in social studies. This is the humanist view which is the dominant one in social work.

The second position again considers that scientific explanations and our ordinary understanding are incompatible but it is then inferred that social scientists must reject the common sense view and adopt a different framework for studying people. This is the position taken by behaviourists such as J.B. Watson and which many social workers consider is the only alternative to humanism.

The third view is that our ordinary understanding is compatible with scientific explanations; social scientists can revise and test rather than wholly reject the ideas in folk psychology. On this view the practice wisdom in social work can form the basis for scientific theories.

In arguing for this final position, I first examine

the rationale behind behaviourism and show how its rejection of ordinary explanations of human actions is not demanded by scientific methodology.

BEHAVIOURISM AND THE EXCLUSION OF MIND

J.B. Watson was the first behaviourist, and indeed coined the name. Writing and lecturing at the beginning of this century, he argued that the proper subject matter of psychology was not the mind but human behaviour. His view was a reaction to the problems he saw in the prevailing orthodoxy of Introspectionism. According to that school, mental phenomena both could and should be studied and the main way of doing so was through observing one's own mental processes - "introspection". Plausible though this idea is, it ran into difficulties.

First, introspection proved to be of very limited use as a method of investigation since, as was soon recognised, many important aspects of our mental processes are unconscious. Nor does it seem that these unconscious operations can they be made conscious even by careful inner observation. Marbe's study in 1901, (reported in Mandler and Mandler, 1964, p.143) was an influential illustration of this shortcoming of the method of introspection. He asked his subjects to compare different weights and to report their mental processes as they did so. But their efforts at introspection did not directly reveal the process that led to their judgments about the relative weights. Instead, they reported experiencing hesitation, doubt, waiting for an answer, and feeling that the answer

had arrived. They concluded that the process of judging was not carried out at a conscious level and introspection alone seemed unable to reveal how it was done.

Behaviourists however argued against introspection even as a method with limited use. They claimed that the whole method was essentially unscientific. The problem they saw was that, since introspective reports were of private experiences, they could not be verified by others. An early behaviourist, de Laguna, asserted that conscious processes:

> can not by the very nature of the case be objects of scientific study. For it is an essential condition of scientific investigation of any phenomenon that observations made by one individual shall be verifiable by others (de Laguna, 1919, p.297).

Finally, behaviourists also criticised the failure of introspectionists to achieve intersubjective agreement. If people exposed to the same stimuli described different perceptions disagreements were common but there was no way of settling the issue. One unresolved dispute between two introspectionists at a meeting of the Society of Experimental Psychologists has become famous as an illustration of how rational debate came to a halt when subjective accounts conflicted. One eminent psychologist, Titchener, after a heated debate with Holt, a colleague, exclaimed:

> 'You can see that green is neither yellowish nor bluish!' and Holt replied: 'On the contrary, it is obvious that a green is that yellow-blue which is exactly as blue as it is yellow' (reported in Boring, 1970).

Because of such perceived problems, behaviourists concluded that introspection was not an acceptable

procedure in a scientific discipline. Zurriff, after an extensive analysis of behaviourism, concludes that: "the central behaviourist position is the rejection of introspection as a method of scientific observation" (1985, p.28).

Watson's solution to the problems of introspectionism was radical. Having decided that it was difficult to study mental phenomena, he took the decision not to study them at all but to concentrate on phenomena which were more readily accessible to reliable and inter-subjectively verifiable observations, namely behaviour. He not only decided that subjective reports of inner experiences could not provide the empirical evidence needed to test theories scientifically, he also excluded from theories the concept of the mind entirely.

Watson turned to the natural sciences for guidance on methodology. However he looked not at the more esoteric realms of theoretical physics but the study of animal behaviour. He was impressed by research such as that being carried out at the time by Edward Thorndike who made considerable progress in understanding animal learning without reference to any mental experiences they might have. Apparently adequate explanations of their learning behaviour could be given referring only to observable features of the animals' behaviour and environment. Extending this behavioural approach to the study of people was, Watson decided, the way to develop a successful science of psychology. He argued that people too could be adequately understood just in terms of behaviour and environment. The behaviourism he advocated aimed, he said:

to apply to the experimental study of man the same kind of procedure and the same language of description that many research men had found useful for so many years in the study of animals lower than man. We believed then, as we do now, that man is an animal different from other animals only in the types of behaviour he displays (Watson, 1930).

Watson went further than this. He held not just that, first, one could not investigate the mind scientifically, and, secondly, one did not need to in order to understand human behaviour, but he also doubted the very existence of the mind: "the behaviourist holds that belief in the existence of consciousness goes back to the ancient days of superstition and magic" (Watson, 1924, p.2). To make scientific progress, his advice to psychologists was:

let us limit ourselves to things that can be observed, and formulate laws concerning only those things. Now what can we observe? We can observe behaviour. (1924, p.6.)

Watson was, for a long time, very influential, and psychologists, following his prescriptions, kept to observable facts, of behaviour and the environment. They did not speculate about hidden processes in the possibly non-existent mind but tried instead to establish causal connections between different types of behaviour and events in the environment. The two central theories that grew out of this research were of those classical and operant conditioning, which specify conditions under which new behaviour is learned or existing patterns tend to fade away in response to the stimuli and re-inforcements the individual experiences.

Behavioural theories are radically different from the type of understanding present-day social workers typically value for they omit people's thoughts, feelings, and

74

intentions. Skinner, another leading behaviourist, is
emphatic that such mental phenomena need not figure in
investigations of behaviour:

> we do not need to try to discover what personalities,
> states of mind, feelings, traits of character, plans,
> purposes, intentions, or any other perquisites of man
> really are in order to get on with a scientific
> analysis of behaviour (1973).

However, behaviourism is not the only way to use
scientific methods in studying people. The central
argument in the behaviourists' position is that assertions
about mental phenomena cannot be verified and are therefore
unscientific. But this inference is invalid. Scientific
theories that are widely accepted have not merely recorded
correlations but often go beyond the facts, explaining
those observed regularities by postlulating underlying
processes which are not directly observable. Natural
scientists talk of imperceptible entities such as sub-
atomic particles. Therefore, mental phenomena cannot be
banned from scientific explanations of behaviour just on
the grounds that they are not directly observable.

Indeed within the behavioural movement itself,
Watson's radical exclusion of unobservable entities was
soon challenged. As early as 1932, Tolman argued that to
provide a satisfactory explanation it was necessary to
introduce the concept of the 'intervening variable',
something which occurred in the individual between the
observed stimulus and the behavioural response. This,
Tolman said, did not need to be directly perceptible — and
so could be a mental process — as long as hypotheses
containing it implied observable results. Most

behaviourists would now take the same view. According to this modified version of behaviourism, it is perfectly acceptable to have theories referring to unobservables such as mental processes but behaviour still remains important to them as the observable test of theories.

Developments in cognitive psychology undermine even further the claims of social workers hostile to a scientific approach that mental phenomena cannot be the subject of scientific study. Explanations in terms only of people's behavioural or physiological responses were found to be inadequate in many areas of behaviour. Psychologists therefore have turned their attention to studying the role people's thoughts and feelings play in determining their reactions to stimuli. Their responses are seen as active interpretations of what they perceive. The importance of cognitive factors is also acknowledged within the related therapeutic techniques. Cognitive-behavioural treatments are as concerned with people's beliefs and emotions as with their behavioural and physiological responses.

The advocates of a scientific approach in social work do not equate it with Watson's type of behaviourism. The picture is somewhat confused though by the fact that many of the most vociferous champions of scientific methods also urge the adoption of the therapeutic methods developed by behaviourists. Their reason for this however is the weight of evidence about their therapeutic success not because they are considered to be the only scientific option. Sheldon makes this clear in his book on behaviour modification (1982). In other publications where he is arguing for using scientific methods, he sees them as

compatible with non-behavioural approaches (e.g. 1979 and 1983).

Hudson and Macdonald also argue that social workers should use behavioural modification therapies but again it is on the grounds of their effectiveness. They say that in dealing with clients the crucial criterion in selecting a therapeutic method is "effectiveness":

> being a "social worker" is different from being a "friend", and it is important to locate the additional ethical constraints imposed by the social work role. And in our view the central such constraint is "effectiveness" (Hudson and Macdonald, 1986, p.9).

On the available evidence, behavioural modification therapies, they argue, meet this ethical criterion more than any competing approach.

The evidence in favour of behavioural modification techniques has so impressed some that they doubt the competence of social workers who prefer other approaches. Brewer and Lait comment:

> we do wonder, however, whether those teachers of social work who have resisted the introduction of behavioural methods should be considered as fit members of academic communities which are supposed to adhere to certain basic standards of scientific debate (Brewer and Lait, 1980, p.101).

Again their reason is the evidence on effectiveness not any unique claim to scientific status: "we would welcome any other approach which can be shown to be effective" (1980, p.190).

Social work critics of science are wrong then to equate science with Watson's extreme form of behaviourism. They are not alone however in making this mistake. Giedymin, reviewing the debate between humanists and naturalists in the social sciences generally, reports a

similar tendency for humanists to attack Watsonian behaviourism although this is not the view of science their opponents are proposing:

> contrary to the claims of anti-naturalists (Winch, Wright, for example) none of the naturalists in my survey claims that descriptions in the social sciences (and humanities) are or ought to be purely phenomenalistic, i.e. in behaviouristic terms, that explanations of human actions are or ought to be mechanistic, without reference to human aims, beliefs, etc. Just the opposite is the case: they all insist that social sciences and humanities are concerned primarily with studying men as rational beings ... they all agree that typical explanations of individual actions in history (and humanities generally) are in terms of aims, intentions, beliefs, available means, existing obstacles, institutional set-ups, etc (Giedymin, 1975, p.290).

Why do so many opponents of science make this mistake? Social work critics do not spell out exactly what they think scientific methods are but they seem to be assuming a simple inductivist view which is now generally discredited.

Francis Bacon is the philosopher best known for this view, though it has recently been shown that this is an inaccurate reading of his work (Urbach, 1987). The standard interpretation of his writings however is that he thought that scientists began by amassing a set of facts, either by casual observation or deliberate experiment. They then employed a set of inductive rules so simple that practically anybody could use them to develop a body of certain knowledge. Scientific method, on this view, is both mechanical and infallible.

What is wrong with this view of science?

First, it assumes that there is a foundation of infallible facts. On closer examination, the distinction between observation statements and theoretical statements

breaks down. All statements, except tautologies, are fallible. (This has implications for the reliability of observations in testing theories. This point will be discussed in more detail in Chapter 7.)

Secondly, it ignores the creative element in theory development both in gathering information and in theorising about it.

Although observing the phenomena they are trying to explain is a necessary step, scientists cannot be merely passive fact-collecters and processors. They need imagination and intelligence to organise and theorise about the facts. In practical terms, collecting ALL the facts is impossible; scientists have to decide which facts are worth gathering. And when faced with a set of facts, there are an infinite number of patterns and correlations which could be identified; scientists have to determine which patterns that they observe are significant.

The creative element is also overlooked in that it fails to recognise the hypothetical status of theories. It assumes that there is some infallible process by which theories are built up from the facts and hence are justified by those facts. But theories go beyond the known facts in two ways. First they generalise from the observed instances to all cases, observed and unobserved, future and past. More importantly, given the subject of this chapter, theories often explain their observations by reference to unobservable processes.

In rejecting the claim that scientific theories are mechanically generated, philosophers have the task of

providing an alternative account. They distinguish between the "context of discovery" (the process of forming theories) and the "context of justification" (substantiating them.) The discovery of theories is seen as a somewhat mysterious, creative process. Most philosophical attention is given to the context of justification because we are interested in how "good" the theory is (how probable, or well confirmed) and this is considered to be independent of how the theory was invented. Hempel expresses a typical view:

> although no restrictions are imposed upon the invention of theories, scientific objectivity is safeguarded by making their acceptance dependent on the outcome of careful tests (Hempel, 1966,p.116).

This view of science does not support the claim that science can learn nothing about the mind. Rather, philosophers such as Nagel (1961, chap. 13) and Papineau (1978, chap.4) have pointed to the similarity between the conjectures of a scientist and our commonsense explanations of actions in which we speculate about the reasons for them. They also argue that empathy, the form of understanding valued by the humanist, should be seen not as a rival to scientific explanations but as an aid in developing them. In understanding human actions, as the humanist has always claimed, we are greatly helped by the fact that we are human ourselves and have access to at least some of our own mental processes. In explaining other people, we can draw on our experience and imagine how it feels to be in the other's position to give us ideas to explain their actions.

Most social scientists accept the need to study

people's thinking in order to explain their behaviour. In the 1930s and 40s, the German sociologist Weber was a famous and influential advocate of the idea that mental phenomena must figure in any explanation of behaviour. He argued (reprinted in Brodbeck (1968, Chap. 1)) that social scientists need to start with an understanding of intentional actions and this involves some reference to what was going on in the person's mind.

Social sciences study people's actions, social organisations, and belief systems; a central feature of all of these is that what they are depends in part on the beliefs and intentions of the people involved. To say that a man is "voting for Smith" is not just to describe his physical movements of putting a cross in the relevant place on the ballot paper but assumes that he has the appropriate knowledge about the voting system and the intention of showing his support for Smith. A child, playing around, might put a cross next to the name Smith but he would not be voting. A description of behaviour alone is insufficient to tell us what action is being performed.

To all except Watsonian behaviourists it has therefore seemed that the basic phenomena of the social sciences must include human mental processes. Consequently in the social sciences two layers of understanding are involved. The agent often has an explanation of what he is doing and this is an essential part of describing what his action is but his action can then be explained by the social scientist.

In summary, once Watson's type of behaviourism is seen to be only one example of science and an extremely narrow one at that, social workers' rejection of scientific

methods on the grounds that they cannot be used when studying the client's mind is untenable. The frequently-heard belief among social workers that the client's subjective experience is a key factor in understanding and helping him need not be abandoned in order to adopt a scientific approach.

THEORIES IMPLICIT IN "PRACTICE WISDOM"

Most social workers think that their ways of understanding clients is quite different from scientific reasoning. If scientific theorising is a mechanical process applied to observable facts it is very different from the social worker's imaginative conjectures about the client's mental experience. But scientists do not work in this mechanical way. They theorise in a way very like social workers' methods of reasoning. In this section I want to examine that similarity.

Most social workers deny using theories in the sense that they do not apply the theories they are taught on their training courses in an explicit and systematic way. They do claim to employ what is widely called "practice wisdom" however. If we examine their work, we can see that, despite their denials, they do use theories in that, like scientists, they generalise from their experience with individual clients, they try to establish causal explanations of how clients' problems arose, and they draw on such understanding to make predictions.

On the issue of generalising, most social workers

would deny that they form general principles about people but would claim that their main interest is in understanding the "unique individual". In case studies and discussions they tend to focus on particular clients and not to generalise about the people they have helped (Compton and Galway, 1975).

However while generalisations are avoided at an explicit level, they must implicitly be used. It is accepted within the humanist tradition that social workers can learn from experience, that insights gained from one client can be applied to others. Indeed such hypotheses are an essential element in practice wisdom. The history of the profession shows a growing acceptance that there are common principles in helping in social work. Beginning as diverse specialist services, it became unified as people recognised that similar skills and knowledge were applicable to a range of problems.

Moreover, social workers are in general not just concerned with noting correlations but, like scientists, with causal explanations. They do not just want to catalogue their clients' experience but to do something to help and so, in any account of social work practice, there are some assumptions about how to bring about change.

The only humanist writer who avoids any reference to causes is Wilkes (1981) but she does so at the cost of producing a view of social work which most would think quite inappropriate. She is hostile to what she deems the "technological" approach of science which emphasises the importance of changing clients and, she claims, tries to impose impersonal and inhuman causal explanations on our

subjective experiences. Equating science with Watson's type of behaviourism, she maintains that the humane approach for social workers involves nothing more than empathising with the client's experience. But she does not construe this advice as a therapeutic principle; it is not proposed as the means to achieving the technical end of changing the client but as the end in itself, a morally valuable enterprise. She believes that all change must be instigated by the client himself and she is so opposed to any efforts by social workers to alter people that she concludes her book with the advice: "do not explain, do not try to change, but just look" (1981). Bearing in mind the statutory responsibilities and powers of social workers today, few can find this advice workable. Social workers have to take an active approach to helping.

Other humanist accounts of social work, however, contain causal explanations. England, for example, provides the following general principle of how change should be brought about in social work: "the social worker consistently strives to understand the client's detailed experience and so to help the client clarify, accept and then act upon this experience" (1986, p.195). Further evidence of the role of causal hypotheses is apparent in the case studies which he offers as good examples of humanist work. In his final case study, for example, the social worker, who is counselling a woman with a chronic physical illness and marital problems, decides to support the client's decision to consult a homeopathic specialist. This support is not offered because of a belief in the

efficacy of homeopathy but because of the psychological
effect she expects it to have:

> if a person subjects himself to a medical procedure
> which is difficult — expensive, painful or
> disreputable — the best justification for undergoing
> it is the fact of a cure: "I'm glad I did it because
> now I'm better." The act of justification activates
> the person's unconscious into affecting the treatment
> (England, 1986, p.192).

Jordan, another leading humanist, also advances
general theories about how people can be helped, for
example: "if the client feels that the worker is seeing him
in terms of a pre-set theoretical framework, or imposing
something alien on him or experimenting with him, or using
him for some other purpose, he is unlikely to co-operate or
benefit" (1979, p.129).

Some of Jordans' principles, though plausible to him,
are highly controversial. For instance, he warns social
workers that: "trying to define and limit problems is self-
protective and unhelpful" (1979, p.72). This contradicts
the accepted view in behavioural psychology which holds, as
Kirk's manual on cognitive-behavioural therapy states:

> as the therapist helps to clarify and differentiate
> between problems, so the difficulties are frequently
> reduced to manageable proportions, and the patient
> begins to believe that change is possible (Kirk,
> 1989, p.15).

There is also evidence from research that social
workers often employ causal explanations in their implicit
reasoning.

Curnock and Hardiker (1979) and Hardiker (1981)
analysed reports social workers had written. Knowing that
social workers deny using theories but have difficulty in
saying what they actually do, the researchers decided to
examine their written work for evidence of their reasoning

and of any theoretical influences. Social inquiry assessments prepared by Probation Officers were studied by Curnock and Hardiker while Hardiker (1981) looked at 25 assessments made by social workers on child care referrals, analysing them in terms of their apparent theoretical slant.

Both studies found evidence of implicit theorising. In making their assessments, the social workers were not just describing the problem but also conjecturing its causes. A typical example Hardiker gives is of a family which was referred because the mother said she could not cope with her new baby. The social worker's assessment was of "a classic case of early bonding failure, possibly due to the physical circumstances of the child's birth, plus the mother's low self-esteem, social isolation and stressful marriage" (1981,p.95). Another example, assessing a case of child abuse, cited many contributory factors such as: a difficult birth, a stepfather with little experience or knowledge of child rearing, depression and social isolation in the mother, and the mother having been physically abused herself.

In the social enquiry reports prepared by Probation Officers too there is evidence that the Officers are trying to identify the causes of the criminal behaviour. The researchers found that the common areas that were examined were: the offender's personality; his health; family dynamics; social relationships (friends and at work); the neighbourhood; and economic circumstances. One Probation Officer, for example, thought that the particular family

dynamics of a thirteen year old boy contributed to his delinquency:

> I think this (marital problem) has some direct bearing on the boy's behaviour because the family situation is one in which conflict is quite apparent; it does not help him feel secure in his family and again he has this problem of finding out his own identity. The parents have unrealistic ambitions for their children. Therefore, he committed this offence along with his mates for reasons of status and group membership (Curnock and Hardiker 1979, p.43.).

Making predictions on the basis of their understanding of the problem behaviour is also an essential element in social work assessments.

The Probation Officers were involved in two types of prediction: the risk of further crime and the effects of the different sentences the courts could make. If they believed that there was a serious risk of re-offending, then they were more inclined to recommend a custodial sentence. Otherwise, their recommendations to the courts were based on predictions about the likely beneficial effect of the various options. Where crimes were thought to be the result of specific factors which were considered amenable to social work help and where the client seem co-operative, they advocated a Probation Order.

In child care work, predictions also figure prominently. Because of the statutory duty to protect the welfare of the child, social workers do not just try to understand the parents' problems but have to act; they have to use their insights to estimate the damage a child would suffer if left with his parents and to evaluate various ways of helping.

The resemblance then between social work practice

wisdom and scientific theorising seems strong. Perhaps one feature of science which has persuaded social workers that their understanding is of a different nature is that so many famous scientific theories make universal claims, such as "all gases expand when heated." In contrast, when social workers cite something such as family disharmony as a cause of delinquency, they would not claim that "all unhappy families produce delinquents." Nor would they make the claim that "all delinquents come from unhappy families." The causes social workers cite are usually seen as stresses, risk factors, or precipitating variables which do not fully determine a specific outcome but only make it more or less probable. If we attempt to formulate the intuitive wisdom of fieldworkers, we are unlikely to find many universal claims but hypotheses of the form "such and such a factor tends to increase the probability of "X" occurring" or "this often causes Y". While this differentiates social work wisdom from some of the most successful theories in science, it does not mark an absolute disparity. The language of probabilities is as much a feature of natural science as are universal laws. Indeed, in the field of quantum mechanics, it is debatable whether it could ever be possible to explain sub-atomic particles in any terms other than probabilities.

In summary, this review of social work reasoning indicates that, inasmuch as social workers are generalising about their clients, conjecturing about the causes of clients' problems, and using their explanations to make predictions, the preferred intuitive way of working looks not so much like an alternative to scientific

theorising but like an early stage of it.

Sheldon has also made this point, suggesting that social workers should see their intuitive ideas as 'embryo attempts at formulation' of general principles:

> within this model, art and science, intuition and formulation, practice and theory, are seen not as adversaries, but as related aspects of the same process of finding out and checking up on our beliefs (Sheldon, 1978, p.13).

FORMULATING SOCIAL WORK METHODS AND GOALS

The personal nature of current social work methods is recognised as a major problem by all who argue for a more scientific approach. It is particularly problematic in relation to evaluative research — studies have been criticised for giving inadequate detail of the social work intervention being evaluated so that others cannot implement the results. I have been arguing that it is possible to make social work reasoning explicit but this is not a simple task as the following appraisal of two research projects illustrates.

Two teams of researchers have devised data collection systems which are intended to elicit information about the methods and goals of social workers. The researchers had the same aim: to develop a way of recording what help social workers gave. They intended the information to be used in evaluating the social work intervention:

until we know what it is we are evaluating and can
formulate relevant descriptive categories for types of
clients, problems, social work/service inputs and
desired objectives there is very little point in
mounting such experimental studies (Goldberg
1979,p.9).

Both teams began by spending time with social workers,
observing their work or discussing it with them. On the
basis of this experience, recording systems were designed
to cover what each team had judged to be the salient
details of the helping process. Raynes reports:

the classification system we have developed enables
one to look with ease at the component parts of the
work carried out by social workers....It will make it
possible to identify the input of social workers so
that the possibility of evaluating their work becomes
greater (Raynes, 1982, p.359).

Goldberg makes a similar claim for the "case review
system" she produced :

as an information system it gives an on-going account
of the size, nature and scope of social work
activities with different client groups.......As a
research tool it can explore possible associations
between aims pursued in different problem situations,
methods and skills used (1979,p.29).

Copies of these systems are attached in Appendix A. A
look at them shows that there is a considerable difference
in the information each team decided to record. These
differences provide an illustration of the point made in
discussing science that observation is far from being a
mechanical, theory-free process. In recording what social
workers are doing and with what aim, it is necessary to be
selective, to decide what features are relevant and how
they are to be classified. Raynes claims to "identify the
input of social workers" and Goldberg says her system
provides an "on-going account of the size, nature, and
scope of social work activities" but if applied to the same

social worker they would produce different accounts of his methods and goals.

Their work is also interesting in that the two teams seem to have had different notions of their own role in devising these systems. Goldberg et al (1979) acknowledge that they were actively involved, with the fieldworkers, in deciding what information was significant and how it should be classified. Raynes et al (1982) however seem to have failed to recognise the active part they played in selecting and classifying the information. They portray themselves as passive collectors of data, the relevance or the classification of the data seeming to be decided by the data themselves rather than by the researchers active organisation of the material.

Let us examine the recording systems in more detail. To start with, the teams chose different ways of collecting data. The Goldberg system is completed by the social worker, and organises the data around each client, following their progress through contact with the social worker; Raynes' team, on the other hand, produced a form which focuses on the social worker, with an observer following him through his day recording his contact with several clients. Collecting the data in such different ways need not necessarily lead to the systems recording different facts but nevertheless it is not surprising that it did focus the attention on different aspects of practice.

The Raynes system is designed to collect three main kinds of information. These are described as individually

"necessary" and jointly providing a "sufficient" and "adequate" description of social work practice, adequate that is, according to the authors, for evaluative research purposes. The three categories are labelled "activities", "purposes", and "issues arising". An activity is defined as "an action that had a clear beginning and end." A few examples are: talking to clients or others, making phonecalls, travelling, and writing letters. This category alone, Raynes says, gives only a partial account of social work: "to identify and define the sum of activities which constitute the work of a social worker is to provide a necessary but not sufficient description of their work" (1982, p.356.) It is also necessary, she says, to identify the purpose of the activity and here 17 categories are listed, each covering a fairly broad range of aims. This set of categories seems a particularly good illustration of the active part played by the researchers. They use the neutral term of "identifying" the categories but produce a set which is likely to be controversial in view of their stated aim of developing a system for use in evaluative research. For instance, the following purposes are all put in one category:

giving/ receiving/ obtaining/ discussing information about client's history; practical situation; relationships with people; opinions; needs; feelings; resources; service use; future plans, with anyone other than a colleague (e.g. client, relative, friend).

This category roughly covers the area of counselling. To many social workers this is the key area in evaluative research where we want detailed information about different methods or styles of work but in this system all the

diverse actions have been massed together, not even distinguishing between talking to the client who is being counselled and to his friend.

The final group of information collected deals with "issues arising" - the problems or needs with which the social worker perceives himself to be dealing. For this, there are 16 categories.

If we turn now to the Goldberg "case review system", we get a very different picture of social work. Her categories were developed not by watching the social workers and "identifying" the salient features as Raynes did but by talking to them and helping them to formulate what they thought were the significant aspects of their work.

Information is collected about a particular client over time rather than recording the day of the social worker. There is most resemblance in listing clients' problems, a category which Raynes calls "issues arising" and Goldberg terms "problems" Both for example have similar categories for physical illness, employment difficulties, delinquency, problems in home management, and housing, but Raynes has one category for 'family' while Goldberg distinguishes between "child behaviour problems," "child neglect," "family relations problems" and "family break-up". Goldberg also further distinguishes between the problems which social workers have identified and those with which they are actively dealing.

Both systems list social workers' activities. The Raynes' form tells us about the physical action of the

social worker – seeing a client face-to face or telephoning for example; the separate category of "purpose of activity" gives more detail of why that action was carried out. Goldberg however tells us only of the social worker's purpose (e.g. exploratory, information/advice, or sustaining/nurturing) but not how this was actually performed. Their use of the term "purpose" is dissimilar. Raynes tells us of the immediate reason for the action – giving or receiving information, for example – whereas Goldberg seems to refer more to the social worker's therapeutic goal, such as "facilitating problem solving" or "review visiting". This difference is further highlighted by Goldberg asking the social worker to state what changes he/she is aiming for while Raynes has no category for collecting comparable data. This disparity may be due to the way the categories were formulated, in Raynes' case by watching the social workers and in Goldberg' by asking them what they were doing.

The variations in these two systems shows that there is no single set of facts or obvious categories of information to collect in specifying what social workers are doing and detailing their goals. There were differences in that one system collected data the other ignored; there were also variations in how it was categorised. Items which one team saw as similar and placed under one heading, the other considered significantly different and covered in two or more categories.

If we judge these data collection schemes in terms of their stated aim of facilitating evaluative research, we can see strengths and weaknesses in each. Their function is

to give a detailed account of the social work input which can then be judged by some measure of outcome. With Raynes' information, we can work out the comparative time and cost of different social work interventions. Goldberg gives us more idea of what the social workers were hoping to achieve and therefore allows a better evaluation of their intentions. Neither would be adequate in many areas of research. If, for instance, the research wanted to compare two methods of counselling, neither provides sufficient data to differentiate for example between behavioural and Rogerian therapies. Nor does either of them tell us of the ethnicity of the worker or client, information which is essential in investigating claims that racism adversely affects work with black clients.

No system however could meet all possible research needs. Although Raynes claims to provide a "sufficient" description of social work, this can only be adequate for some research purposes. To ask "what do social workers do?" may look a simple question but it is misleading. People who ask it do not want to be told all the minutiae of social workers' activities but only those details which are or are thought to be causally significant in their clients' responses. Saying what social workers are doing involves deciding which actions are relevant and how they are to be classified. And people differ in their views about what is causally significant. For some perhaps, it might be important to know the precise wording in what was said to the client, others might think the way it was said was more important.

The simple inductivist view of science mistakenly sees scientists as neutral observers, collecting data in an unbiassed manner. These two research projects in social work illustrate the variations in what observers notice when looking at the same area. They also show that the task of formulating social work methods and goals is far from simple.

CONCLUSION

At present, social workers rely mainly on practice wisdom which, because of its individual and private nature, cannot be evaluated in detail by the standard procedures of science. Critics of a scientific approach maintain that this state of affairs cannot be altered; practice wisdom, they say, cannot be articulated as scientific theories.

The argument for this view that I have examined in this chapter is the claim that scientific theories must be about observable behaviour whereas social workers' practice wisdom is largely concerned with understanding the client's subjective experience. The humanist writers in social work suggest there is a striking contrast between social work and scientific reasoning. On the one hand, social workers regard their clients as rational, purposive people and want to understand what is going on in their minds. Such understanding, they claim, is achieved by a creative, imaginative process, drawing on their personal experience, empathic skills, and intuitive wisdom. On the other hand, scientists are depicted as strict behaviourists who focus exclusively on behaviour, ignoring mental processes. For

these writers, adopting a scientific approach would entail not just a modification but a total transformation of social work.

Saying that a scientific approach to human behaviour must take the extreme form of behaviourism however is wrong. The view of scientific method which would endorse this claim is faulty, overlooking the creative element in science and the conjectural nature of theories. Adopting a scientific approach in social work would not involve social workers' giving up their interest in the hopes and feelings of their clients.

Moreover the account of scientific methods I presented does not look strikingly different from humanist methods at least in the area of developing theories. I have argued that there are similarities in the way a scientist theorises and how social workers reason about their clients as they build up "practice wisdom". Both are making a conjecture about the causal processes behind the phenomena they want to explain. Science however offers no mechanical process for formulating theories; the task of articulating practice wisdom, though possible, is not simple.

The crucial difference between scientists and social workers lies not in how they theorise but in how they subsequently test their conjectures. Science provides methods for testing theories, for deciding whether social workers' practice wisdom is wise or not.

CHAPTER FIVE

FREE WILL AND CAUSALITY

INTRODUCTION

People are generally thought to have free will: an ability to choose what to do and to initiate a course of action. Indeed, the distinction between actions and events mentioned earlier presupposes this assumption: a bodily movement is an action if the person intended it to happen; otherwise it is just an event such as the muscle tremors caused by Parkinson's disease. But some social workers claim the idea of freedom of action conflicts with the determinism of science and so creates another obstacle to a scientific approach in social work.

There is no particular difficulty in thinking of our bodies as part of a causally determined physical world; we can accept that our liver and kidneys function according to natural laws. Problems arise though when we consider our thoughts and intentional actions. We generally feel that we have some freedom in choosing how to act; we can deliberate and it is up to us to follow one path rather than another. But if all our actions are fully determined by antecedent conditions they are the only ones we could have taken. In this case, our claim to act freely seems problematic.

Many fieldworkers seem to accept both that people have

free will and that their behaviour is caused. The dominant humanist tradition considers intentional actions are free. Most fieldworkers however also accept some deterministic theories, as the discussion in the previous chapter about their use of theories reported; they look for the <u>causes</u> of child abuse, for example, or of juvenile delinquency.

The social work literature though presents a different picture, with many humanists arguing that the beliefs in free will and determinism are incompatible; the issue thus becomes another aspect of the humanist versus scientific debate.

Those who defend the belief in free will cite the incompatibility of determinism as another reason for rejecting a scientific approach or only allowing it a partial role. Free human actions, say Downie and Telfer, are "beyond the reach of complete scientific explanation" (1980,p.125). Determinism, it is also claimed, radically conflicts with the humanist view of people, implying that people are "puppets" (Downie and Telfer, 1980, p.129), "slaves" and "victims" (Perlman, 1965), and "plastic" (Howe, 1987, p.29). In addition, it is claimed that determinism conflicts with current views on ethics: "if we are not free then our belief in moral responsibility will require radical revision" (Downie and Telfer 1980, p.129). Another moral problem is that determinism is said to be at variance with the basic social work principle of "client self-determination", i.e. "the practical recognition of the right and need of clients to freedom in making their

choices and decisions in the casework process (Biestek, 1961, p.100). Acceptance of this principle, it is argued, commits social workers to a belief in free will and therefore to a rejection of determinism, e.g. Hollis (1964), Perlman (1965), Whittington, and Stalley (both in McDermott, 1975).

The other side of the dispute is mainly occupied by behaviourists who support a determinist view and maintain that our sense of free will is illusory. Human behaviour, says Sheldon is a phenomenon in the physical world and "must obey the same laws of cause and effect" (1982, p.26). Behaviourism therefore challenges views of human consciousness "which represent it as some sort of disconnected entity, impervious, when it chooses, to environmental influence" (Sheldon, 1982, p.27).

In fact the other main psychological approach in social work — psychoanalysis — also takes a deterministic view of human actions but many of its supporters seem to ignore this aspect. Hollis (1964) for example, a leading advocate of psychoanalytic social work, argues against determinism on the grounds that it is incompatible with free will while not acknowledging that it is a feature of the theory that she champions. Because of this, the art/science debate in social work is again in practice mainly conducted between humanists and behaviourists.

The free will/determinism debate is one of the classic problems of philosophy. There are three main positions on the issue: (a) libertarianism: we have free will in a sense that implies that our actions are not determined; (b) hard

determinism: all actions are determined and our familiar sense of freedom is illusory; and (c), a view which receives little support in social work writings but which I think offers the most convincing answer, compatibilism: our notions of freedom and determinism can be reconciled.

My aim in this chapter is to present the case for compatibilism and show that using scientific methods does not preclude people acting freely.

THE PHILOSOPHICAL DEBATES

The problem of free will and determinism has a long history. The belief that we are free agents seems to be challenged by any thesis which implies that our actions are pre-ordained.

The problem arose in ancient Greek culture which had the concept of "Moira" or "fate" - the idea that all our behaviour was the inexorable working our of our destiny. This seemed to leave no scope for people to shape their own histories, leading the Stoic philosophers to the gloomy conclusion that "each of us is assigned a role to play in the tragedy of life ... and there is nothing for us to do but say our prescribed lines as best we can" (quoted in Dennett, 1984, p.2).

Christian theologians have also been troubled by the problem. They argued that if God, being omniscient, knows everything that has or will ever happen, then all our future actions are already fixed; the results of our

apparently free deliberations are already known to God. Like some social workers today, they were worried about the moral implications of this. They believed that people could only be held morally responsible for their actions if they had free will but had difficulty reconciling this with their belief that God knew in advance what actions people would (freely) take, (see e.g. St. Augustine, reprinted in Berofsky, 1966, p.269).

Nowadays the success of the natural sciences in developing deterministic explanations is the major source of doubt over the existence of free will.

The determinist thesis, in brief, is that every event has a cause. Indeterminism is however now widely accepted at the subatomic level in quantum mechanics so the thesis is usually modified to apply only to macroscopic events. The thesis that all events are caused is not empirically refutable but its plausibility has been greatly increased by the progress scientists have made in discovering causal laws. As O'Connor argues:

> the evidence that all events have causes is simply the spectacular success of modern science. Science is based on the belief that natural events fall into causally ordered patterns, a belief that in the early stages of science was something of an assumption without a great deal of evidence to support it. But the assumption has been amply justified by the history of science. Where scientists have looked for causes, they have found them ... Determinism is both suggested and confirmed by the scientific picture of the world (1971, p.48).

Some have shared the dominant social work view that free will and determinism are inconsistent. The general form of their argument is set out by O'Connor:

102

(A) Every macroscopic physical event has a cause.
(B) All human actions are macroscopic physical events.
(C) Therefore: All human actions are caused.
(D) Any event that is determined could not have happened otherwise than it did.
(E) Therefore: No human action could have happened otherwise than it did (O'Connor, 1971, p.61).

It is then claimed that an action is free if and only if the agent could have done something else in exactly the same circumstances. Given premises A,B, and C above, this implies that, since no human action could have been different, no human action can be free. Therefore either determinism or our belief that we have free will is false. Some then accept determinism (hard determinists) while others keep their belief in free will (libertarians).

This stark choice has however been challenged by many philosophers who argue that the two concepts can be reconciled. This "compatibilist" view has a long history. David Hume (1739) provided an early and detailed version of it; other proponents have been T. Hobbes (1651, Chap.21), J.S. Mill (1867), P.H. Nowell-Smith (1967), A.J.Ayer (1976), and D.Dennett (1982).

The basis of the compatibilist view is a repudiation of the claim that an action is free if and only if the agent could have done something else in identical circumstances. We feel free, they suggest, when our own wishes and decisions influence what we do as opposed to the times when our movements are wholly caused by outside factors. An action is free insofar as at least some of the causes which determine it are the agent's own beliefs, desires and intentions. Free actions are not wholly

103

determined by events external to our thoughts. If our hand moves because we intend to wave goodbye, we are acting freely, whereas the sufferer from Parkinson's disease finds that his hand moves whether he wills it or not.

However this account leaves no · place for indeterminism, rejecting the claim that a free agent could have chosen either option in precisely the same situation. To the compatibilist any action is fully determined by the combination of external factors and our own desires and choices. For a free agent to choose A rather then B, some factor, perhaps a feeling or a wish, must be different to tip the balance and lead to a different action. An agent could not have done something else in <u>identical</u> circumstances.

A more detailed account of this position is provided in the following sections which critically examine the alternatives.

FREE WILL

Let us begin by looking at the arguments for free will. Libertarians claim that people are free in the sense that more than one option is possible and they can__choose which action to take. Downie and Telfer, expressing a libertarian view in social work, put the main point clearly, describing the type of freedom they believe exists as: "the possibility that the choice might be different, not merely IF the circumstances were also different, but different in the same circumstances" (1980, p.134).

Libertarians sometimes appeal to personal experience

to support their view. Perlman, for instance, (in McDermott, 1975, p.68) cites our sense of having a free choice as evidence for free will. When we are deliberating about what to do, we commonly have the feeling that we are free to choose and, even when we have fixed on one course, we feel that we "might have" chosen an alternative. Dr. Johnson offered a famous though poorly developed version of this argument: "Sir, we KNOW our will is free, and THERE'S an end on't" (in Boswell, 1740).

The value of our experience as evidence of freedom is however debatable. First, many critics have pointed out that our experience is fallible. O'Connor (1971, p.18) for instance comments that our feeling of being free is, sometimes at least, illusory. Someone acting under hypnosis, for instance, will follow the hypnotist's instructions believing that he is acting of his own free will but those who have watched the whole performance will be convinced that it is a case of post-hypnotic suggestion, a clear instance of not acting freely. The weight of this criticism has been questioned. Some libertarians point out that this scepticism applies to all our beliefs. Our knowledge of the physical world relies on our sensory experience which may at times be false but we still consider the evidence of our senses to be generally trustworthy. Cornman, defending the libertarian's appeal to experience of freedom, says:

even if we conclude that the evidence we have in all these cases does not give us knowledge, as the skeptic avers, we may still fairly maintain that the evidence makes it reasonable for us to accept the hypothesis in question (1987, p.106).

Another criticism of individual experience as evidence for free will is that although introspection may tell us we feel AS IF we could have chosen another course of action, we never actually experience taking any alternative. We take one option and, however convinced we are that we could have chosen another, we cannot check this belief by seeing if we take a different course on a future occasion. A key feature of the libertarian's account of free will is we could have done something else in exactly the same circumstances and two different occasions can never be identical in every respect. At the very least their timing is different.

Libertarians are criticised for failing to give a detailed account of "free" actions. They accept that our reasoning is a crucial element in exercising free will but to them it is not the full story. Our free actions, they claim, are influenced but not determined by our thoughts. Even after weighing up the pros and cons of the alternatives facing us, we have an element of freedom in deciding which action to take and the choice we finally make is not determined by our beliefs, wishes, and intentions. But this notion of an "undetermined" free act has proved hard to analyse clearly.

An undetermined choice implies, according to the libertarian, that even if all our reasoning had been the

same, we might at the end of our deliberations have made a different decision. Why then do we take one choice and not another? It seems from this account that we cannot have a REASON for it, so it looks as if our choices are only a matter of whim or chance: we "just happen" to come down on one side. But free, responsible actions are meant to be a matter of choice not chance.

Simple indeterminism — the claim that insofar as our actions are free they are not caused — is, on its own, inadequate. The indeterminism the libertarians want to claim is very different from that found in science. In quantum mechanics the undetermined movements of subatomic particles are a random matter of chance. There are times when our decisions are also like this. For example, we may find such compelling reasons for two options that we cannot make up our minds. We may finally resolve the debate by tossing a coin to decide which course to take, but this kind of random outcome is not what libertarians, or anyone else, mean by "exercising our free will." Indeed it is a case of abdicating our freedom, of saying "I find it hard to decide so I shall let chance decide for me." We would not hold a man responsible for an action if it were determined by the haphazard fall of a coin. In this example though, we might consider him responsible for deciding to settle the issue by tossing a coin since this decision was a free action. The man who freely decides to toss a coin, or to take any action, does not "just happen " to do so; he CHOOSES to do so. Taylor, a believer in free will, notes the difficulty:

behaviour that is mine must be behaviour that is within my control, but motions that occur from no causes are beyond the control of anyone (1983, p.45).

The libertarian, Taylor says, needs the concept of an agent who is in control of his actions and who is capable of initiating actions:

in the case of an action that is free, it must be such that it is caused by the agent who performs it, but such that no antecedent conditions were sufficient for his performing just that action (Taylor, 1983, p.48).

But efforts to define this concept of agency have met with criticism. The claim that a free agent could have done something else even though all the circumstances were identical implies that "I" am more than the sum total of my thoughts, feelings, memories, etc. There is an "I" who can stand back from my reasoning and make a free decision. "I" am responsible for my actions. But what kind of entity is this? It is hard to describe this agent who is separate from our thoughts, feelings, etc. Hume, writing in the eighteenth century, commented on its elusiveness:

for my part, when I enter most intimately into what I call MYSELF, I always stumble on some particular perception or other, of heat or cold, light or shade, love or hatred, pain or pleasure. I never can catch MYSELF at any time without a perception, and never can observe anything but the perception (Hume, 1739).

Nowell-Smith (1967, p.282) also finds fault with accounts of a self who is apart from our general character and wishes. Libertarians typically describe a free act as "self-determined" and then talk of the "self" as a subject who does the determining. Nowell-Smith points out that this differs from our general use of "self" compounds, such as self-adjusting, self-regulating, self-controlled, and self-governing. In these cases, we do not assume that

there is a part of the object or body called the "self" which adjusts, controls, etc. We may say that a central heating system is self-regulating but we do not picture it as having a self which does the regulating. We mean it is capable of monitoring the temperature and switching on or off as required without any outside interference. People are self-determining in this way to the compatibilist. Someone's free choices are self-determined because they are "determined by HIS motives and character, as opposed to forced on him by circumstances or other people" (Nowell-Smith, 1967, p.283.) The libertarian, rejecting this analysis of self is left, Nowell-Smith claims, with a vague idea of a self which "is neither an empirical object nor displayed in characteristic action."

If we accept determinism however we can avoid the libertarian's problems. The self is equated with the mind and the agent's final choice of action is fully determined by his deliberations and external factors. Moreover, while libertarians use our familiar sense of freedom as evidence for their case, compatibilists can also draw support from our ordinary views of behaviour. If we examine an example of what we would normally think of as exercising our free will, the compatibilist's account fits commonsense usage better than the libertarian one.

This can be illustrated by examining an instance of what is generally thought to be a free action. Imagine that someone has been offered a new job and is deciding whether or not to accept. He weighs up the pros and cons of the new opportunity. Perhaps they present a conflict

between his short- and long-term career goals, or between personal commitments and job ambitions. After deliberation, he comes to a decision and writes a letter accepting the post.

How has he exercised his free-will? The compatibilist would say that his decision was free because it was what he wanted, the decision was the outcome of his appraisal of the relative merits of the options in the light of his goals, values, etc. He would not have been free if, for instance, someone had forcibly made him write the letter of acceptance. This seems in keeping with our commonsense views where we expect people to have a reason for their choice. If they are exercising their free will, people do not "just happen" to drift in one direction rather than another. In everyday speech, we think people have reasons for their free actions and can generally explain why they reached a particular decision rather than one of its alternatives. This seems to fit the compatibilist account that choice is determined by our thinking and that some element of our thinking would have to be different for us to opt for another action.

In summary then, I have argued that libertarianism runs into difficulties in providing a detailed account of free actions and human agency. Compatibilism, I have claimed, avoids these problems but its account of human freedom is significantly different. The human agent is not, as the libertarians would claim, a "prime mover", able to initiate actions uninfluenced by preceding events but an element in a causal chain. How significant an element

though is the issue in examining the claim that determinism implies that our ordinary view of human agency is radically at fault.

HARD DETERMINISM

A common concern among social work critics of determinism is that it threatens our sense of being an agent "in charge" of what we do; it implies that we are merely "puppets", "slaves", or "plastic" people, to use expressions commonly encountered in the social work literature. These fears cannot be dismissed as just scare-mongering since many behaviourists, taking a "hard" determinist position, endorse them though they themselves do not find them upsetting.

Skinner, for instance, asserts that, in moving from the libertarian to the determinist perspective "the direction of the controlling relation is reversed: a person does not act upon the world, the world acts upon him" (1971, p.206). Scientific determinism, according to Skinner, does radically alter our sense of self: "the man thus portrayed is a stranger, and from the traditional point of view, he may not seem to be a man at all" (1971, p.195). He says of the free agent humanists believe we are:

> his abolition has long been overdue. Autonomous man
> is a device used to explain what we cannot explain in
> any other way. He has been constructed from our
> ignorance, and as our understanding increases, the

very stuff of which he is composed vanishes (1971, p.196).

Such views are common among behaviourists. Chien (1972, p.6) reviewing the behavioural movement reports that "the prevailing image among psychologists is that of Man as an impotent reactor ... He is implicitly viewed as a robot." Behaviourists claim that the "self" is causally insignificant: Sechenov (1935, p.334) maintains "the real cause of every human activity lies outside man"; Skinner says that psychology "must abolish the conception of the individual as a doer" (1947, p.40). They argue that all our mental processes are ultimately determined by environmental factors and therefore can be fully explained in terms of them; hence mental processes can be ignored. Zurriff reports that a core assumption of behaviourism is that "behaviour is a function of environmental independent variables only" (1990, p.179).

Before addressing the philosophical issues in this view of behaviour, it should be noted that the behaviourists' view is in fact based more on methodological needs rather than philosophical arguments; external factors are more easily studied than mental processes. Zurriff in a philosophical critique of behaviourism describes how "the concept of the agent stands in the way of the objective behavioural science conceived by behaviourism" (1990, p.178). First, they have a commitment to developing a scientific study of behaviour and this implies an acceptance of determinism: "for the behavioural program to succeed in establishing a science, lawfulness in behaviour

112

is necessary" (Zurriff, 1990, p.178). Secondly, they have a commitment to studying observable phenomena and the deliberations of the human agent are thus problematic. Skinner, explaining his exclusion of mental terms, says: "the objection to inner states is not that they do not exist, but that they are not relevant in a functional analysis" (1953, p.35). Zurriff sums up the behaviourists' rejection of the idea of the human agent: "lawfulness, objectivity, observability, and scientific explanation ... can be achieved only if, as a working assumption, agency is abandoned." (1990, p.178).

If, as I have argued in Chapter Four, the mind can be studied scientifically, behaviourists' methodological argument for rejecting the concept of a human agent is fundamentally weakened.

But what of the philosophical arguments for this view? The compatibilist argues that even if we are determined this does not threaten our sense of autonomy. Of course we are affected in many ways by the world around us but even libertarians acknowledge that. Behaviourists such as Skinner though undervalue the <u>interaction</u> we have with our surroundings. The world acts upon us but we in turn act upon the world and it is this which gives us some control over what happens to us. We are free insofar as some of the causes of our actions are our own volitions and preferences.

The main philosophical attack on compatibilism however is that this account of freedom is unsatisfactory. Critics claim that such a sense of being responsible for our

actions is illusory. Actions may be caused by "our" volitions but since, according to determinism, these are themselves determined by external factors the individual could not have chosen any alternative course of action. Taylor , rejecting compatibilism, asserts that "far from solving any problem, it only camouflages it"(1983, p.42).

The compatibilist's defence is that these critics underestimate the significance of the contribution human mental processes make to the causal sequence. Dennett (1984, Chapters 2 and 4) for instance accepts that it is an implication of determinism that all our thoughts and wishes are ultimately fully determined by external factors but he questions the significance of this point, offering an interesting, evolutionary explanation of our ability to be in charge of what we do. To describe something as the ultimate cause generally suggests that it was prior to other causes. If we take a long enough view of the causal history of a man's action, then in a sense, it was ultimately determined by external factors. Indeed, if we go far enough back, mankind did not exist and there was only the environment. But things have changed a lot since then. Mankind has travelled through the evolutionary process, acquiring bodies, brains, and reasoning skills. At one time in the past, it might have been true to describe people as wholly determined by their environment but, as they have acquired rationality and language, they have become increasingly capable not just of reacting but of interacting with the world around them. Therefore we are now, even at birth, capable of a complex interaction

with our environment. We are not completely self-made selves, but we can claim a significant share of the responsibility. The human agent is an element in a causal chain but such a major one that he has a clear identity.

Let us examine the libertarians' criticisms (and fears) of this account of human agency.

To begin with the most extreme view about our helplessness, fatalists, such as the Stoic philosophers, claim that determinism implies that everything is inevitable, the inexorable working out of causality. Butrym seems to be expressing this view when she claims that determinism is incompatible with social work's goal of trying to bring about change, implying that, if determinism were true, social workers would be unable to save their clients from their pre-destined fates. Without a belief in free will, she says:

> it would be difficult, if not impossible, to find a good enough rationale for the objectives of social work which are primarily concerned with change and thus are intrinsically antagonistic to a deterministic philosophy of life (Butrym, 1976, p.47).

Her conception of determinism seems to imply that, for instance, some children are doomed from birth to become delinquents while others are destined from the start to become depressed: the interventions of social workers cannot alter their fate.

Dennett takes issue with fatalists, arguing that a closer analysis of their reasoning shows that their fears are mistaken. Their argument is of the general form:

> if determinism is true, then (since all our acts will have sufficient causal conditions) no act of ours is avoidable (Dennett, 1984, p.102).

The fatalist thinks this implies that we are powerless, we cannot avoid our predestined future. What does it mean to say that something is unavoidable? "Avoid", Dennett points out, belongs to the family of verbs we use to describe human agency. We can avoid, prevent, bring about, ensure, etc. All of these verbs describe us "making a difference" to what happens and this, of course, is one of the aspects of having free will that libertarians are so anxious to protect. In what sense however do we "make a difference"? We cannot alter the future, replacing one event with another, because it has not happened yet. As Dennett says:

> the future consists, timelessly, of the sequence of events that will happen, whether determined to happen or not, and it makes no more sense to speak of avoiding these events than it does to speak of avoiding the events that have already happened (1984, p.124).

It is therefore a mistake to talk of avoiding a "real" future event, because if we avoided it, it did not happen and therefore is not a real future event. When we talk of making a difference to the course of events, we mean that we have altered what "would have happened" without our action. If we say, for example, that we prevented something, we mean that what we did led to a different outcome from the one we would have expected if we had done nothing. We do not change the actual future but our predicted future.

> All the verbs of "making a difference" involve a tacit comparison between the way the world was APPARENTLY going to go, and the way it turned out to go (Dennett, 1984, p.126.).

According to this view, in a determined world, when

116

human agents intervene, they do affect what occurs. So Butyrm is wrong in thinking that determinism implies that social workers cannot step in and improve the lot of people in distress.

A sense of coercion is common to the metaphors chosen by social work critics of determinism; they use emotional images of "slaves", "puppets", and "victims." But in what sense does determinism imply such duress?

O'Connor (1971) provides a possible source of this fear of being constrained. He suggests that a major cause of hostility to determinism arises from confusing causation and coercion and supposing that determined behaviour is coerced. But it is misleading to imagine a causal law as some kind of slavemaster, whipping us into line if we try to do anything on our own initiative. O'Connor argues that to suppose that causes "coerce" events is to confuse prescriptive with descriptive laws. The laws established by Parliament permit certain behaviour and forbid deviations under pain of punishment. With respect to these laws it is meaningful to talk of coercion, but not with the laws of nature. The planets are not compelled to follow the orbits assigned to them by relativity theory while secretly yearning to deviate along different paths.

In ordinary usage, we say that we are coerced when we are made to do things by external factors despite our wishes and, in such cases, our actions are not thought of as free. This distinction between forced and free action is preserved with determinism, as O'Connor indicates:

to say that my conduct is free is merely to say that

it is under my own control. And it is under my own control if it is guided by my own intentions, motives, and desires. But to say this is certainly not to say that my conduct is in any way UNCAUSED (1971, P.74).

Another possible source of the slave and victim imagery is the fear that, if our actions are determined then, in principle, they are predictable. This can create a fear that we might be controlled, not by causal laws, but by an intelligent being. And, from the metaphors chosen by social workers, the fear seems to be that this could be an evil rather than a loving being — an evil scientist (probably a behaviourist) who uses his knowledge of causal laws to control and manipulate us to his own ends. Dennett (1984) notes how common this nightmare is in the literature on free will. But, surprisingly, there is considerable agreement on the question of predictability between libertarians, compatibilists, AND determinists. Even the libertarians typically agree that some prediction is possible for they concede that human actions are often subject to regularities. From a knowledge of someone's general character, we can have a reasonably good idea of what they are likely to do in future. It would be very surprising, for example, if John Major voted for the Labour Party at the next general election.

The complexity of human behaviour however is generally thought to rule out precise prediction. Skinner (1974), a determinist, says such prediction is impossible, likening the complexity of human behaviour to that of a rainstorm. While physicists could make some predictions about the general pattern of its behaviour, they would be unable to predict with confidence the exact trajectories of each

droplet of water.

We tend to think of complexity being due to the sheer number of independent and unpredictable factors involved, as in the raindrops example, but Dennett stresses that our rationality also adds to the difficulty. Through evolution, we have become relatively intelligent and reflective beings, so that causes do not have a simple, consistent impact but are actively appraised by us:

> when we think of causation, we tend to think of nicely isolated laboratory cases of causation, where a single, repeatable, salient effect is achieved under controlled circumstances. Or we think of particularly clear cases of everyday causation: Hume's billiard balls, sparks causing explosions ... Thus when we think of someone CAUSED to believe this or that, we tend to imagine them being SHOVED willy-nilly into that state (Dennett, 1984, p.33).

Our reactions to incoming information, however, are different from a billiard ball's, being far more complicated since they involve so many factors. We can examine it rationally, judge its truth in the light of our past experiences, decide whether it suits our goals, etc. And to confound anyone trying to predict our reaction even further, we may, feeling stubborn, bored, or frivolous, act to alter our response from what was expected. We do not just receive information but process it in a highly personal way and it is this which makes the final use of the information "our choice".

In contrast to social workers' fears of being controlled by an omniscient scientist, the social sciences, at present, tend to produce only probable explanations but, as far as both libertarians and determinists can see, this is all that we can expect them to achieve, though we could

expect to increase this probability.

To summarise, libertarians and hard determinists both claim that if determinism is true then our familiar sense of being in control of our actions is an illusion; we are merely organisms reacting according to deterministic laws to events in the world around us. But this imagery substantially underestimates the complexity of human rationality and of our responses to events. The "self" which they allege disappears in a deterministic world is a major factor in producing our actions so that it is still correct to claim that "I" am responsible for what I do.

MORAL RESPONSIBILITY

Stalley (in McDermott, 1975) and Downie and Telfer (1980) both raise a frequently-made objection to determinism, claiming that it undermines our concept of morality. Even if this were true, it would not be grounds for saying that determinism is false: deeming an implication of a thesis unwelcome as opposed to establishing that it is untrue does not challenge its truth. However compatibilists would dispute the claim that determinism has this implication.

Libertarians argue that the assumption of free will is an essential element in our views on moral responsibility. It is argued that we blame people for bad behaviour only when we think that they could have avoided it. If for instance it is shown that an apparently criminal act was in fact due to a brain tumour destroying the person's normal

control over his conduct, we accept that the perpetrator was not "acting of his own free will" and refrain from blaming or punishing him. But, it is often argued, if all our actions were determined, we could never say that someone could have done other than he did and therefore we could not say he should have. Hence acceptance of determinism, the libertarian claims, implies rejection of our standard views of morality.

The compatibilist response is that this conclusion rests on a misunderstanding of what is meant by "could have done other" in a moral context. I have already discussed difficulties in the libertarian account of freedom; of differentiating an undetermined choice from a random chance, and of clarifying their concept of "self". Difficulties with these issues mean that their notion of "could have done other" is also far from clear.

Nowell-Smith (1967) gives a typical version of the compatibilist view. He argues that although the agent could not have done other than his actual deed in one sense, since it was determined, this is not the sense generally implicit in our moral language. He agrees that we assign moral responsibility only if we believe that the agent could have acted otherwise but an examination of how we judge whether he could have or not suggests that it is compatible with determinism.

Libertarians want to use "could have done other" in a categorical sense, that is to say without any conditions attached, but this, Nowell-Smith suggests, conflicts with common usage. Claims that I "could have" done something

are generally hypothetical not categorical, implying I would have if something else had also been the case. In other words, they refer to a tendency or capacity in the agent. Consider how we establish the truth of a claim that "I could have" done something I did not actually do. We look at similar instances and the person's past behaviour to judge whether the action someone claims he "could have" done is something that he is generally capable of doing. Nowell-Smith gives the example of a man reading Jane Austen's novel "Persuasion". If he tells us that although he chose "Persuasion", he could have read "Emma", this claim seems plausible. In fact it would be odd if someone had the necessary skills to read one of Jane Austen's books but not another, since they are in the same language and of comparable complexity. If the man reading "Persuasion" claims though that he "could have" read and understood "Werther" in the original despite his ignorance of German, then we would reject the claim that he "could have" read it.

Nowell-Smith then considers whether this analysis of "could have" is consistent with our ordinary use of moral terms and judgements. If we think that someone could NOT have done something different, we excuse him from moral responsibility, but how do we judge whether this is so? One criterion often employed is that we do not expect people to do things which are completely outside what humans have generally managed to do. We would not, to take an extreme example, say that someone "should have" done something which would have required him to move faster than

the speed of light. Another criterion is that external factors have not prevented someone from doing the right thing. "I could not keep my promise to meet you because I was held captive at the time" is a valid excuse which releases you from your moral obligations. Some internal factors are also accepted: neurological illness for instance can excuse "bad" behaviour.

In all these cases, the ordinary view is that the bad actions were not voluntary but in some way forced upon the agent, either in the literal sense of physical coercion or as the effects of causal laws outside his control. But if all human behaviour is determined, the libertarian argues, then all our actions are the effects of causal laws outside our control.

The answer, Nowell-Smith replies, lies in defining what behaviour we say we can control. We do not punish the man who was forced to do the wrong thing because "we know that it will do no good to punish him" (1967, p.296). The areas of behaviour which we censure are those where we know from experience that the agent can be influenced by our reaction and may consequently alter his behaviour or where others, seeing him punished, may alter their actions.

> A breach of a moral rule is only considered to be culpable when it is attributable to the agent's character, his vice or moral weakness" and "moral traits of character are just those traits that are known to be amenable to praise or blame" (Nowell-Smith, 1967, p.304).

On this analysis, we say that someone "could have done something else" and is therefore morally responsible for what he actually did do, when the "something else" is an action which, from previous experience, we think is

generally within his competence, and which he would have done if he had shown the right moral trait, if he had tried harder, had been able to try harder, had been less selfish, etc.

Dennett (1984) also criticises the libertarian claim that, in ordinary usage, if we say someone "could have done other" we mean it in the categorical sense that a free agent is able to take either option <u>in identical circumstances</u>. This, Dennett says, is not the sense in which, in everyday life, we ask if someone could have acted otherwise. Questions about freedom in this categorical sense raise metaphysical issues about the state of the universe but when we want to judge whether someone could have acted differently we do not indulge in philosophical debate: "we never show any interest in trying to answer the question we have presumably [according to the libertarians] just asked" (1984, p. 135). Rather, if we are interested in whether or not someone "could have done other" we check whether the alternative is something we could reasonably expect the person to be able to do, in the way outlined by compatibilists like Nowell-Smith. In deciding on moral responsibility, we need to distinguish between the actual (what the agent did), the possible (what he was capable of doing), and the impossible (what was beyond his ability.) If the only alternative act were impossible, then we would say that the person could not have done otherwise.

CLIENT SELF-DETERMINATION

The other moral difficulty social workers have perceived in determinism is its alleged incompatibility with the important social work principle of client self-determination. Acceptance of "the right and need of clients to freedom in making their choices and decisions" (Biestek, 1961, p.100), it is argued, commits social workers to a belief in free will and therefore to a rejection of determinism.

My argument so far shows that I believe this worry is unfounded but not only does the compatibilist account reconcile determinism and this principle but also it helps to clarify what the principle means.

A client is self-determining to the degree that his actions are free, that is to say that they are the result of his own wishes, deliberations, and choices. Moreover, this degree can be significantly affected by the way social workers offer help.

For example, an elderly lady faced with the possibility of being admitted to residential care can be easily swept along by well-meaning officials, a doctor or a social worker who is convinced that it is "in her best interests" to go into a home and who simply tells her that this is best for her. Alternatively, social workers can try to increase the client's ability to make the decision for herself. They can make sure that she has all the relevant information, in particular all the information that the social workers themselves used in assessing her needs, for instance of what life is like in a residential home, or the alternative services she could receive if she

stayed at home. They can encourage the client to reflect on the decision, to think out her priorities and consider how they would be satisfied in the alternative systems of care. Remembering her age and infirmity, they need to allow her reasonable time to make a choice. Above all, they can accept and act on her decision even when they think it is the wrong one. They may believe that she would be happier in a residential home but if the client values her independence and the familiarity of her home more than the physical comforts of residential care then they should accept her decision to stay at home.

The interpretation I have given of the principle of client self-determination is consistent with, in fact identical to, the interpretations found in social work textbooks, even in those written by people who claim that their interpretation conflicts with determinism. Hollis for instance seems to have a very similar view of the principle:

> what we really mean by this concept is that self-direction, the right to make his own choices, is a highly valued attribute of the individual. The more he can make his own decisions and direct his own life the better, and the less the caseworker tries to take over these responsibilities the better (Hollis, 1964).

Stalley's account is essentially the same:

> the function of the caseworker is not to direct the client but to assist his deliberation. This helps to ensure that the client acts on his own reflective desires rather than on impulse or in response to external pressures (in McDermott, 1975, p.115).

The main problem in relation to the principle of client self-determination, it seems to me, is not in

protecting it from determinists but in deciding how to implement it. While one can give a fairly straightforward account of what the principle means, putting it into practice is far from simple because, in many instances, it conflicts with other moral principles and legal duties so that social workers may have no alternative to overriding clients' wishes. The elderly lady described above is an example of an independent, responsible person but she is the exception rather than the rule in modern social work. Because of statutory responsibilities, much social work is either with people whose ability to make responsible decisions is limited to some degree, for example, children, the mentally ill, and the mentally handicapped, or with clients whose free actions are judged unacceptable in some way, for instance criminal offenders or families who are not caring for their children adequately. Respecting the client's right to self-determination while also taking into account his ability to make responsible decisions and the rights of others to protection from his actions is a complex moral calculation.

The principle of client self-determination is not, in practice, threatened by the thesis of determinism but by the realities of current social work responsibilities.

CONCLUSION

My purpose in this chapter was to challenge the widely-held view in social work that free will and determinism are incompatible since it is an element in many

social workers' hostility to science. Those who accept this incompatibility fall into two groups, libertarians and hard determinists. I have argued that the former, rejecting determinism, have difficulty in providing an adequate account of a free undetermined action or a free undetermined self while the latter group, accepting determinism, conclude that our commonsense view of ourselves is fundamentally wrong and must be radically altered. The view that free will and determinism can be reconciled seems to me to provide the least problematic solution. Its analysis of a free action differs from that proposed by the libertarian but, it has been argued, it is consistent with the way freedom is talked about in everyday life. Determinism worries many people because it seems to threaten our familiar and valued sense of being responsible decision-makers, in control of our actions and our destinies. It is only if you think of rational human beings as, in fact, very simple reactive organisms that these fears look plausible. Once you acknowledge the complexity of our thinking, determinism does not threaten the belief that "I" make decisions about what "I" should do.

The people who need social work help suffer from many social disadvantages. Their freedom of action is threatened from many directions, by poverty, illness, or prejudice, but not by social workers' acceptance of determinism.

CHAPTER SIX

THE LIMITS OF EMPATHY AND INTUITION

INTRODUCTION

I concluded, in Chapter Four, that social workers and scientists have much in common in the way they develop explanations. The crucial differences lie in how they then test their conjectures and decide on their plausibility or probability. In this chapter, I examine the means of evaluating their understanding that most social workers seem to rely on: their individual intuitive and empathic skills.

The concepts of empathy and intuition occur frequently in the social work literature; understanding clients is said to be achieved by using these personal skills. Although they appear together so often that they seem a single idea, the two concepts are significantly different.

Empathy refers to the ability to use our own experience to imaginatively "enter into" another's private, mental world. "Empathy puts one into the feelings and experiences of the other" (Goldstein, 1986, p,.68). It enables a social worker to "know the client's problem almost as if he were living it" (England, 1986, p.23). An

example of such empathy is drawing upon one's own experience of loss when working with a client who has suffered a bereavement in order to put oneself, in a sense in his position and imagine what mental experiences he is having. One can conjecture not only that the client is sad but also how this feels.

The other skill said to be so important in social work is intuition. Intuitive reasoning and intuitive judgement are frequently used but rarely defined terms in social work. Intuition seems to have two distinctly different meanings. First, it is used to refer to a direct insight, not gained by reasoning but in some other non-specified way. It is most commonly used in claiming to sense directly the mental experiences of clients. Goldstein, for example, uses it in this way when he claims that social workers have the capacity " for 'knowing' in internal ways the inner state of others at times without the benefit of specific clues" (Goldstein, 1973, p.66). Brandon also uses it in this sense when he talks of social workers' ability to have "a direct awareness of life, direct communication, direct awakening, seeing people as they really are" (Brandon, 1979, p.19).

This type of intuition is private and somewhat mysterious. Goldstein, for example, does not explain how social workers can know "without the benefit of specific clues". The main question about this type of intuition concerns its reliability. Is the worker seeing people as they "really" are, as Brandon maintains. The person who

has the intuition tends to feel confident that it is true, it is psychologically convincing. For others however its plausibility can only be judged by whether later events or behaviour support it, whether for instance a client said by intuition to be angry acts in a way which corroborates this claim.

In its second usage, intuition is taken to involve some form of reasoning but its distinctive feature is that such reasoning is implicit, not carried out in the conscious, explicit manner of science. It is this type of intuition which seems to play the main role in social work. When assessing a client and deciding how to intervene, social workers do deliberate. They may draw on the theories they have learned during their training or on the ideas acquired through experience but they do so in an unsystematic, piecemeal way where they may not be fully aware of what ideas have influenced their final decision. The acceptability of their conclusion is based on a personal judgement, an intuitive appraisal of its plausibility, whether it "makes sense" or "feels right" to them.

Let us turn now to a closer examination of these skills and the claims made about them.

EMPATHIC UNDERSTANDING

The term "understanding" is ambiguous in relation to human actions because of the ability to empathise. Sometimes it is used as in the natural sciences: we

"understand" something when we can explain why it happened. At other times, the claim to "understand" another person can mean that one empathises with them and thinks one knows what it feels like to be in their circumstances.

Many humanists argue that the goal of the social sciences is to understand other people in the sense of knowing how they feel. This, they maintain, renders the study of human actions methodologically different from the study of the natural world (Collingwood, 1946, is a famous advocate of this view).

Social work critics of science have an additional reason for supporting this argument: the humanist goal of empathically understanding people, they claim, is also the way to help them. They propose a therapeutic as well as a methodological argument for rejecting science. The experience of being empathically understood by a fellow human being, it is claimed, provides the supportive setting in which a person in trouble can reflect on his difficulties and find a solution. Empathy is seen as therapeutically essential, not just the only means at our disposal for understanding other people.

England (1986, p.24) asserts that "it is experiencing the empathic helper which is itself the principal therapy". Jordan also claims empathic understanding is necessary for therapeutic success:

> empathy implies that the helper "feels with" the person in trouble; that by imaginatively entering the other's situation, he engages his own emotions in such a way as to share the other's responses. I am suggesting that this is an essential part of helping (Jordan, 1979, p.20).

The most influential advocate of this view is probably the psychotherapist Carl Rogers whose "client-centred therapy" (1957 and 1959) has been very well received by social workers. Similar views have been expressed by the main social work opponents of science, e..g. England (1986), Goldstein (1984), Jordan (1979), Ragg (1977), and Wilkes (1981), all of whom share the belief that people have a great capacity for solving their own problems. Therapists can help by providing the right setting in which our natural drive towards growth and development can be fully realised; therapists do not need any special knowledge or scientific expertise in order to promote change.

Rogers, for instance, believes that we are all striving for "self-actualisation" and, given the right kind of supportive relationship, can explore our thoughts and feelings and work out new ways of resolving any difficulties we face:

> the individual has within himself the capacity and the tendency, latent if not evident, to move forward toward maturity. In a suitable psychological climate this tendency is released ... It is evident in the capacity of the individual to understand those aspects of himself which are causing him pain and dissatisfaction ... It shows itself in the tendency to reorganize his personality and his relationship to life in ways which are regarded as more mature. (Rogers, 1961, p.35).

After studying the types of "relationship" which promote improvement, Rogers concluded that there were four important factors: the therapist's empathy, unconditional positive regard, and genuineness, and the client's

134

recognition of these qualities in the therapist. The therapist variables, which have become known as the "core conditions" of counselling, were amplified as follows:

"Empathy" refers to the ability of the therapist to sense accurately the client's feelings and thoughts and to appreciate their significance. "To sense the client's private world as if it were your own, but without ever losing the "as if" quality - this is empathy" (Rogers, 1957, p.98).

In showing "unconditional positive regard", the therapist communicates a positive, non-judgemental, acceptance of the client's experience. He is valued as a person regardless of any evaluation of his behaviour. "To the extent that the therapist finds himself experiencing a warm acceptance of each aspect of the client's experience as being a part of that client, he is experiencing unconditional positive regard" (Rogers, 1957, p.98).

The therapist who is "genuine" expresses only the thoughts and feelings which he really has; he does not adopt a "professional" manner which disguises his real reaction to the client. "The therapist should be, within the confines of this relationship, a congruent, genuine, integrated person ... within the relationship he is freely and deeply himself, with his actual experience accurately represented by his awareness of himself" (Rogers, 1957, p.97).

While Rogers lists three core conditions, most other writers single out empathy in particular as the main

therapeutic element. Being empathically understood is "the principal therapy" (England, 1986, p.24) and "an essential part of helping" (Jordan, 1979, p.20).

Most writers who stress the importance of empathy claim that there is little or no place for theories from the social sciences in social work. Empathy, if accurate, gives the therapist an understanding of the client's view of his problems; social science theories however provide alternative accounts. To a behaviourist, for example, the client's feeling of fear is re-classified as a conditioned response. A psychoanalyst re-interprets the client's own version of his private experiences in the context of unconscious processes outside the client's direct awareness. Explanations which go beyond the client's own account are deemed unnecessary; instead it is claimed that the relationship is all-important. Rogers, for instance, claims that:

> no approach which relies upon knowledge, upon
> training, upon the acceptance of something that is
> taught, is of any use ... If I can provide a certain
> type of relationship, the other person will discover
> within himself the capacity to use that relationship
> for growth, and change and personal development will
> occur. (Rogers, 1961, p.32).

Goldstein also subscribes to the self-healing power of people and their ability to find their own solutions. Given the right therapeutic relationship, he maintains, people "are capable of redefining and resolving the obstacles that block the path toward a more rewarding and confirming existence" (Goldstein, 1984, p.5).

Ragg (1977) argues that scientific theories are not

just unnecessary but positively harmful because they reformulate the client's thoughts and experiences. They replace:

> the client's everyday description of his situation. The social caseworker understands the client in terms of a conceptual framework, the logical structure of which is quite foreign to that in which the client conceives of himself and his situation, (Ragg, 1977, p.77).

Such re-interpretations are damaging, Ragg argues, because they distract the client and therapist from the client's own way of thinking of his experience and it is within his framework, Ragg maintains, that the remedies are to be found. Clients, he says, must not be re-classified as "systems" or "sets of psychological forces" i.e. in terms alien to them. The social worker must stay firmly within clients' personal views of themselves and their circumstances, helping them to describe and reflect upon that experience so that they can possibly see it in a new and less troublesome way. The client, not the social sciences, is where to look for understanding:

> at the heart of treating people as people is the necessity of recognising them as the only source of knowledge about what they are trying to do (Ragg, 1977, p.60, emphasis added).

England also urges social workers to reject the expert role offered by their theoretical training in the social sciences. He stresses the similarity between ordinary, friendly helping and his view of helping in social work: it "becomes a matter of "common sense"....social workers "understand" others in the way that everyone understands the experience of others" (1986, p.33) and (p.38) "the help

of the social worker is not in significant ways distinct from the help that people receive informally."

Jordan (1979) concedes that social workers may use other methods or techniques but then says that they must be employed within an empathic relationship or "all this technique will seem like so much gimmickry" because "if the client feels that the worker is simply seeing him in terms of a pre-set theoretical framework ... he is unlikely to co-operate or benefit" (1979, p.129).

To summarise, these writers claim that the ability to empathise is the main skill in social work and an approach which re-defines the client's experience is wrong. Our empathic skill, they say, offers a source of understanding not available to scientists studying the natural world. Furthermore, empathy is the therapy; social workers need personal skills in making therapeutic relationships, not scientific theories which explain human behaviour in concepts different from the client's own account.

To this group of writers, social workers may claim expertise inasmuch as their ordinary empathic skills are particularly well developed. They can also achieve understanding not shared by the general population because they work with people in unusual or extreme circumstances. They can therefore have particular awareness of, for example, the experience of coping in extremely deprived circumstances, or of how parents feel when they learn that their child is severely handicapped. They should not however aim at expertise based on theories couched in

abstract and esoteric terms and tested according to the cannons of the natural sciences.

On analysis though, I shall argue, this account of social work makes it a very limited service. If empathy is indeed the foundation skill then the skills of workers are severely restricted, first in terms of the scope of their understanding and, secondly,in terms of the effectiveness of their help.

THE SCOPE OF EMPATHY

First, let us consider to what extent social workers can empathise with their clients. England claims that one person's understanding of another is only possible when we are able to make a "link" between the other's experience and our own:

> he (the social worker) only knows the character of his client's meaning because he himself knows, in general, what it is to experience such mental or emotional states and can sensitively extrapolate from them (England, 1986, p.28).

But the people social workers try to help often have extreme or unusual experiences. Some clients report sensations which find no echo in the life of the typical social worker — the psychotic experiences of someone with schizophrenia for instance. Others may describe experiences which resonate with our own to only a limited degree. Suppose we wish to understand a mother who has assaulted her child. We may be able to empathise with the anger and frustration she was feeling at the time. Social

workers who are also parents may find this particularly easy. But can they then empathise with the experience of venting that rage on a child by physically attacking him? Their own experience would normally be restricted to feeling that fury and controlling it. Indeed, this example illustrates a pervasive feature of social work: people often become clients because they are out of the ordinary; they are the parents who have lost control, or the teenagers who have given into the temptations of crime. Relying exclusively on experience shared by clients and social workers can only provide a partial understanding.

Another limitation of empathy is that it applies only to conscious motivation. Thomas (1979, p.87) points out that empathy applies only to mental processes that an individual is aware of. It provides no means of understanding unconscious processes. Since a belief in the influence of the unconscious has been widely accepted by social workers, this restriction on understanding cannot be acceptable to many.

The scope of empathic understanding has also been criticised by philosophers. Nagel (1961) argues that reaching an empathic understanding does not satisfy our curiosity about someone's actions. In fact Nagel (1961, p.484) suggests that empathising does little to answer our questions. Returning to the example of the mother who injured her child, empathy may help to make her actions seem familiar and intelligible to us because, from our own experience, we have noticed that anger and frustration go

with at least a desire to hit out. But knowing that two experiences are often correlated does not in itself tell us why they tend to occur together; empathic understanding, Nagel contends, does not in itself explain anything. Suppose we could empathise with the mother, several questions would remain: why was she in that state; what were its causal antecedents; why could she not control her anger as most mothers do, is she likely to harm the child again, etc. And yet these seems crucial questions for social workers if they are to help her and to decide whether it is safe for the child to stay at home.

Even if an empathic relationship has the therapeutic power that these writers claim and, through it, the woman will eventually be helped to resolve her own difficulties, empathy is not enough to meet the statutory responsibilities of social workers. First, the social worker is involved because society condemns child abuse; there is no guarantee that the client will share these standards or that her own resolution of her problems will include better care for her child. Secondly, the social worker has to make a judgement about the safety of the child at present. All therapies take time and it may be too dangerous to leave the child at home while they are carried out.

Understanding of any kind is only a means to an end in social work — the primary goal is to help people. As the foundation for making decisions and acting, empathy is severely limited, leaving unanswered any questions about

causal processes outside the conscious knowledge of the client.

Can a case be made for saying that empathy is at least a necessary skill in social work?

At first sight this seems plausible because writers of all theoretical persuasions emphasise beginning a social work assessment by understanding the client's view of his difficulties. Butrym, for instance who recommends a psychodynamic approach, stresses:

> the importance of personalised helping within which both proper understanding can be gained and due attention given to the subjective experiences of those who have a problem (Butrym, 1979, p.89).

Hudson and Mcdonald, in their textbook on behavioural social work, also emphasise the point:

> contrary to the impressions of some critics of our orientation, behavioural social workers begin by listening carefully to the client....the client should be encouraged to give as much detail about it (her problem) as she is able (Hudson and Mcdonald, 1986, p.62).

But the apparent unanimity of authors is questionable because of the ambiguity of the term "understand" when applied to human actions, leaving it unclear whether all or only some writers are saying that <u>empathic</u> understanding is necessary.

England (1986, p.28) explicitly states that this understanding involves empathy; social workers can only understand if they have had similar experiences:

> he (the social worker) understands confusion not because he has experienced <u>this</u> confusion but because he has been confused; he understands loss, depression or love because of his own experience of loss, depression or love. This is a necessary condition of all human understanding.

It is more plausible however to claim the reverse:
that social workers can only empathise if they can
understand. Winch is a philosopher famous for arguing that
the social sciences need to understand people's own
perception of their actions, a point similar to the
consensus view in social work that helpers must begin by
understanding the client's view of the problem. However,
in analysing this understanding, Winch gives empathy a
secondary role.

The argument rests on the issue of how social workers
are to recognise that they have had a similar experience to
the client's, that their loss, although not identical, is
comparable to the inner sensations the client has. Winch
contends (1958, p.119) that learning the language is the
primary task. To make the comparison, we first have to be
able to describe experiences, to identify what the client
is experiencing before we can determine which, if any, of
our own experiences are comparable. Thus, to return to
England's example, to empathise with someone's loss, we
first need to understand the meaning of the word loss and
identify that this is what the client is feeling before we
can turn to our own experience and decide whether we have
had a similar sensation. Only then are we in a position to
empathise.

On this analysis of understanding other people,
empathy is not necessary nor indeed is it possible until we
already have some understanding. To understand a client,

social workers need to understand the language in which he reports his experience and this is possible even if they have not shared that experience. I can understand that a client is experiencing auditory hallucinations or feels that his thoughts are being controlled by some outside force without knowing how this feels.

Empathy then has very limited scope. If social workers could only understand and help clients with whom they can empathise, they would be able to provide only a narrow service but, I have argued, it is not an essential element in understanding the client.

The limitations of empathy become even more apparent when we consider the evidence on its effectiveness as a therapy.

THE POWER OF EMPATHIC THERAPY

Rogers claims that a relationship which contains his three core conditions is sufficient for effective helping. Empathy, non-judgemental warmth, and genuineness are, he says, the "necessary and sufficient conditions of therapeutic personality change" (Rogers, 1957). What evidence is there to support such claims about the power of empathy?

The main appeal from social work writers is to our own experience. "We can recognise this in our own experiences of seeking help" says Jordan (1979, p.21). "We know from our own experience that this is a necessary attribute of the helping person", claims England (1986, p.24). And,

indeed, I expect that most people can think of occasions when such empathic understanding did seem to make it easier to cope with a problem or think of a way of tackling it. The lesser claim that the therapeutic relationship is important though not sufficient receives widespread support.

Both behaviourists and psychoanalysts, for example, have accepted that the quality of the relationship influences the effectiveness of their particular techniques. Freud (1912) held that, for psychotherapy to work effectively, the therapist needed to form a "working alliance" with patients. This alliance, he thought, was based on patients recognising that the therapist was understanding and well disposed towards them. If patients experienced warm and positive feelings from the therapist, Freud suggested, they were more likely to respond well whatever therapy was being used. Similar views are found in behavioural textbooks where a positive relationship is seen as important in helping communication and motivating the patient in therapy (e.g. Hawton et al, 1989, p.5).

The claim that empathic understanding is <u>sufficient</u> however is far more controversial. An appeal to our own experience is less successful here when one considers the severity and complexity of clients' problems. But, unusually for a group who are mainly opposed to scientific research in social work, these writers appeal to research evidence to support their claims.

Rogers himself differs from most of his supporters in

believing that empirical research is essential for developing effective therapeutic services and is therefore to be encouraged. Most of the research has studied his hypothesis that the relationship is therapeutically sufficient. If he is right, this has the implications that the long training required by other therapeutic approaches is unnecessary and that their often bitter rivalries are irrelevant in terms of outcome. This contentious claim has been investigated in several studies.

The first requirement was to find a way of measuring the three "core conditions", namely empathy, unconditional positive regard, and genuineness. Truax and Carkhuff (1967) developed rating scales which independent assessors could be trained to use in a consistent way. These are the scales most widely used.

The research studied people receiving psychotherapy, not social work clients. Most research has been not on client centred therapy itself but on the associated claim that, whatever method being used, therapists who score highly on the core conditions would be more successful than their low-scoring colleagues.

The first reports on the research indicated that the hypothesis was supported. In a review of 14 studies, Truax and Mitchell (1971) concluded that Rogers' hypothesis had been strongly corroborated. Moreover, therapists who scored badly on the core conditions seemed to harm their patients, having a higher rate of deterioration. These results seemed to offer strong support to the claim that

146

the core conditions were of major therapeutic importance.

These positive conclusions are often cited by social workers as evidence for their preferred way of working (e.g. Goldstein (1973, p.67), England, (1986, p.24), and Howe (1987, p.5)). But they do not take account of the fact that these apparent confirmations were quickly disputed and serious doubt cast on their reliability.

On closer analysis, it was argued, the studies did not provide the strong corroboration that Truax and Mitchell had claimed. As Garfield and Bergin (1978, p.245) tactfully expressed it, Truax and Mitchell "de-emphasized findings that did not coincide with those predicted by Rogers' hypothesis." For example, Rogers claims that all 3 conditions are necessary but the studies did not bear this out. Indeed in some cases, low levels of one condition were associated with improved outcome. A study of 40 hospitalised people with schizophrenia found that those whose therapist showed low levels of genuineness improved more than those who exhibited high levels (Truax, Carkhuff, and Kodman, 1965).

In 1973, Mitchell published a reanalysis of the 14 studies concluding that the evidence in favour of Rogers' hypothesis was much weaker than he had first judged. His original claim that Rogers' theory was strongly supported by the results is unwarranted in the light of the following statistics. In measuring the correlation between each core condition and patient outcome, he reported that of 109 correlations between empathy and outcome, only 24

correlations were significantly positive, of 108
correlations between warmth and success, 34 were
significantly positive, and, in relation to genuineness, 26
out of 88 correlations were found to be positive. Moreover
in 6 cases, genuineness was found to be negatively
correlated with success.

Later research continued to produce conflicting
results. Some studies provided some support for Rogers'
theory; others found a correlation between only one of the
core conditions and therapeutic success; some found no
correlation. In a further review in 1977, Mitchell reaches
an even more subdued conclusion:

> the recent evidence, although equivocal, does seem to
> suggest that empathy, warmth, and genuineness are
> related in some way to client change but that their
> potency and generalizability are not as great as once
> thought. (Mitchell, 1977, p.481.)

Research interest in Roger' hypotheses has waned as
the evidence seems to be against them. Some conclusions
though are generally agreed to have been demonstrated by
the evidence. The strong claim that a relationship
containing the three core conditions is <u>sufficient</u> for
therapeutic success is disconfirmed by the evidence. Even
the claim that they are a <u>necessary</u> condition is not
corroborated but it does seem plausible to claim that their
presence may increase the chances of improvement, whatever
the theoretical orientation of the therapist.

In view of this evidence, one can conclude that social
workers' empathic skill may be valuable in increasing their

therapeutic effectiveness but, on its own, it offers a very limited way of helping people.

INTUITION

Although many humanist writers in social work stress the central importance of empathic understanding, it does not carry such weight in practice. As my review of current social work methods showed, most fieldworkers do more than empathise with clients' experiences; they try to explain the problems in terms of factors outside the conscious awareness of clients. But, for the most part, their reasoning is intuitive; their "practice wisdom" is rarely explicitly stated. Curnock and Hardiker's (1979) analysis of social work assessments and social enquiry reports found that social workers generally report the client's point of view but go beyond it in their explanations. Social science theories, although not used in an explicit, systematic way, are influential as evidenced by reports in which social forces or unconscious processes are cited as significant causes of the client's current plight.

In the discussion of training, it was noted that students make a personal choice of which elements of their training to accept. This freedom continues in fieldwork. Individual workers are in general responsible for judging the accuracy of their assessments of clients, deciding how to help, and evaluating their efforts.

Most social workers approve of the present state of affairs. They consider that social work should be based on

the personal, subjective form of understanding contained in practice wisdom. "It feels right," "it makes sense to me," "it works for me" are the type of comments fieldworkers use to describe their reasons for accepting an explanation.

This individual approach has been criticised on several counts. For instance, it offers no way of building up a public knowledge base in social work and enabling one social worker's insights to be shared with others. But the most serious criticisms concern its reliability and the quality of the social work service it produces. Is the practice wisdom of fieldworkers really wise or only the embodiment of their personal values, prejudices, and misguided beliefs?

The poor reliability of individual social work judgement was demonstrated in Chapter Two in examining the results of evaluative studies mainly carried out in the U.S.A. Social workers in the 1950s and 60s had used and refined methods of working which they confidently believed were very effective. Their personal evaluations of their work were very positive but controlled trials failed to corroborate their optimism.

More detailed information about the defects in the current style of working can be obtained from the public inquiries into child abuse tragedies. Before looking at these, some points need to be made about their significance.

First, social workers do not carry all the responsibility for protecting children nor are they solely

to blame for these tragedies. Several other professions are involved and the most important decisions are made by the courts. However, as Blom-Cooper commented in the Beckford Report, of which he was Chairman, (1985, p.14), social workers are very influential: they make assessments and recommendations and "research suggests that such recommendations are likely to be acted upon".

Secondly, no-one should expect social workers to be infallible; whether or not they use scientific methods, there will be occasions when the decision which looks best on the available evidence turns out to be wrong. However, these inquiries were held because it was thought that social workers, and others involved, made unreasonable decisions given the evidence that was available and that they should have considered. The Beckford Report (1985, p.287) concludes that Jasmine Beckford's death was "both a predictable and a preventible homicide". The Carlile inquiry reached a similar judgement: "we conclude that Kimberley Carlile's death was avoidable through the intervention of welfare agencies" (1987, p.216).

An examination of the inquiry reports, I shall argue, reveals that the recurrent criticisms of fieldworkers made in them can be linked to the dominant non-scientific approach; the mistakes and oversights are not instances of unusually bad practice but of unusually tragic consequences flowing from the standard style of working.

For example, the difficulty fieldworkers generally have in being explicit about their work caused problems at

all stages. A D.H.S.S. (1982) review of the 19 child abuse inquiry reports published between 1973 and 1981 found several common criticisms relating to this. Fieldworkers were criticised because of the frequent absence of sufficiently comprehensive written assessments (1982, p.39), making it difficult for others to know on what they were basing their subsequent work, or to check the accuracy of their judgements. The lack of clear plans and goals was also a recurrent criticism leading to problems in supervising and evaluating their interventions and to difficulties in co-operating with the many other professions involved with the families. The Beckford Report (1985) also found fault on this issue, particularly commenting on the failure of the social worker involved to formulate her goals. If she had done so, the report considered, her seniors might have been able to see that she was focusing entirely on the parents' welfare and that she did not have the goal of protecting Jasmine.

Testing their intuitive judgements was another area where social workers were criticised by the inquiry reports. The standard scientific concerns for the range of evidence and its reliability were overlooked, producing judgements which were wrong and which would have been challenged by easily available evidence. Fieldworkers placed undue reliance on the judgements they reached in interviews with the families without subjecting them to further test.

The 1987 inquiry into the death of Kimberley Carlile

provides a clear example of this fault. The family had recently moved into Greenwich and the Social Services Department had been informed by the previous local authority that there were concerns about the children's welfare. In visits to the family, social workers had not been allowed to see Kimberley. After receiving allegations of child abuse from neighbours, the Team Manager, Mr. Ruddock, wrote to the parents stressing the need for the children to be seen and examined. The whole family then came, unexpectedly, to the social services department where Mr. Ruddock interviewed them. As a result of this interview, Mr. Ruddock's concerns, though not removed entirely, were reduced to the extent that he did not call a case conference or treat further investigation and intervention as urgent. He told the inquiry of his assessment of the family's behaviour:

> it was almost an archetype for a happy family scene
> ... I therefore could not have been more reassured by
> the family dynamics than I was by this overall display
> on this occasion" (Carlile, 1987, p.111).

Three months later, without having been seen again by a social worker, Kimberley was killed by her stepfather. The medical examination revealed that she had been tortured and starved for several weeks.

The social worker acknowledged how wrong his assessment of the family had been, telling the inquiry:

> the huge disparity between these very powerful and
> compelling presentations of positive behaviour and
> what we now know to have been the underlying reality
> is difficult to explain or analyse, and my experience
> here may be a useful lesson to others faced with this
> type of problem (Carlile, 1987, p.112).

But the inaccuracy of his judgement is not only apparent with hindsight; there was plenty of evidence at the time to undermine such an optimistic assessment. If the social worker had checked his judgement by looking for a wider range of evidence, he would have learned several worrying details: the fact that this was a newly formed family, the mother had been in prison, the children in foster homes outside London, the stepfather was a new boyfriend, all previous ones having been violent. In these circumstances, the inquiry report commented, even the most mature people would have difficulty in establishing a happy family so quickly. Moreover, the parents had failed to take Kimberley to medical appointments and refused the offer of a nursery place for her; keeping the child out of public view is a known danger sign. A medical examination would have found signs of the ill-treatment Kimberley was receiving at this time. A less complacent attitude to the impression the family made in one interview would have led Mr. Ruddock to make further inquiries and he would have quickly found evidence challenging that first favourable impression.

Besides being satisfied by a narrow range of evidence, social workers were criticised for failing to judge the reliability of the evidence. Bias is a major concern in scientific research and, in child abuse, two common sources of bias are the family and social workers themselves. Parents who are abusing their children have strong motives for concealing the truth from social workers. And yet

inquiries have found social workers trusting parents to an excessive degree. The Beckford Report (1985, p.116) criticises the social worker on this score:

> this expression of trust in what she was told by Beverley Lorrington (the child's mother) both about herself and the children permeated every aspect of Ms. Wahlstrom's work. She was, fatally, much too willing to believe everything "her clients" (the Beckford parents) told her.

The D.H.S.S. (1982, p.36) review of 19 inquiry reports offers several other examples where social workers showed undue confidence in information from families to the extent that they failed to check it by considering other evidence that was available, and so produced quite erroneous assessments.

Social workers themselves are also a source of bias; their emotional reactions to clients and their hopes that their efforts are being successful can significantly alter their judgements. In evidence to the Beckford Inquiry, this point was made. Professor Greenland commented that "the loss of objectivity is a common factor in the management of high risk cases" (Beckford, 1985, p.217). It was apparent in the care of Jasmine Beckford: "as soon as the social workers thought they saw the first signs of improved conduct on the part of Morris Beckford and Beverley Lorrington, an overweening optimism took hold" (Beckford, 1985, p.127). The Malcolm Page Report (1981, 3.63) also criticised the social workers for not noticing evidence of failure:

there were strong indications that the treatment plan
was failing ... the signs were there ... to read but
they were not interpreted and did not lead to a
critical examination of the treatment plan and of the
options available.

The widespread indifference among social workers to
empirical research was also apparent in these inquiries.
Failure to know or use research on the risk factors of
child abuse was noted in several reports, adversely
affecting their outcome. Social workers failed to
recognise the significance of evidence which research had
shown to be associated with child abuse. The repeated minor
injuries of Maria Mehmedagi, associated with poor
development and a poor parent/child relationship, should
have alerted her social worker to the strong possibility
that she was being abused (D.H.S.S., 1982, p.29).

The Beckford Report (1985) criticised both the
fieldworker and her senior for not knowing the research
literature on child abuse. For example, Jasmine's weight
chart provided a textbook example of the association
between children's growth and their exposure to persistent
abuse; she was underweight while cared for by her parents,
growing towards the normal range while in foster care, and
returning again to an abnormally low weight when returned
to her parents. Her social workers, not appreciating the
significance of her weight, thought that she was being
adequately cared for.

The criticisms of social work practice that are so
clearly highlighted in the child abuse cases but which are
also more generally applicable are not directed at

intuitive reasoning tout court. I am not suggesting that social workers should stop making intuitive judgements. The criticisms are of the way these judgements are used. Social workers show too much confidence in their immediate, intuitive appraisals based often only on personal interviews. They fail to treat them as tentative hypotheses which need to be formulated clearly and then subjected to testing.

This is a lesson that has been learned to some extent in one area: the accusation that they may be racist and that racist prejudices may be distorting work with black clients has caused social workers to feel doubtful about their intuitive knowledge and to subject it to critical appraisal. Few social workers consciously hold racist views but, it is claimed, their intuitive reasoning can be distorted by false beliefs about other cultures. Their reasoning draws on the background knowledge acquired through their life but they have mainly been brought up in Britain, a pre-dominantly white society with an imperial history and a strong tradition of feeling superior to black people. Their "folk psychology" therefore will probably contain many assumptions which reflect these racist prejudices.

To support this claim, critics cite statistics about the treatment of black people by the social services. Black offenders have been found to be under-represented among people on Probation Orders but over-represented in the prison population (Whitehouse, 1978), leading the

author to question what assumptions were being made by the Probation Officers who wrote the relevant social enquiry reports. Others have noted the unusually large number of black children who are taken into care (Cheetham, 1982, 17), suggesting that social work assessments may be distorted by racist judgements of the inadequacy of black parents.

On the issue of racism at least many social workers have been led to question the accuracy of their immediate intuition. The evidence of the statistical data suggests that the accusations of racism may have some truth but this is very unsettling and difficult to deal with for social workers who place such high value on their implicit wisdom. It implies that their folk psychology, instead of being a reservoir of sound commonsense wisdom, is tainted by false beliefs and prejudices. However the evidence I have presented here shows that it is not only in relation to racism that their intuitive reasoning can be faulty.

SOCIAL WORK AS ART

Among humanist writers, only England (1986) gives much attention to the question of the reliability of empathy and intuition. He shares the naturalists' concerns about the effectiveness current social work methods, saying that social work:

> should try harder to be precise; social workers have
> not developed any adequate tradition of intellectual
> scrutiny and criticism, and their thinking — in the
> job and in writing — is often lazy (England, 1986,
> p.6).

Again like the advocates of science, England argues that the first step in rectifying this fault is to encourage social workers to make their reasoning explicit so that it is open to criticism and evaluation by others. He would like to see them writing up detailed case studies explaining the understanding they reached and why.

But for England scientific methods have no role in appraising these case studies. Social work training, he claims, must teach students: "why scientific credentials are both impossible and inappropriate for their [social workers'] task" (England, 1986, p.132). Instead, he maintains, social workers should look to the arts not the sciences for their exemplar; literary criticism rather than experimental research is the model for evaluating social work. In the way that literary critics evaluate the coherence and plausibility of a novel, fellow social workers can examine case studies and judge the adequacy of fieldworkers' understanding of their clients. Drawing on their own experience, other social workers may notice biasses or omissions in the intuitive reasoning.

England's proposals are not new. Such peer review is valued by most social workers to a degree, as is evidenced by the established practices of supervision and case discussions. It has also been respected in publications. For instance, the Almoner, a journal for hospital social workers, in the 1940s and 50s carried a regular column for social workers to send accounts of their work and, in subsequent weeks, others sent in critical comments.

But why does England regard the only useful forms of criticism and evaluation to be the opinions of other social workers, ruling out so emphatically the standard testing procedures of science? His claim appears to have some plausibility at first glance because he presents the reader with a choice between behaviourism as the scientific approach and empathic understanding as the way social workers understand the feelings and thoughts of clients. I argued earlier that equating science with behaviourism in the social realm is a fallacy. And England shows awareness and indeed acceptance of this conclusion. He proceeds to argue that the social sciences resemble social work in that they need to study people's subjective experiences (England, 1986, p.78). However he does not then reexamine his reasons for rejecting science although his initial arguments are only directed against behaviourism. But once science is not equated with behaviourism the initial plausibility of his position is destroyed.

"INTELLIGIBILITY" NOT "TRUTH"

Another defence of social workers' rejection of scientific methods has recently been offered in the context of advocating psychoanalytic theories (e.g. Yellolly, 1980, and Pearson et al, 1988).

When American social workers first proposed psychoanalytic theories as an appropriate base for social work in the 1920s, they did so because they agreed with

Freud's own view that psychoanalysis was a science and moreover a successful one. In the light of the poor results of experimental evaluations of psychoanalysis, these assumptions now look dubious, creating a serious problem for analysts and their supporters. One response has been to argue that psychoanalysis is not a science and therefore should not be judged by the criteria against which it has fared so badly. Instead, it is suggested, psychoanalysis should be seen as a "hermeneutic" discipline in the humanist tradition, blending into our folk psychology. When analysts offer an interpretation of patients' experiences, their aim is not to "explain" them in a scientific sense but to present a new way of looking at them which analysands may find leads to a new, richer, and more satisfactory understanding. If analysis helps, it is not that it provides patients with a true understanding but a more comfortable one. According to this view, analytic theories should not be judged as true or false but as helpful or unhelpful. Yellolly, drawing on this new account of psychoanalysis, explains:

> from this point of view, psychoanalytic interpretations are neither true nor false; their justification lies entirely in their subjective significance for the patient, and whether for him they make sense, in that they present his experience to him in a new and revealing light (Yellolly, 1980, p.160).

This re-classification contradicts Freud's own view that his work was scientific but Habermas, a leading exponent, claims that Freud's judgement was excessively influenced by the dominant, positivist culture of his time,

leading him to claim mistakenly that his theories met the prevailing standards of scientific knowledge:

> because Freud was caught from the very beginning in a scientistic self-understanding, he succumbed to an objectivism that regresses immediately from the level of self-reflection to contemporary positivism in the manner of Mach [Ernest Mach, the philosopher] and that therefore takes on a particularly crude form. (Habermas, 1971, p.252).

The "hermeneutic" defence of psychoanalysis replaces the scientific concepts of truth and probability with that of "intelligibility". Interpretations are judged by whether they "make sense", whether they are "helpful". This approach will look very familiar to social workers who already use these criteria. But who is to make the judgement of helpfulness and how can we settle disputes if judgements about interpretations differ?

Habermas' solution is to assert that it is the analysand not the analyst who is the ultimate arbiter: "analytic insights possess validity for the analyst only after they have been accepted by the analysand himself" (Habermas, 1971, p.261).

This answer however poses a problem for social workers. Unlike analysts they are not usually urged to adopt analytic theories in order to provide therapy; analysis is a lengthy process and it is not generally considered feasible to provide it within the work demands of a social work agency. Analytic theories are mainly recommended as a source of explanation for social workers to use in understanding clients. Yellolly (1980, p.162) makes a typical claim:

> Freud's theories are a rich source of hypotheses in
> regard to the dynamics of behaviour and the
> development of personality.... As an <u>action</u> theory for
> social work, however, it has less value.

Pearson et al. also make this distinction:
"psychoanalytic understanding has a relevance to social
work practice which should not be confused with therapy"
(Pearson, 1988, p.45). Its value, they say, lies in
offering social workers an understanding of their clients'
experiences, when working with: "individuals and families
who are facing periods of painful transition in their
lives, through illness, handicapping conditions or old age;
both children and adults who are experiencing loss, either
through bereavement or separation; and families who are
suffering severe interpersonal conflict or disruption in
family life.

> It is precisely in these areas of difficulty that
> social workers need to draw upon the understanding
> which can be derived from psychoanalytic thinking
> (Pearson, 1988, p.44).

How though are social workers to judge the accuracy of
their understanding if they are not offering their
interpretations to the clients for their verdict? Unlike
the analysts in the hermeneutic school, social workers
cannot depend on the client's judgement about whether it is
helpful but must, it seems, rely on their own judgement.
Their conjectures about the experience of the client would
then be accepted if they were helpful to the social worker;
their justification would lie in their "subjective
significance" to the fieldworker rather than to the person
whose experience is being understood. Studies of current

practice, cited in the last chapter, suggest that this is the way that social workers have been using psychoanalytic theories and indeed all other theories anyway.

These recent advocates of Freudian theories seem to give most weight to social workers' feeling that they understand their clients; questions about the validity of that understanding or the benefits to clients are not raised or regarded as irrelevant. Indeed Pearson et al. concede that psychoanalysis has not been supported by the research evidence: "Whenever psychoanalytical theory and therapy has been put to the test of experimental scrutiny it tends not to fare very well" (1988, p.18). But these empirical results do not deter them from recommending analytic theories to social workers. They do so not because they think the theories are true or the therapy effective but because they think social workers will find them helpful in the sense that they enable workers to <u>feel</u> that they understand the complex world of the client.

Pearson quotes Freud (1927, p.253) approvingly for saying that his ambition in developing his theories was not so much "to help suffering humanity" but "to understand something of the riddles of the world in which we live." Leaving aside the point that, unlike Pearson, Freud wanted to "understand" in a scientific sense, his aim looks inappropriate for social work. For social workers surely the priorities must be the other way around; their professional duty is undoubtedly "to help suffering humanity."

Moreover, as these authors accept, social workers' understanding does not just lead to a personal sense of comprehension but influences the decisions they make about clients. They are not private psychotherapists; most are employees in statutory services with extensive duties and legal powers. If social workers use psychoanalytic theories – as, for example, Pearson recommends – to help them understand "families who are suffering interpersonal conflict", they may then use that understanding in their statutory role perhaps to make decisions about the welfare of the children. A Freudian rather than an Adlerian interpretation of a mother's behaviour may tip the balance in deciding whether a child returns home or stays with foster parents. The criterion of "subjective significance" looks inadequate in this context when an interpretation influences decisions and actions which can have such major impact on others.

Social workers' understanding has public consequences and therefore the accuracy of their understanding is also of public significance.

CONCLUSION

Social workers at present rely heavily on their empathic and intuitive skills to understand and help clients. Some claim that empathy should be the central skill of social work, essential both as a means of understanding and as a therapy. I have criticised this point of view for leading to a very impoverished account of

social work. Our ability to empathise with clients is markedly limited and research evidence indicates the weakness of empathy as the sole therapy.

Most social workers use more than empathy; they use "practice wisdom", an implicit set of ideas incorporating folk psychology, insights gained from experience, and elements of their theoretical training. This wisdom is used intuitively, that is, their reasoning is largely informal. The plausibility of explanations is judged by the individual, using criteria such as "it feels right" or "it makes sense to me." Since this practice wisdom is rarely made explicit, it is difficult to criticise and evaluate in detail. Its results though, the decisions and actions of social workers, can be evaluated; research and the public inquiries into child abuse tragedies demonstrate its poor reliability while the inquiry reports illustrate the tragic consequences of social workers' mistakes.

In this chapter, I have not challenged the use of intuitive reasoning in itself but rather the confidence social workers place in it. They rely heavily on their personal and immediate judgements of people, not recognising how limited and possibly biassed is the evidence on which they are reasoning, nor appreciating the need to check their judgements.

If social workers are to develop more reliable and effective ways of helping their clients, they must give up

their present contentment with empathy and intuition based
on a narrow and biassed range of evidence. They need to
make their intuitive wisdom explicit and subject it to
independent tests; they need to use scientific methods.

CHAPTER SEVEN

WHAT COUNTS AS EMPIRICAL EVIDENCE?

INTRODUCTION

Having criticised the non-scientific methods of
appraisal preferred by most social workers at present, let
me now consider their arguments against using the standard
means of testing employed in the natural sciences.

Scientists test theories against the empirical
evidence. They deduce observation statements from a theory
and then, through experiment, ascertain whether those
statements are true or false, whether the world conforms to
the picture predicted by the theory. In the debate about
using such methods of testing in the social sciences, the
dominant issue has been what counts as empirical evidence
in the social and psychological realms.

In social work, the long-standing debate has been
between humanists and behaviourists. Both sides have
accepted a positivist philosophy of science which leads to
the conclusion that only reports about behaviour not mental
phenomena constitute empirical evidence. Humanists have
then argued that such evidence does not provide an adequate
test of psychological theories. Satisfactory evidence,
they claim, must be psychological and, to collect this, we
need to use our empathic and intuitive skills, not just the
observation skills said to be used in the natural sciences.

Recently however this classic debate has been thrown into confusion by developments in the philosophy of science. There is widespread awareness that the positivist view of science has been discredited and that there is therefore a need to re-appraise the dispute in social work and the research practices which have been based on this philosophy.

There is however disagreement about the implications of these philosophical changes in social work. Two radically different philosophies have been proposed as the replacement for positivism, leading to conflicting views on the potential role of scientific methods in social work.

On the one hand, some challenge the status of science. They argue that if the positivist account of science is false then so is any claim that science is empirical, i.e. that theories can be tested against the independent evidence of our sense experience. If science is not empirical then, they argue, it is not superior to the humanist ways of reasoning. Adopting a relativist position, they claim that scientific method is no more valid than any other form of reasoning, or, perhaps more accurately, there are no means of adjudicating between them. Therefore, science should no longer be held up as a model to social workers and they should be allowed to continue uncriticised in their humanist tradition.

On the other hand, some, including myself, argue for a broader, empiricist view of scientific methods which bridges the gap between the two sides in the science dispute. This empiricist philosophy does not equate empirical with behavioural and so leads to a research

methodology which avoids the objections levelled by humanists at positivist research.

In this chapter I shall present and defend this position. I begin by surveying the philosophical discussions that have led in recent years to the social work re-assessment of positivist research. The first section examines the criticisms of social work research which, it is alleged, are due to researchers' acceptance of a positivist research methodology. The nature of this methodology is described before assessing the validity of the criticisms levelled at it. The following section looks at the criticism which has most weight — that the insistence on behavioural evidence to test psychological theories placed a severe constraint on research. I examine the ways researchers thought this requirement could be met before turning to criticisms of positivism itself. Despite general agreement that this philosophy of science is wrong, it will be instructive to examine its faults to understand the present controversies about what should replace it. The next section argues against the relativist's claim that there is no independent evidence with which to test theories. The final part proposes the revised, empiricist view on evidence and discusses its implications for social work research.

POSITIVISM AND SOCIAL WORK RESEARCH

While many of the new critics of social work research support a scientific approach, they question the way this has been attempted. Labelling the traditional research

methods "positivist", they condemn them for leading to research which has failed to address the important issues in social work. Weick (1987) claims that researchers have been "either solving the wrong problem or solving a problem not worth solving". Another critic, Heineman complains that:

> in a misguided attempt to be scientific, social work has adopted an outmoded, overly restrictive paradigm of research. Methodological rather than substantive requirements determine the subject matter to be studied. As a result, important questions and valuable data go unresearched (Heineman, 1981, p.515).

Research has been particularly restricted, it is alleged, by the demand that theories must be tested by quantifiable data. Researchers are accused of thinking that measurement is so important in science that they have allowed "measurability" to be the criterion for deciding what should be studied, and in the process have overlooked urgent social problems which cannot be dealt with in this way and so have ignored the needs of fieldworkers. Ruckdeschel and Farris (1982, p.275) complain that positivist researchers "have made a ritual of measurement and therefore cannot answer the questions that are relevant for effective practice." Heineman also believes positivism demands that variables be quantifiable and this has handicapped research:

> this requirement that concepts be definable by quantitative measurement operations has significantly restricted the scope and nature of the questions studied in current social work research (Heineman, 1981, p.373).

Positivism has also been blamed for leading researchers to concentrate on the results of social work intervention and giving little attention to the process of

helping. Smith complains that because of adherence to a postivist methodology the large-scale controlled trials carried out in social work suffered "from the serious limitation that it is impossible to tell what the outcomes were outcomes of" (1987, p.406).

Another criticism is that research has concentrated on overt behaviour, overlooking the individual's subjective world. Ruckdeschel and Farris complain that:

> a major shortcoming of many measures and concepts used by researchers is that they lack a relationship to the perspective of the actors (in this case, the clients) within the studied reality (1981, p.417).

Positivist research, they maintain, studies "a manipulable object" whereas they want to understand a "communicating and intentional subject" (1981, p.418), a point of view most fieldworkers share.

The persistent call is for "qualitative" rather than "quantitative" research. Allen-Meares and Lane (1990) summarise the contrasting assumptions of these two views of research. The quantitative model depicts "research as only empirical, objective data collection associated with experimental and quasi-experimental knowledge-building designs" (1990, p.452). The qualitative model, on the other hand, aims: "to understand and record sensitively the subjective perspectives and interpretive processes of individual subjects in situations" (1990, p.454). The latter reflects the typical fieldworker's concern with the subjective experiences of clients and, its supporters argue, can succeed where the quantitative model has failed to produce research on the issues that really matter to social workers.

To determine which of the criticisms now being

levelled at social work research can fairly be blamed on positivism, I shall first clarify the implications the positivist philosophy of science had for research methodology in the social sciences.

First a general point: empirical evidence in science is provided in the form of observation statements. Scientific reasoning, whether deductive or inductive, involves relationships between sets of statements, not between statements on the one hand and perceptual experiences on the other. A deterministic theory is tested by deducing observation statements from it. The role of observation is to enable the scientist to decide whether a particular observation statement is true or false, so testing the accuracy of the theory from which it has been inferred.

The positivist account of science made a sharp distinction between theoretical and observation terms. At the intuitive level, there is a clear difference between the statements "this child has blue eyes" and "this child is suffering from maternal deprivation." To describe the eyes as "blue" is to refer to a property the presence of which we can verify uncontroversially by direct observation. Moreover, if several of us look at the child, we can usually agree whether or not his eyes are blue. To a positivist, "blue" would be an observation term, meeting the definition provided by Carnap (1953, p.367), a leading positivist, that observation terms correspond to an observable quality whose presence or absence can be established by observers in a relatively short time and with a high degree of agreement.

Statements about "maternal deprivation", on the other hand, cannot be checked so easily. Understanding the meaning of the term requires some knowledge of the psychological theory in which it occurs; to decide whether or not a child is maternally deprived is a difficult process and one which, in the current state of development of the theory, is likely to be controversial. Terms like this were classified by the positivists as "theoretical".

The positivists maintained that because the truth or falsity of observation statements could be established by sense experience alone such statements provided the empirical evidence for testing theories.

In the social sciences, acceptance of the positivists' definition of "observable" had major consequences for what was judged to be empirical evidence. Psychological terms were held to refer to non-observable properties or processes in the mind and therefore they were classified as theoretical, not observational. Behavioural terms, however, were deemed observable and so they could provide the empirical base of the social sciences. What someone does, rather than what he believes, hopes or feels, provided, for the positivist, what Nagel (1961) called the "competent" evidence for testing social scientific theories.

From a common positivist base, two forms of behaviourism developed.

The strict type of behaviourism, associated with Watson and Skinner, as I discussed in Chapter 4, limits itself to behaviour in both its observations and its theories. The aim is to establish relationships between

observable behaviours and features of the environment without reference to mental phenomena. It is thus a radical departure from our commonsense way of explaining intentional behaviour. It has proved to be unacceptable to the majority of social workers and has been the main target of anti-scientific criticism.

The second form of behaviourism is not in such sharp contrast with our ordinary way of understanding human actions. The importance of the mind in explaining conduct is acknowledged but mental processes are only allowed to figure in theories; they are not considered observable. Tripodi (1983, p.82) proposing this view in a textbook on social work research makes this point: "observations cannot be used to describe directly the moods and attitudes of clients". All theories and assertions about the mind, it is claimed, need to be subjected to empirical test; observable behaviour provides the necessary empirical evidence.

Social work researchers have argued that this second form of behaviourism can encompass our ordinary view of human action and can be used to test fieldworkers' practice wisdom. The intuitive and empathic skills in understanding each other which social workers value so highly are allowed a place in that they can form the basis of theories but they are said to produce only speculative hypotheses which need to be tested by empirical (behavioural) evidence.

Opposition to this claim is widespread among social workers however. Some opposition stems from overlooking the differences between the two types of behaviourism. The humanist view discussed in Chapter 4, for instance, claimed

that science, as behaviourism, could not study mental phenomena, a criticism only pertinent to the first type.

Smith (1987, p.408) also levels at behaviourism in general a complaint only applicable to the narrow form. He asserts that research can only evaluate practice which is "trying to bring about behavioural change in clients" and so concludes that it has little relevance to the prevailing non-behaviourist styles of working. Behaviourists would reply that the aim of therapy is not necessarily behavioural; the positivist requirement is that there should be some behavioural evidence of whether it has been achieved.

The more recent criticisms, mainly expressed by supporters of a scientific approach, are directed at the second type of behaviourism; social workers have complained that the behavioural rule has placed a major, damaging constraint on research efforts.

To what extent can their complaints be blamed on positivism?

Consider first the claim that accepting positivist ideas on methodology led researchers to insist that all data should be measurable. To support their allegation that researchers have made "a ritual of measurement and therefore cannot answer the questions that are relevant for effective practice", Ruckdeschel and Farris (1982, p.275) cite Hudson's two axioms of social work practice as a typical example of the position they dislike: "if you cannot measure the client's problem, it does not exist" and "if you cannot measure the client's problem, you cannot treat it" (Hudson, 1978).

It is easy to find further evidence in social work research textbooks that researchers have indeed placed great emphasis on quantifiable variables. Tripodi (1983, p.44) for instance, expresses an orthodox view when he says that all terms must be operationally defined, and adds that this involves defining "dimensions of a concept so that they can be measured".

The practical consequences of this emphasis are not clear; the critics do not give examples of research to illustrate their claim that it has led to inadequate or irrelevant studies. But even if their allegation were true it seems unreasonable to blame it on positivist methodology itself. The only sense in which positivist philosophy can be said to insist on "measurability" is if it is taken in the very broad sense of meaning "observability". Theories are tested by empirical evidence in the form of observation statements. These can simply assert that a variable is present or absent; there is no requirement that they should be quantifiable. Although some researchers' views are inexplicit, others are quite clearly aware of this point. Wodarski (1981, p.3) in his textbook on research methods talks of the need to operationalise concepts in terms of <u>observables</u>. Reid and Smith (1989, p.195), in another research textbook for social workers, recognise the confusion the term "measurement" has caused and specify that they are using it in the broadest sense where it is synonymous with observation.

Neither can adopting a positivist methodology be held responsible for the fault Smith (1987, p.406) finds in social work research. He complains that researchers have

concentrated on outcome and have avoided the equally important task of studying the social work input. I argued in Chapter 4 that this criticism is, in part, justified, but positivism does not place any ban on such research. Moreover, though the difficulty of formulating practice methods may have daunted researchers, this is a point on which they have clearly changed. Fischer (1978), Wodarski (1981), and Thyer (1989) all share Smith's view that a more detailed study of the social work process is essential in future evaluative research.

Qualitative research is also not ruled out by positivism if the term refers to studies of people's subjective experience. But positivist methodology is responsible for stipulating that all psychological theories need to be tested against behavioural evidence and this requirement, although placing no absolute ban on the study of mental phenomena, does impose a severe constraint on it. Therefore social work criticisms of the limited nature of positivist research are, I think, partially justified. However, this positivist view on empirical evidence has been severely criticised so let us turn to a closer examination of it.

POSITIVISM AND BEHAVIOURAL EVIDENCE

Positivists would claim that the stipulation that all psychological variables are tested against behavioural evidence places no restriction on what can be studied scientifically. Their two main arguments have been, first, that all psychological language can be reduced to behavioural; and, secondly, that all psychological terms

can be operationally defined in terms of behavioural reports.

Consider the reducibility thesis first. This asserts that we can specify the criteria for using any psychological term in terms only of directly observable phenomena, e.g. environmental and behavioural factors. If this is so, then a behaviourist, in principle at least, can say anything a humanist might want to using only language about observable properties.

The thesis has a certain plausibility if we consider the way we learn to understand and use psychological language. A child learning the concept of anger, for example, does so by hearing the word being used by others in a variety of settings. He needs to work out the rules for when it is appropriate to describe someone as angry and, for this to be possible, he must be able to observe some differences between the contexts in which it is used and those where it is not. He will notice, for instance, that it is often associated with loud voices and critical comments and less often with laughter and smiles. Carnap expounded the thesis as follows:

> there cannot be a term in the psychological language, taken as an intersubjective language for mutual communication, which designates a kind of state or event without any behaviouristic symptom. Therefore there is a behaviouristic method of determination for any term of the psychological language. Hence every such term is reducible to those of the thing-language (Carnap, 1975, p.371).

We may illustrate this thesis with an attempt to reduce a psychological concept to behavioural terms. Tolman, a behavioural psychologist, tries to define "the rat expects food at location L" in non-psychological terms:

179

> when we assert that a rat expects food at location L, what we assert is that <u>if</u> (1) he is deprived of food, (2) he has been trained on path P, (3) he is now put on path P, (4) path P is now blocked, and (5) there are other paths which lead away from path P, one of which points directly to location L, <u>then</u> he will run down the path which points directly to location L (in Taylor, 1964, p.79).

This thesis has been criticised on the grounds that it is not practically feasible to reduce psychological terms to behavioural (e.g. Putnam, (1978), Scriven, in Krimmerman, (1975, Chapter 32), Krimmerman (1975, p.356) and Taylor (1964)). Carnap argued that behaviour and environment are important factors in our reasoning about other minds and learning to use psychological language. While this is not disputed, it is the complexity of our rules for using mental terms which is seen as the stumbling block.

We may illustrate this by returning to Tolman's attempted reduction of "expect". He has specified the kind of behaviour which would indicate that the rat expected to find food at L but his account is not completely equivalent to what we generally mean by "expect". If, for example, the rat went to the path which went to L but, before, he could run down it, someone picked him up, on Tolman's account he can no longer be described as expecting to find food. In ordinary usage though we would still say that he expected food but that the unexpected factor of being picked up had altered his behaviour. Tolman could also accommodate it by adding the proviso "and if he were not picked up" to his initial definition. The problem to critics of the reducibility thesis is that there are so many of these factors which we can allow for in ordinary

usage that the psychologist cannot realistically hope to spell them all out in advance. Putnam illustrates this point:

> it may be perfectly clear to everyone in a given situation that Jones is jealous of Smith's reputation. But one couldn't give anything like a 'scientific proof' that Jones is jealous of Smith's reputation. It isn't, for example, that 'Jones said blah-blah and people who say blah-blah are generally jealous'. Even if it is true that people who say blah-blah are generally jealous, one can easily envisage an indefinite number of situations in which someone might say blah-blah and not be jealous. So it is more like "people who say blah-blah are likely to be jealous unless special circumstances obtain and no special circumstances obtained in this instance"....One can't "verify" Jones is jealous in isolation: one would have to verify a huge "psychological theory" which covered all the "special circumstances". And this, of course, is implicit in our knowledge of people, and our ability to use psychological descriptions - not something we can state explicitly (Putnam, 1978, p71-2).

Therefore the reducibility thesis is wrong, philosophers have argued, and it is quite unrealistic to try to implement it. Behaviourists seem to endorse this point by their actions. Zurriff, in his overview of behaviourism, having explained why behaviourists deny that any reference to psychological states can be classed as observational, goes on to comment:

> nevertheless, in practice, most behaviourists use action language almost exclusively in describing behaviour. Action-neutral descriptions of behaviour are difficult to formulate, and action language is therefore used for convenience. There is a trade off between observational purity and usefulness (Zurriff, 1985, p.42).

In research, behaviourists have not generally followed cumbersome attempts like Tolman's to reduce psychological terms to behavioural but have used operational definitions. It is standard for research textbooks for social workers to

tell them that, at the start of their research, they need
to define their terms operationally (e.g. Tripodi, 1983,
p.44, and Reid and Smith, 1989, p.58).

Bridgeman (1927) provides an early and classic account
of this approach. The central idea is that the meaning of
every theoretical term must be specified by prescribing a
definite testing operation that provides a criterion for
its application. For example, 'intelligence' could be
operationally defined as the score obtained under specific
conditions on a specific I.Q. test. Operationalism, as
originally expounded by Bridgeman, holds that the meaning
of a term is fully and exclusively defined by its
operational definition:

> the concept of length is therefore fixed when the
> operations by which length is measured are fixed: that
> is, the concept of length involves <u>as much as and
> nothing more</u> than the set of operations by which
> length is determined. In general, <u>we   mean by any
> concept   nothing more than a set of operations</u>; the
> concept is synonymous with the corresponding set of
> operations (Bridgeman, 1927, p.5, my emphasis.)

The claim that terms are <u>defined</u> just in terms of how
they can be measured has been criticised. For instance, if
the length of an object is defined as the mark to which it
reaches when placed against a standard, rigid, measuring
stick, then it does not apply to the circumference of a
cylindrical object which cannot be measured in this way.
To give meaning to the concept of the length of a
circumference of a cylinder, one would need to specify a
new method of measuring it. It would seem that length of a
cylinder is a new concept. This is contrary to the
commonsense view that the concept of length is the same in
both instances but Bridgeman maintains that the second
measuring system does indeed define a new concept of

length. He argues that the common sense view considers that two procedures measure the same property if they are consistent in the sense that in areas where they are both applicable, they produce the same results. But the claim that they generally give the same results is an empirical generalisation and hence might subsequently be shown to be wrong. Because of this fallibility, Bridgeman considers it would not be safe to regard two procedures as operationally defining the same term.

Hempel (1965, p.123) holds that Bridgeman's advice to regard each measuring system as referring to a different concept is not followed in current scientific practice and he argues that if it were followed it would hinder scientific development. Scientists, Hempel claims, do consider that two procedures which meet the consistency requirement refer to the same term. Physical theory assumes one concept of length and many, more or less accurate, ways of measuring it. Operational criteria, Hempel suggests, are not treated as definitions in science. A definition is stipulative; it states what meaning you are assigning to a term. Scientists treat operational criteria as empirical; on this view they are fallible and subject to modification. It is more appropriate then to talk of observation or measurement theories rather than of definitions.

This distinction between operational definitions and observational theories is more than a linguistic quibble. Definitions can be stipulated by researchers — for example "by 'cohesive family' I mean a score above fifty on the

family cohesion questionnaire" - and, to a great extent, others have to accept their prescriptions. Tripodi (1983) for example presents operational definitions in this way to social workers, saying that "operational definitions are arbitrary, but they allow evaluators to translate concepts into variables, which are measurable dimensions of a concept." (1983, p.7).

When the testing procedures are seen as theories however they are not deemed "arbitrary" but open to critical appraisal; the researcher is not claiming to define the term but to have proposed a measure of it. In assessing the research study, others may question how satisfactory a measure it is. And social work criticisms of what they call "positivist" research can be more satisfactorily reformulated as a general complaint that wholly behavioural measures do not provide an adequate measure of psychological concepts. The criticisms of the reducibility thesis presented earlier lend support to their allegation.

However this is one of the areas in which positivism has been discredited. In this context the significant philosophical change is the revision of the positivist's sharp distinction between observation and theoretical terms. It is now generally agreed that there are no infallible observation statements to provide an absolutely secure base for science. As Smith, quoted earlier, has told social workers: "facts are not as hard as is often assumed". Those that the positivists thought had this character turn out, on closer analysis, to go beyond the evidence of our senses and to be fallible. Popper

provides a brief account of this point:

> we can utter no scientific statement that does not go
> far beyond what can be known with certainty "on the
> basis of immediate experience". Every description
> uses universal names (or symbols, or ideas); every
> statement has the character of a theory, of a
> hypothesis. The statement "here is a glass of water"
> cannot be verified by any observational experience.
> The reason is that the universals which appear in it
> cannot be correlated with any specific sense-
> experience. (An "immediate experience" is only once
> "immediately given"; it is unique.) By the word
> "glass", for example, we denote physical bodies which
> exhibit a certain law-like behaviour, and the same
> holds for the word "water" (Popper, 1959, Chap. 5).

It might be argued that observation and theoretical terms could be distinguished by saying that, unlike theoretical terms, an observation term can be ostensively defined. That is, we can point to objects in the world and say "that is blue", "those are eyes", but not "that child is maternally deprived." However, although there seems to be a significant difference here, it is a matter of degree rather than of kind. As Polanyi (1967) has shown, even learning an ostensively defined term requires making some unverifiable assumptions. If we take the concept of "blue" as an example, we can teach someone what blue means by pointing to various blue objects. However we are pointing at the objects as well as the blueness and the learner needs to conjecture what common property is being referred to. Telling him it is the colour we are picking out will only help if he already understands the concept of colour and to have learned this he will have needed to have made theoretical assumptions.

If all observation statements presuppose some theoretical assumptions, then they are not direct, infallible reports of experience. The underlying theory

can be rejected or modified and consequently the truth-value of the report can alter. The foundations of science are not as firm as positivists thought. Popper describes the situation graphically:

> the empirical basis of objective science has nothing "absolute" about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down into any natural or "given" base (Popper, 1959, p.111).

If the observation/theoretical dichotomy falls, what are the implications for science? Some distinction between observation and theoretical statements is essential to the empirical view of science. Observations are deemed to provide evidence for or against a theory because they are seen as in some way independent of that theory. At this point, relativists and empiricists part company. The former claim that so-called "empirical" evidence is so infused with theoretical assumptions that it provides no independent test of theories. Empiricists hold that there is still a significant difference between observation and theoretical statements though they should be seen as at different points on a continuum rather than belonging to completely separate classes of statement.

RELATIVISM

Some social workers argue that the defects of positivism are so great that they undermine the whole scientific enterprise. Hence, they maintain, any claims that science produces more reliable knowledge than other forms of reasoning are unfounded. This "relativist" view has been expressed by, among others, Rein and White (1981),

Heineman (1981), Paley (1987), Howe (1987), and Witkin and Gottschalk (1988).

Some then argue that social workers should stop worrying that their preferred personal style of working is in some way inferior to scientific reasoning. They should no longer "feel guilty about the subjective judgements for which they can offer no theoretical justification" (Paley, 1987, p.170).

But how do relativists move from the proposition that all observation statements are to some degree theory-laden to the view that there is no empirical base to science? The social work writers tell us little about the philosophical arguments which have influenced them. Smith (1987, p.403) tells us that, in the natural sciences "facts are rarely as 'hard' as is often assumed." Heineman (1987, p.378) gives a little more detail: "there can be no direct or untainted perceptions because all observation is shaped by theory." But how these statements are linked to research practices is not explained. A survey of their writing however shows that Kuhn is by far the most frequently cited philosopher (though usually only in a footnote). Feyerabend (who takes a similar but slightly more radical position to Kuhn) comes a distant second, while Paley (1987) briefly mentions Wittgenstein. Therefore, in discussing relativism, I shall concentrate on the relativist interpretation of Kuhn's philosophy.

Kuhn conceives of scientists working within a complex structure which he calls a "paradigm". Scientists who share a paradigm are working on the same theoretical system but share far more than this; they agree on what procedures

and techniques to use in applying the theory; they share metaphysical principles, values and attitudes. The paradigm is more than a theory; it is a shared world view. Kuhn means this more or less literally. Scientists 'see' the world through their paradigms and scientists in different paradigms, he claims, do not just explain facts differently but actually see them differently. Observation statements are not just permeated by some theory but by the specific theory for which they are supposed to provide evidence. Their meaning, Kuhn maintains, varies from one paradigm to another so there is no common, shared language in which the relative merits of paradigms can be judged. This extreme conclusion follows from his ideas about how the words in which we report our experiences get their meaning.

Positivists had two theories of meaning for their separate categories of observation and theoretical terms. "Observation terms" corresponded to observable properties in the world, but theoretical terms took their meaning from their position within the network of concepts in a theory. "The concepts of science are the knots in a network of systematic interrelationships in which laws and theoretical principles form the threads" (Hempel, 1966,p.94).

The problem now arises though that if observational terms are theory-laden, then they cannot get their meaning just from corresponding to the world. The relativists' solution is to extend the holistic theory of meaning to all terms. Although relativists are among the most vociferous critics of positivism, "the irony is", Newton-Smith points

out, "that Kuhn and Feyerabend have inherited from positivism the general holistic conception of the meaning of a term as given by the role of the term within a theory" (1981, p.155).

The relativists' view of scientific advance is not of a theory being tested against the hard data of reality and being corroborated. So-called empirical tests do not expose the theory to external evidence but check one part of the theory against another. The terms which are classed as observational are part of an all-embracing structure or paradigm in which the meaning of every one is connected with the others. Which terms are regarded as observational is more a question of the confidence scientists have in a particular aspect of the whole rather than a factor of their relationship to the world. Rorty defending this view stresses:

> the holistic point that words take their meanings from other words rather than by virtue of their representative character, and the corollary that vocabularies acquire their privileges from the men who use them rather than from their transparency to the real (Rorty, 1979, p.368).

With this holistic theory of meaning, judging the rival, epistemic value of paradigms becomes problematic. In order to judge competing ideas, it is of course necessary to be able to compare them. Scientists would say that they compare by weighing up the evidence in favour of each but the very existence of any shared level of evidence is challenged by a holistic theory of meaning. If we have a change of paradigm, then, since words get their meaning from their position within the paradigm, we have a change of meaning of <u>all</u> terms. Taking Newtonian and Einsteinian

189

physics as an example, as Newton-Smith

critically comments:

> Not only do they mean something different by "mass";
> they also mean something different by "the needle
> points at 4", "look it's turned green", and so on
> (Newton-Smith, 1981, p.12).

If we apply this idea to theories familiar to social workers, taking psychoanalytic theory and learning theory as rival paradigms, it does not just imply the expected result that analysts and behaviourists will differ about what they mean by reports which are clearly theoretical such as 'this person has repressed his anger' or 'this behaviour is positively reinforced by the actor's environment'; it implies that they will mean something different when apparently using the same words, such as saying that 'this is a person' and 'this person says he is worried'. With this holistic theory of meaning then there is no common language in which we can state the evidence and so comparisons cannot be made. The paradigms are said to be 'incommensurable'.

For many critics of this holistic theory of meaning, Newton-Smith reports, "its consequences are sufficiently absurd to justify its rejection" (1981, p.157). More specific objections are made as well.

Kuhn tries to avoid some of these absurdities by claiming that the meanings of terms only change when we have a paradigm shift but that it stays constant through the "minor" modifications scientists make as they work within a paradigm. If small alterations led to meaning change then it would be difficult for any scientist to find a colleague who spoke the same language but it seems clear

that they do communicate. Although some such concession looks essential, Kuhn is criticised for his response:

> Kuhn does not provide an adequate criterion for determining how much change is required before there is a change in paradigm. This means that he has not provided a means of determining which theory changes generate variation in meaning (Newton-Smith, 1981, p.155).

This leads to difficulties for his theory of meaning. Psychoanalysis and behavioural psychology are generally accounted different paradigms. But the question arises whether, within psychoanalysis, the shift from Freud to Adler is a minor one or sufficient to warrant claiming that they meant different things by their common terms. Or should even the changes Freud himself made in his theories be judged enough to imply that the older Freud saw the world differently from the younger Freud? Kuhn offers no guidance on this point.

Kuhn's views are also criticised for conflicting so sharply with scientific practice. Scientists working on different theories show every sign of being able to communicate and to agree on a level of observation reports.

Putnam (1981, p.114) objects to the Kuhnian incommensurability thesis because, he argues, it rules out translation of any kind: between paradigms, between cultures, and even between older forms of our own language and the present day. However, translation and understanding others is possible, Putnam claims, pointing out that even Kuhn assumes he can talk meaningfully and in a paradigm-neutral way when he presents his incommensurability thesis. Kuhn cites Galileo as an example of someone with a different paradigm from our own

and therefore, if his thesis is true, with no language in common with us. However Kuhn then finds no difficulty in talking about Galileo's ideas believing both that he is giving an accurate account and that we shall have no difficulty in understanding him. Putnam complains: "to tell us that Galileo had "incommensurable" notions and <u>then to go on to describe them at length</u> is totally incoherent" (1981, p.115).

Putnam suggests that proponents of the incommensurability thesis and the holistic theory of meaning are confusing or conflating 'concept' and 'conception'. The concept or the reference of a term stays constant through translation although the conception, our associated beliefs about it may change:

> when we translate a word as, say <u>temperature</u> we equate the reference ... with that of our own term 'temperature', at least as we use it in that context....But so doing is compatible with the fact that the seventeenth-century scientists, or whoever, may have had a different <u>conception</u> of temperature, that is a different set of beliefs about it and its nature than we do (Putnam, 1981, p.117).

If we turn to the social work advocates of relativism, they seem to fall into two groups: the wholehearted and the faint-hearted. Howe (1987) and Paley (1987) welcome the consequences of relativism. For them, questions about collecting empirical evidence are redundant; the various approaches to social work cannot be compared on epistemological grounds. Their interest is in the question of the next chapter - how do scientists evaluate theories, or, to put it in their terms, why do social workers prefer one or other approach?

Others however appear faint-hearted in their

relativism and seem more concerned with rejecting the behavioural restrictions on research rather than abandoning the whole scientific enterprise. Heineman (1981), Smith (1987), and Ruckdeschel and Farris (1981), for example, appear to want to reject positivism but not to move to accepting a purely relativist position.

Smith is scathing of Sheldon's firm advocacy of a scientific approach, saying such a view is now out of date because of the developments in philosophy. He describes it as:

> an anachronism, rather like seeing an airship in flight; the effect is bracing, and it is nice to know that the feat can still be brought off, but one would not choose it as a way of crossing the Atlantic (Smith, 1987, p.404).

This looks an unequivocal indictment of science and yet his subsequent plea (p.414) is for "a variety of research approaches .. ethnographic 'hanging about', the analysis of system data, and the use of a quasi-experimental design in the assessment of outcomes." None of these seems incompatible with an empiricist philosophy of science.

Heineman's position is also unclear. She explicitly describes herself as a relativist but still declares that "science represents our best efforts at solving important problems for which there can be no guaranteed or permanent solutions" (1981, p.391). She does not explain how she, as a relativist, judges science to constitute "our best efforts".

It is of course possible to be a relativist and choose to work within the scientific culture for non-

epistemological reasons, but to some extent these writers seem unclear how much they are rejecting along with positivism. The main conclusions of these writers are that scientific research methods need to be extended rather than abandoned. In wanting to liberalise scientific methods, they may find that the revised empirical position provides an adequate answer.

EMPIRICISM

The recurrent complaint of social workers about positivist research is that it is quantitative not qualitative. The dispute is complicated by the fact that the key term "qualitative" is used in vague and ambiguous ways. Two features which seem recurrent themes in the various calls for a change of direction in research are, first, a desire to study people's inner thoughts and feelings, and, secondly, an appeal to researchers to experiment with novel ways of studying people. I shall deal with these two issues separately.

First, positivist research has been accused of ignoring the client's subjective experience. This is only true of the strict form of behaviourism which did not study the mind; positivism in general cannot be accused of excluding all reference to the mind. But studying thoughts, feelings, and beliefs was undoubtedly harder when researchers were trying to satisfy the positivist requirement for behavioural evidence only. If this requirement is now deemed misguided, what are practical implications for social work research?

To the empiricist, the fallibility of all observation statements does not imply that science has no empirical base. Nor does recognising that an observation report involves some theoretical assumptions imply that it assumes the very theory it is being used to test. The rejection of a sharp observation/theoretical distinction however does have significant implications in the social sciences. The positivists' rule that only behavioural reports were empirical was based on the assumption that there was a sharp divide, with behaviour on one side and psychological states on the other. Such a firm rule is as we have shown inappropriate. But by what criteria can we judge where statements fall on the continuum? We need to answer this to understand how research methodology in social work can move away from the behavioural restrictions on evidence.

Research textbooks agree that the two main criteria for judging the adequacy of observations and measurements are "validity" and "reliability".

"Validity" is defined by Reid and Smith (1989, p.199) as "the extent to which a measure corresponds to the "true" position of a person or object on the characteristic being measured ... it attempts to capture an elusive property of measurement: 'its truth value'." How valid for example are the official unemployment statistics as a measure of the level of unemployment in this country? Validity cannot be ascertained beyond doubt: it "is inevitably a matter of judgement based on evidence and inference".

"Reliability" is an aspect of validity which is treated separately because it can be assessed reasonably precisely. It refers to the extent to which observations

or measurements are consistent over time and between observers, whether, for instance, a blood pressure test produces similar results when repeated on the same patient whose blood pressure has not changed or when used by different doctors. The two main methods of judging this are (1) test- re-test consistency, and (2) inter-rater agreement (sources: Reid and Smith, 1989, p.199, and Freeman and Tyrer, 1989, p.133).

The long-standing social work dispute about research can be rephrased in terms of these criteria. Researchers have used behavioural indicators because they tend to have higher reliability than fieldworkers' subjective judgements but fieldworkers then complain that these measures have low validity, being only a crude and inaccurate measure of their psychological concepts.

Burch and Mohr's (1980) study is, I think, a good example of research which is flawed in this way, having outcome measures which are reliable but of poor validity given the complex psychological theories being tested. The study evaluated a social work programme for parents who had physically abused their children, comparing a group receiving the experimental help with a control group receiving the standard service. The groups were matched on the factors considered causally significant such as age, social class, and degree of social isolation. The new treatment was based on the assumption that child abuse results from an interaction of social and psychological factors. It was postulated that abusing parents tend to be isolated, to lack knowledge about child development, to be under stress which hinders their ability to solve problems,

and to lack the ability to nurture their children because of inadequacies in their own upbringing.

The programme had several goals: 1. to change their feelings, attitudes, and values about parenting; 2. to increase their knowledge of child development; 3. to provide support to ease their isolation; 4. to provide a nurturing experience. Treatment consisted of a weekly two-hour group meeting in which there were episodes of socialising, educational presentation on parenting skills and child development, and small group discussions on personal problems and parenting skills which were also intended to provide a nurturing experience.

Evaluation was based on rating scales which allegedly measured social isolation and attitudes to and knowledge about child-raising. Completing them before and after treatment, the experimental group scored significantly higher on the second rating than the control group.

The reliability of these rating scales is evidently high and is not at issue; their validity is. The programme had a complex psychological theory about child abuse and offered a treatment with many elements. These rating scales seem a test of only parts of the theories underlying treatment. At best they have a tenuous link with the hypotheses about the importance of stress and lack of nurturing in creating abusing parents. Testing whether the programme had indeed met its aim of providing a nurturing experience or reducing stress might be complicated but, since it failed to do so, the study can be accused of having low validity. Moreover, these hypotheses are typical of the kind used by fieldworkers and which they say

positive research cannot adequately test. This study does nothing to refute their claim. Oddly, the study did not record what is surely the most valid and reliable evidence of success or failure – the incidence of further child abuse.

Researchers might defend their practice by pointing out that, though humanists may question the validity of behavioural evidence, it does seem to be more reliable than psychological reports. It is clearly true that we often use psychological language in different ways while many behavioural descriptions are quite uncontroversial. It must be remembered however that in ordinary language, many terms are often very vague and imprecise, particularly in comparison with terms in science, and in this respect psychological language is not exceptional.

Colour is often cited as a paradigm example of an observable property but in general speech there are many disputes about the use of colour predicates. There might be considerable agreement on what counts as a primary colour like red or blue, but there is ample scope for argument when it comes to shades like turquoise, violet or amber. These colour concepts have much vaguer, varied rules of use. However, all colour concepts could be given a precise and consistent meaning by reference, for example, to wave length. Two factors are important in doing this. First, scientists have developed an extensive theory about colours which allows them to give any shade a precise description in terms of wavelength; secondly they have developed instruments which enable them to measure wavelengths accurately. The gradual refinement of concepts

usually goes hand-in-hand with theory development.

The differences apparent in ordinary usage between behavioural and psychological terms then are a matter of degree rather than of kind. Moreover, intuitive concepts typically need to be made more precise to standardise usage for scientific purposes. With psychological terms, this is usually done partly by reducing the concept to behavioural or other relatively uncontroversial indicators and partly by teaching people the skill of making consistent judgements.

Research widely known amongst social workers which illustrates this procedure concerns the relationship between "high expressed emotion" and the relapse rate for schizophrenia. From clinical experience and research on the relapse rates for schizophrenia, it seemed plausible that patients who returned to live with families in which there was high expressed emotion were more likely to suffer a relapse than those in families with low levels of expressed emotion.

In a series of studies, Brown et al. (1958), Brown et al. (1962), Brown et al. (1972), Vaughn and Leff (1976), evidence for the causal significance of levels of expressed emotion was accumulated and the concept itself increasingly refined. To test their hypotheses, the research team trained observers to use this complex concept of "expressed emotion" with very high inter-user agreement.

The way this was done is interesting both because it reveals the complexity of the task and because it uses a combination of empirical indices and user skill. Researchers are told not only how to rate expressed emotion

but are given guidance on how to collect the relevant information. In two methodological papers, Rutter and Brown (1966) and Brown and Rutter (1966) describe in detail (a) the most appropriate techniques of interviewing, describing a style which encourages respondents to develop their own lines of thought rather than being guided too much by the interviewer; (b) a semi-structured interview schedule, listing the areas on which information is required; and (c) the criteria for rating expressed emotion. This final category is broken down into five measurements: of the level of critical remarks, degree of emotional overinvolvement, hostility, warmth, and positive remarks. The last measurement is made simply by counting the number in an interview of a specified length. The others are measured in a more subjective way but nevertheless consistently. People are trained to measure them by being given examples of the type of comments which would indicate high or low levels but, unlike a positivist's operational definition, no attempt is made to spell out the full procedure for rating expressed emotions, the interviewer's skill in applying the concepts is an essential component. To achieve consistency on this, training is provided, using case examples and videos to help standardise judgements. The research team report that by these methods they are able to get very consistent rating of the extent of "expressed emotion" (Berkowitz et al. 1981).

Accurate rating has become of clinical importance since later research has (a) confirmed the significance of high expressed emotion in triggering relapse in schizophrenia, and (b) shown that family work which helps

the relatives lower the level of expressed emotion is associated with a drop in relapse rate.

This example illustrates how complex psychological concepts can be studied empirically. Using the concept correctly requires training; human skills are, as the humanist claims, necessary in research. But this is true of both natural and social sciences. In medical research, for instance, doctors need training to ensure correct measurement of blood pressure.

Another empirical study well-known to social workers illustrates how researchers can meet social workers' demand that they should study the client's subjective experience.

Brown and Harris (1978) investigated the social causes of depression in women. Earlier research had implicated what was imprecisely called "life events" in the causation of depression and methods had been worked out for interviewing people and rating their "life event score" in a consistent way. Brown and Harris however thought that a major defect in earlier rating systems was that they had not considered the "meaning" of the life event to the individual; they had treated "a wide range of events as alike that are not alike. The birth of a child does not mean the same thing for all women" (Brown and Harris, 1978, p.81). The existing scheme of rating life events was therefore considered inappropriate:

> incidents once classified as 'events' were treated as equivalent as far as severity of threat, disruption, and the like were concerned. We now needed in some way to bring meaning back (Brown and Harris, 1978, p.85).

The interviewing schedule was changed to elicit not only whether life events had occurred but the woman's

response to them "in the sense of the thoughts and feelings she had before, at the time, and after the event" (p.86). Each interview was tape-recorded and the tapes later used to complete 28 rating scales covering each event. These were then used to "make a judgement about the likely meaning of the event for the average person in such circumstances" (p.90). To avoid bias, the raters did not know whether or not the women had developed depression after experiencing the life event.

These more recent research studies show that, even if social workers are justified in complaining that their researchers have not given enough attention to the client's experience, the deficiency cannot be blamed on research methodology itself.

Turning now to the second theme apparent in the call for qualitative research: researchers are told that they should be innovative, trying many novel ways of studying people. Numerous new methods are suggested e.g. case studies, participant-observation, ecology, phenomenology, ethnographic "hanging about" (Smith, 1987, p.404), and ethnomethodology. One article (Ruckdeschel and Farris, 1981, p.419) even reports that the notations of jazz music and the "deep structures" of transformational grammar have been useful aids to understanding.

As Thyer (1989, p.312) complains, judging these new research ideas is difficult because the authors make only vague allusions to them and give us no practical examples of the type of research they would produce to illustrate their alleged value. One common feature seems to be that the researcher gets involved with the people being studied.

In the detailed case study, the participant observation or the unstructured interview, the researcher is actively engaged with those whose experience he is trying to understand. This seems to contrast with the image of a scientist as a neutral observer collecting facts but, as I have argued in this chapter and Chapter 4, this image is misleading. These methods, since they are designed to study meaningful human actions, conflict with extreme forms of behaviourism but are otherwise compatible with an empirical approach. Indeed some recent textbooks include sections on qualitative research (e.g. Reid and Smith, 1989).

The controversy about these methods of study does not centre on whether they have a useful part to play — the empiricist can have no a priori objection to them as a source of theories and in this capacity they may be very fruitful. The question is what weight should be given to the theories they produce. Since they resemble the way "practice wisdom" is acquired by the individual fieldworker, they are open to the same criticisms about reliability that I made in Chapter 6. To the empiricist therefore they need to be empirically tested. Relativists however would claim that they had their own internal validity; they are a rival to scientific methods rather than a component. While a few, such as Paley and Howe, are thoroughgoing relativists, the majority of social workers who advocate these innovations do not make it clear where they stand on this issue.

In summary, I have tried to illustrate here the revised empiricist view on the testing of theories. On

this account of empirical evidence in the social sciences, the positivist/humanist disagreements no longer apply. The humanists' insistence that understanding another person requires personal skill is to some degree accepted; we cannot write out all our background knowledge fully in behavioural language; neither can we make accurate reports without drawing on this background knowledge. Our ordinary language is however often vague and imprecise, leading to inconsistent usage and leaving considerable scope for bias to influence the judgement. For scientific purposes, these defects need to be minimised and the research examples I have quoted give some indication of how this can be done. In none of these features however does social science differ significantly from natural science.

Social work criticisms of research efforts at testing psychological theories can, in principle, be refuted by this revised version. Theories involving elusive psychological concepts can be tested empirically and it is easier to study the subjective experiences of the client. However, the fact that I have had to turn to related disciplines to find examples of innovative research suggests that social work researchers have not yet fully appreciated the possibilities.

CONCLUSION

The widespread reluctance among social workers to formulating and testing theories empirically in the recognised scientific method has traditionally been based on a behavioural/positivist view of science. Positivism

implied that psychological theories needed to be tested by behavioural evidence but social workers claimed such evidence provided an inadequate test of their ideas and preferred instead to rely on personal intuition and empathy. This resistance to scientific methods of testing now needs to be re-appraised since the model of science they reject has been generally discarded. But, as I have argued, the sharp dichotomy between observation and theoretical terms which is crucial to the positivist position is unfounded and should instead be seen as a spectrum. As behavioural terms were classified as observable and hence able to provide empirical evidence while psychological terms were deemed to be theoretical, this alleged dichotomy is at the centre of the humanist/naturalist conflict.

I have also argued that social workers' reasons for opposing empirical testing based on a positivist account do not apply to other, more satisfactory, views on the role of empirical evidence in science. Research studies in related fields illustrate how psychological theories can be adequately tested empirically.

Researchers in social work, influenced by philosophical criticisms of positivism, have themselves now become critical of their traditional approach, conceding that fieldworkers had some grounds for decrying past efforts. There are however substantial disagreements about what the philosophical changes have been and their implications for research practices.

"Positivist" research is widely attacked but, I have argued, some of the defects of former research cannot be

directly attributed to positivism. Studying the results but not the content of social work services for example has been a common fault in evaluative research but it does not follow from any positivist doctrine. Researchers are also criticised for insisting that evidence should be not just observable but quantifiable. While this may have unduly restricted the scope of their work, again it cannot be blamed on positivism. The difficulty of implementing the positivist ruling on empirical evidence, excluding all psychological terms, undoubtedly complicated the researchers' task. Meeting this proviso seems to have distracted them from what should be their first priority of conducting research that meets the needs of social workers and their clients.

While there is widespread agreement that previous research was defective, there are sharply differing views on what should now be done. The major split is between those who take a relativist view and those who advocate some modified but still empiricist position.

The relativists claim that all so-called observation statements in science are not just theory-laden (a point empiricists accept) but derived from the very theory which they are supposed to be testing, therefore providing no independent check on the theory. I have presented the arguments against the holistic theory of meaning on which this claim is based.

On the empiricist side there is disagreement about the practical implications of the philosophical change. There is agreement that it permits a more liberal methodology but just how liberal is disputed. A key issue is expressed in

terms of rivalry between quantitative and qualitative research.

In recommending "qualitative" research, the main desire seems to be to study the subjective experiences of clients. While positivism cannot be accused of banning such research, its behavioural restriction on evidence made it difficult and the modified empiricist view of evidence makes such research easier.

Another element in the move towards qualitative research is a desire to see researchers trying out novel methods of investigation - ethnomethodology, phenomenology, ecology are all cited as possibly fruitful approaches. For the empiricist, such methods may have a part to play in developing our understanding but the theories they produce need further empirical testing.

CHAPTER EIGHT

WEIGHING THE EVIDENCE

INTRODUCTION

Scientists test theories empirically; they evaluate them by collecting evidence through experiments and comparative trials. Empirical evidence however does not have a decisive impact on a theory, either in proving or in refuting it. If social workers were to carry out empirical research, how are they to interpret their results? There are competing accounts of how scientists weigh the evidence and what makes them decide to accept or reject a theory. The disputes are not so much about the decisions scientists have made but about <u>how</u> and <u>why</u> they made them, why for example they prefer Einstein's physics to Newton's.

In social work, this philosophical issue has been addressed most by those arguing for a relativist position about knowledge. Their attack on the rationality of science has two strands: first, as discussed in the preceding chapter, the argument that there is no independent empirical evidence by which to test a theory; secondly a claim that there are no universal rational criteria by which the evidence can be weighed. Deciding whether to accept or reject a theory in the light of the evidence, the relativist claims, is not a rational process

but determined by social and psychological factors. Howe (1987) for example alleges that: "dominant forms of understanding are based on social processes rather than empirical validity". Paley (1987), who shares this view, sees those who advocate a scientific approach in social work as "imperialists" trying to impose their preferred culture on a mainly humanist social work group.

This relativist view, which is of recent origin, has far-reaching implications for social work. It claims that arguments for using scientific methods such as those I have presented in this thesis are irrational. Thus relativism is often used to endorse the prevailing style of work where appraisal of theories and evaluation of work is left mainly to the individual fieldworker. As yet, this point of view has been little challenged by the proponents of a scientific approach though this will be one of my aims in this chapter.

The issue of interpreting the evidence has received attention from one strong supporter of scientific methods in social work: Brian Sheldon. Because of concern about social workers' failure to look for falsifications of their theories or to pay attention to counter-evidence, he has been interested in clarifying the role of refutations in scientific reasoning to show social workers what they ought to be doing. He turns to Popper's philosophy of science in which falsifications play the central part, but, as I shall discuss, there are problems with this account of science. Sheldon is quite right however to stress the importance of refutations in science though there are better accounts of

their significance than Popper's.

Other than Sheldon, the philosophical debates about appraising theories have received little attention from empiricists in social work, who seem to believe that analysing results can be left to statisticians. Their view is perhaps influenced by the fact that the predominant philosophical debate has been concerned primarily with deterministic theories, i.e. ones that make categorical predictions. Such theories are rare in the social sciences: most are probabilistic or statistical, predicting outcomes only with a certain probability. The same issues arise though in relation to both types of theory.

My primary concern then in this chapter is to provide an empiricist account of how scientists judge theories. The first section, on inductive support, discusses the problems of induction and offers a Bayesian account of how scientists decide that a theory is more probable given the evidence. Evidence can count for or against a theory as Sheldon has emphasised and the second section addresses his concerns, providing a critical look at Popper's philosophy and explaining how an inductive approach deals with refutations.

I then examine the relativists' claim that theory appraisal is wholly determined by social and psychological factors, not the empirical evidence. Although I disagree with their contention that epistemological criteria play no part, I shall argue that they are right in saying that deciding what to do in social work cannot be determined by epistemological criteria alone. Deciding to act involves

not only judging whether one's therapeutic hypothesis is probable but also whether one's goals and means are morally, politically, and economically acceptable.

INDUCTIVE SUPPORT

To test a theory, scientists deduce some of its empirical consequences and then carry out research to find out whether those predictions are accurate or not. If for instance we have the simple hypothesis that all swans are white, then it implies that if we find a swan it will be white.

If the predictions are true, it does not follow of course that the theory is. It is logically possible that the theory is false even though the prediction is true. We might for example have studied the swans in Hyde Park and seen that they were all white; it is still possible that some of the unstudied swans, in Kew Gardens for instance, are not white.

However, though verified predictions cannot establish the truth of a theory, they are often said to give it inductive support. As scientists testing a theory collect more and more confirmations, their confidence in the theory generally increases. The reliability and justification of inductive reasoning are the problems which have interested philosophers.

First, consider its reliability. We usually in our practical life assume that past experience is a guide to the future; this is reasoning inductively. The problem

with inductive proof is that the evidence for our theory is never conclusive. The general conclusion is reached on the basis of our limited experience of particular instances. A theory about ALL planetary movements, for example, is supported by observations of relatively few planets. But how can we be sure that our generalisation is true, that hitherto unobserved planets will behave in the same way as the ones we have seen, or that the ones we have studied will keep to their current paths? The answer is that we cannot know with certainty.

Bertrand Russell's story of the chicken is a famous illustration of how the conclusion of an inductive inference is not certainly true: every morning the farmer went to the chicken run to feed the hens; the chicken noticed this regularity and, reasoning inductively, began to look forward to the farmer's visits and would run out to meet him and expect the food; one day though, just before Xmas, the farmer did not keep to his routine of bringing food but picked him up and killed him instead.

Although inductive arguments cannot guarantee the truth of their conclusions, most of us would still accept that they provide our best method of reasoning beyond our experience. The chicken was certainly disappointed on his last day by the farmer's actions but, since he did not know that the farmer was only fattening him up for market, the chicken's expectation of food, given his past experience, was reasonable though wrong. It is in fact difficult to imagine what we would do if we did not accept that the past was a guide to the future (and if the world did not behave

in a way consistent with that assumption). We could not for example learn language if we did not expect people to use words in a fairly consistent way.

In science, one can never prove with certainty that a theory is true but, in most philosophies of science, (with the notable exception of Popper's, discussed later) it is accepted that inductive support provides grounds for increasing confidence in a theory.

Philosophers, however, have shown that it is difficult to justify induction. David Hume, in the eighteenth century, gave the most famous account of this. The most obvious justification is that induction works; it may be wrong sometimes but on the whole it produces fairly reliable ideas. But Hume pointed out that this type of argument is circular: the past success of induction is being used as evidence for its future reliability when what we have been asked to justify is the very principle of using the past as evidence for the future.

Despite many philosophical efforts, the problem of justifying induction has never been solved to everyone's satisfaction. Hume though did not see his criticisms as undermining our use of induction. In his view, we cannot help believing in a physical world and reasoning inductively about it. Scientists in general follow this approach, accepting that a theory can never be conclusively proved but thinking that it can be judged as more or less probable in the light of the empirical evidence.

For most social workers, although they are hostile to scientific method, doubts about induction are not among the

reasons for their antipathy. Indeed, induction plays as much a part in humanist social work as in a scientific approach. Most social workers believe that through their work experience they can develop "practice wisdom" and, in using past experience as a useful source of understanding of the present, they are evidently reasoning inductively. The problem of induction is however an element in two schools of thought in social work: it forms a part of the relativists' attack on the rationality of science and it has led some naturalists to propose Popper's philosophy of science in which induction plays no part. Both will be discussed later.

Learning from experience plays an essential role in science. The exact mechanism has however been debated by philosophers. How much confidence, for instance, should one invest in a theory in the light of evidence; how do we compare theories; and when should we reject one altogether? The most satisfactory account, I think, is provided by the Bayesian approach which holds that "scientific reasoning is reasoning in accordance with the calculus of probability", (Howson and Urbach, 1989, p.12). Not only does this account seem to avoid difficulties met by rival philosophies but it has a plausibility capturing as it does the way scientists talk about their reasoning. Dorling (1979, p.180) reports that it is rare to find any scientist in the last three hundred years who does not talk in terms of probability. The Bayesian approach also has the attractive feature of providing a uniform account of deterministic and probabilistic theories.

The Bayesian approach is named after Thomas Bayes, an eighteenth century clergyman, who derived the important theorem which determines how much more probable a theory is in the light of a new piece of information than it is without that evidence. If h is the theory being tested, e a bit of evidence, and k the background knowledge accepted prior to the test, what we want to know is whether e supports h and, if so, to what degree it does so. Bayes theorem works out how much more probable the theory is, given the new evidence from the test. The scientist estimates the theory's "prior" probability, its likeliness given background knowledge without the new evidence. Bayes theorem then calculates its "posterior" probability in the light of the additional information.

Bayes theorem can be expressed thus:

$$P(h/e.k) = \frac{P(e/h.k) \ (P(h/k)}{P(e/k)}$$

In explaining what this equation means, we can also see how Bayesian reasoning captures the generally accepted features of scientific decision-making.

The P(h/e.k) — on the left-hand side of the equation — is the answer wanted: the new (posterior) probability of h in the light of the test result. Its value is determined by the values of the 3 probabilities: P(e/h.k), P(h/k), and P(e/k).

First, the P(e/h.k) indicates how firmly the hypothesis predicts the result. The higher this probability is, the more the evidence will support the hypothesis. In a deterministic theory, if h implies e then P(e/h.k) = 1.

It follows from this that if e refutes h then P(e/h.k) = 0, which is the lowest probability, and h is falsified or, in Bayesian terms, maximally disconfirmed.

Secondly, the posterior probability of h is dependent on its prior probability: P(h/k), its likelihood given background knowledge alone, before doing the test. Howson and Urbach argue that assigning prior probabilities is, in practice, an unavoidable aspect of science: "scientists always discriminate, in advance of any experimentation, between theories they regard as more or less credible and, so, worthy of attention and others" (1989, p.80). Calculating the prior probability involves a subjective judgement in the sense that individuals with different knowledge, beliefs, or backgrounds may assess it quite differently. Social workers should find this part of Bayesianism very familiar since so much of their current style of individual working requires personal judgements of the plausibility of their understanding. The Bayesian approach does not imply that social workers should abandon this aspect of their current methods of reasoning but that they should treat it as the first rather than the final stage in evaluating a theory.

The third probability, the P(e/k) indicates the likelihood of e given background knowledge alone. The degree of support e gives an hypothesis depends on how much more likely e is given the hypothesis than it is on background knowledge alone. This reflects the value scientists give to surprising predictions. Howson and Urbach (1989, p.86) argue that this intuition is true in

everyday experience as well. They give the example of a soothsayer. If he predicts that you will meet a dark stranger sometime and you do so, your confidence in his predictive powers will not be much increased since meeting a dark stranger is almost inevitable if you live in Britain. If however, the soothsayer were to predict the correct number of hairs on the head of that stranger, you would be amazed and your scepticism would be shaken because, without the hypothesis that the soothsayer can foretell the future, making such a correct prediction is highly improbable.

Having spoken only of the merits of a Bayesian approach, let me turn to its critics. Bayes theorem itself is not controversial. The axioms from which it is derived are common to most accounts of probability. The main criticism levelled at the Bayesian approach concerns its subjective element: fixing the prior probability of a theory is said to be a subjective decision by a scientist. In this respect, this philosophy fails to meet the hopes of objectivists like Popper, Lakatos, Carnap and, in statistics, Fisher, Neyman and Pearson who all want an account of science in which to use Lakatos's words: "the cognitive value of a theory has nothing to do with its psychological influence on people's minds" (1978, vol.1, p.1).

On this point, Howson and Urbach have two main defences of Bayesianism: first, the failure of their opponents to develop an adequate objective method of assigning prior probabilities, and, secondly, the failure

of their critics to take account of how limited the subjective element is.

The rival probability approach has tried to establish the objective probabilities of theories, using only factual data and the logical structure of the hypotheses – and no opinions. The assumption is that, if two people have the same factual data, they should assign the same prior probabilities. Theoretical views or personal factors should have no influence on the computing of probabilities.

Howson and Urbach (1989, Chapter 3) argue that not only have the objectivists failed in their task but also failure is unavoidable. Purely objective criteria for determining prior probabilities do not exist; all methods must make some assumptions about the data:

> no prior probability or probability–density distribution expresses merely the available factual data; it inevitably expresses some sort of opinion about the possibilities consistent with the data (1989, p.289).

The seekers after objectivity fear that, if prior probabilities are assigned in part subjectively then Bayesian reasoning is: "a record merely of the whims of individual psychology" (Howson and Urbach, 1989, p.289). This, argue Howson and Urbach, is to greatly overestimate the significance of the subjective element. While the individual decides the initial probabilities, what happens to them subsequently is determined by the probability calculus.

They suggest the analogy with deductive logic. This does not tell us whether the premises of our argument are true or false but, once we have decided their truth-value

by independent means, it dictates the valid inferences we can make from them. Similarly, Bayesian theory does not tell us the prior probabilities of our ideas but, once these have been given a particular value, it computes their posterior probability. "As far as the canons of inference are concerned, neither logic [neither inductive nor deductive] allows freedom to individual discretion: both are quite impersonal and objective" (1989, p.290).

One might expect that the subjective element would lead to radical variations in the assessment of the probability of a particular theory in science and this seems counter to experience. Natural scientists generally reach a high degree of agreement on the merits of their theories. However, applying Bayes theorem, major disagreements do not usually last for long because, when weighed against a common body of evidence, the posterior probabilities typically converge rapidly as evidence accumulates. A low prior probability will be substantially increased by empirical evidence while a high one will only be affected slightly. Therefore, after a few tests have been done, most of the initial difference will have disappeared.

Scientists often discuss their theories in terms of probabilities. Evidence cannot prove a theory with complete certainty but it can make it more or less probable. The Bayesian approach which bases scientific reasoning on the probability calculus offers a coherent account of inductive reasoning which captures the generally accepted features of scientific judgements.

REFUTATIONS

Sheldon has been a persistent advocate of a scientific approach in social work and a critic of current social workers' methods. His particular concern has been social workers' disregard for refutations of their theories. Social workers, he complains, make little effort to find refutations or, if some are found, they fail to take them seriously as a challenge to their ideas. They use "theories containing built-in defences against disbelief" (Sheldon, 1978, p.14) so that any apparent refutation can be accommodated. Sheldon claims too that social workers have such vague goals that virtually any outcome can be interpreted as a favourable one. He cites the case of Mary, a schoolgirl known to a particular Social Services Department, who was expelled from school. On the face of it this is an undesirable event suggesting that current social work efforts to help had not yet succeeded. Her social worker however did not see it in a negative light. Expulsion from school was, Sheldon complains, "massaged into "a not altogether unwelcome opportunity to re-evaluate Mary's educational options" (1987, p.583).

This apparently uncritical attitude is condemned by Sheldon. He sees it as a dereliction from the kind of standards accepted in science. But, in arguing for a more critical approach, he is influenced by Karl Popper's account of science. This philosophy, as I shall argue, has serious flaws and a better account of the role of refutations in science is provided by Bayesianism.

It is easy to see the immediate appeal of Popper's philosophy to Sheldon since both are prompted by a common concern. Popper worked briefly as a social worker in Alfred Adler's child guidance clinics in Vienna. He reports that many of his friends had enthusiastically embraced psychoanalytic theories but Popper became worried by what he saw as their uncritical acceptance of them. They interpreted <u>all</u> evidence as confirming the theory; nothing seemed to count against it. The study of psychoanalysis:

> seemed to have the effect of an intellectual
> conversion or revelation, opening your eyes to a new
> truth hidden from those not yet initiated. Once your
> eyes were thus opened, you saw [apparent] confirming
> instances everywhere: the world was full of [apparent]
> VERIFICATIONS of the theory. Whatever happened always
> confirmed it (Popper, 1963, p.34).

Popper reports that he was spurred into studying the philosophy of science to try to understand how science differed from this, as he saw it, dogmatic approach and reached his well-known conclusion that the hallmark of a scientific theory is that it is falsifiable. While there is general agreement however that refutations play a central role in science, his account of science has been severely criticised.

Let me begin with a brief account of his philosophy before detailing the main objections. Popper concedes that we cannot prove that a scientific theory is true but, because of the asymmetry between positive and negative results we can know if it is false. As noted in discussing induction, true predictions do not imply that the theory from which they were deduced is also true. When the prediction is false however it has a quite different

logical impact on the theory; clearly the theory must be false. We can reason deductively:

1. if T then P
2. not P
Therefore not T.

The theory is falsified. Although finding hundreds of white swans never conclusively proves the hypothesis "all swans are white", discovering one black swan disproves it.

Popper makes the extreme claim that not only can evidence not prove a theory but also it cannot increase its probability. Scientific theories, to Popper, can never be inductively supported only deductively falsified. When scientists draw out the empirical consequences of their theories and design experiments to test them, they are not trying to confirm their theories but to disprove them. They do not prefer one theory because it seems more probable or better supported by the evidence but because it has so far withstood their attempts to falsify it. Scientists, according to Popper, can never conclusively establish that a theory is true or, as a Bayesian would claim, that it is more probable than another; the only statement a scientist can make with confidence is that a theory is false:

> the method of falsification presupposes no inductive inference, but only the tautological transformations of deductive logic whose validity is not in dispute (Popper, 1959, p.42).

Where inductivists describe positive results as "supporting" a theory, Popper talks of "corroboration". This sounds similar but is substantially different in Popper's interpretation. Popper's "corroboration" carries

no implication of increased probability. Indeed he claims that the theory remains a highly improbable conjecture but, by withstanding attempts at refutation, it shows its "fitness to survive". To call a theory well-corroborated is to report on its past performance not to make a prediction about its future merits. If it were seen as an indicator of its future performance, this would involve inductive reasoning, which Popper forbids.

Popper has been immensely influential in the social sciences. However such damaging criticisms have been made of his account of science that it should not be held up as a model for social workers.

First, Popper claims that the one certain feature of science is that an observation statement, when found to be false, can falsify a theory. Yet Popper agrees with the argument reported in my previous chapter that observation statements are fallible. If it is logically possible that the observation is wrong then it is logically possible that the theory, although apparently falsified by it, is true. The alleged objective certainty is in fact illusory.

In practice, scientists sometimes query the results rather than accepting them as counter-evidence. Newton is reputed to have done so when the Astronomer-Royal reported observations which conflicted with Newton's theories; on repeating his observations, the Astronomer-Royal found he had made a mistake.

Another criticism of Popper's methodology is that it is not the way scientists actually work. They do reason inductively and talk of theories being more or less

probable. At best he is prescribing what scientists ought to do not describing what they are doing. This option though is also criticised. Putnam (1980) raises an objection which seems particularly pertinent to social work. Popper's attempt to exclude induction, he argues, is based on an unreal picture of science. To Popper, scientists are only interested in knowledge for its own sake whereas, Putnam points out, science is about developing knowledge which can be used; therefore scientists must be concerned with the future reliability of their theories, not just their past performance and so inductive reasoning is an unavoidable part of any practical science:

> when a scientist accepts a law, he is recommending to other men that they rely on it — rely on it, often, in practical contexts. Only by wrenching science altogether out of the context in which it really arises — the context of men trying to change and control the world — can Popper even put forward his peculiar view on induction. Ideas are not JUST ideas; they are guides to action (Putnam, 1980, p.335).

For social workers, this point seems particularly apt. They want theories for practical purposes. Accepting a theory affects their actions, having major repercussions on clients, possibly leading to a child being removed from his parents, or an offender being recommended for a custodial sentence rather than a Probation Order. Social workers have to make predictions; they have to decide which hypothesis is more reliable, trustworthy or probable.

Imagine the feelings of a client who is told by a Popperian social worker that he is taking his child away from him not because the social worker has reasonable grounds for concluding that the child is likely to be

abused but because he has a highly improbable conjecture on the subject which has not yet been falsified. The sophisticated client might reply that, in the interests of science, the child should be left at home so that the social worker can test his hypothesis; removing the child would stop the social worker from ever having the right circumstances in which his conjecture could be falsified. This however is not an option open to the social worker. The law requires him to act in the best interests of the child and this entails reasoning inductively from past evidence to future probabilities. As Putnam asserts:

> since the application of scientific laws does involve the anticipation of future successes, Popper is not right in maintaining that induction is unnecessary. Even if scientists do not inductively anticipate the future (and of course they do), men who apply scientific laws and theories do so (Putnam 1980, p.355).

Another major problem for Popper's philosophy is the Duhem-Quine thesis. Duhem (1905) and Quine (1953) both pointed out that experiments in science involve more than a single conjecture and an empirical observation. To derive a prediction, additional premises are needed, at least to state that the "initial conditions" are met, i.e. that the circumstances to which the theory refers are present. In the simple example of the hypothesis "all swans are white" we need an additional premise that "this is a swan" before we can infer "this is white." If observation shows that this swan is black, we may infer that the set of premises as a whole is false but not specifically that the hypothesis is.

Falsification then does not hit a specific hypothesis. When a prediction is proved wrong, scientists have to make some change in the premises but no specific hypothesis is logically targeted by the refutation. Consequently, as Quine emphasised:

> any statement can be held to be true come what may, if we make drastic enough adjustments elsewhere in the system...Conversely, by the same token, no statement is immune to revision (1953, p.43).

In fact, scientific theories are rarely as simple as the swan example and so a falsified prediction usually contradicts a far bigger set of premises than just two, complicating still more the question of which premise is at fault.

Indeed, as Kuhn (1962) and Lakatos (1970) have highlighted, predictions in science are typically derived from several theories. Kuhn, as was mentioned in the last chapter, describes scientists as working within a "paradigm". Lakatos has a similar concept of "scientific research programme", a unit consisting of a so-called "hard core", the central theories such as Newton's laws, and a "protective belt", comprising the auxiliary theories which are needed to link the hard core to empirical observations. The latter are described as "protective" because, when faced with falsifying evidence, scientists will generally revise them rather than the central theories.

When we apply Popper's ideas on falsification to a paradigm or a scientific research programme, we run into serious problems.

Popper claimed that "scientific" theories, unlike

"non-scientific" ones, were falsifiable. However Lakatos and Kuhn make the point that some of the most highly valued scientific theories would be classed as unscientific by Popper because, on their own, they are not testable: "exactly the most admired scientific theories simply fail to forbid any observable state of affairs" (Lakatos,1970, p.100).

Putnam illustrates this point with Newton's theory of universal gravitation. This is a law which specifies the force every body exerts on every other body but:

> this theory does not imply a single basic sentence! Indeed, any motions whatsoever are compatible with this theory, since the theory says nothing about what forces other than gravitations may be present. The (gravitational) forces are not themselves directly measurable; consequently not a single prediction can be deduced from the theory, (Putnam, 1980 p.358).

Testing such an abstract theory only becomes possible when it is connected with lower level theories. In Lakatos's terms, it forms part of the "hard core" of the Newtonian research programme. While the research programme is "progressive" or the paradigm is successful, scientists assume that the core theories are correct; any falsification is not seen as challenging them. Counter-examples are dealt with either by altering one of the lower level theories or merely by leaving it to one side as an anomaly while the more fruitful aspects of the theory are explored.

This version of how scientists treat the most highly valued theories is a serious challenge to Popper's account of science. In the face of these criticisms, Popper gave up his initial claim that theories can be judged scientific by

seeing whether they are logically falsifiable. He modified

his position to the claim that scientists should treat

their theories as falsifiable. They could do this, he

suggested, by being willing to state in advance what

evidence will make them give up a theory:

> CRITERIA OF REFUTATION have to be laid down
> beforehand: it must be agreed which observable
> situations, if actually observed, mean that the theory
> is refuted (Popper, 1963, p.38).

Returning to Popper's original goal of demarcating

science from what he considered to be the pseudoscience of

psychoanalysis, he claims that psychoanalysts are not

scientific because they are not willing to propose any such

criteria:

> what kind of clinical responses would refute to the
> satisfaction of the analyst not merely a particular
> diagnosis but psychoanalysis itself? (Popper,1963,
> p.38, footnote 3).

Lakatos argues that this does not differentiate

psychoanalysts from scientists:

> but what kind of observation would refute to the
> satisfaction of the Newtonian not merely a particular
> version but Newtonian theory itself? (Lakatos, 1970,
> p.101).

Sheldon (1978, p.14) draws on Popper's argument to

criticise social work use of psychoanalytic theories which,

he complains, have "built-in defences against disbelief."

The ability of supporters to deal with all counter-evidence

without altering the central ideas he condemns as a

"theoretical sleight of hand." But we can direct Lakatos'

argument at Sheldon himself and ask, in relation to the

research programme that he admires, namely behaviourial

learning theories: what results would lead a behaviourist

to give up not merely one hypothesis but the entire

approach?   In his own textbook on behaviour modification
(1982), Sheldon does not tell us what results would make
him give up this approach.   He does not seem to regard a
single refutation as making him question his confidence in
the central learning theories. But in failing to live up to
Popper's prescriptions, Sheldon is in line with most
scientists.

Popper's account of how scientists deal, or should
deal, with refutations runs into difficulties because, when
a prediction from a complex set of hypotheses is found to
be wrong, deductive logic cannot pinpoint the defective
premise.   The Bayesian approach can however offer an
account of how scientists decide which hypothesis to give
up or modify. Howson and Urbach (1989, p.97) argue that,
when a set of premises have been falsified, it is possible
to   determine "which hypothesis suffers most in   the
refutation".   Starting with the prior probabilities of the
theory and the auxiliary hypotheses individually,   it is
possible, using the probability calculus, to determine the
posterior probability of each,   given the   falsifying
evidence.   Differences in the prior probabilities lead to
sharply asymmetric effects on the posterior probabilities
in the light of falsifying evidence.   The   authors
illustrate this with an example where the prior probability
of the theory, $P(t)$, equals 0.9 and that of the auxiliary
hypothesis, $P(a)$, equals 0.6. The posterior probabilities
are strikingly different: $P(t/e) = 0.8787$ while $P(a/e)$ =
0.073.   The probability of a has been markedly reduced
while that of t is only slightly affected.   A substantial

difference is found even if the prior probabilities are much closer. If P(a) is kept at 0.6 and the P(t) is reduced to 0.7, the posterior probabilities work out as P(t/e) = 0.65 and P(a/e) = 0.21. Given results like these, a scientists's decision to keep the theory and modify the auxiliary hypothesis is reasonable.

Hence the behaviourist (or psychoanalyst) who has a single therapeutic failure and decides that it challenges the assumptions made in that particular application rather than rather than the learning (or psychoanalytic) theories themselves is not irrationally protecting a pet theory but acting reasonably given his assessment of the relative support for the theories and the auxiliary assumptions.

For Popper, the hallmark of a scientist is willingness to reject a theory in the light of the evidence. Lakatos, with his talk of scientists "protecting" a theory, makes it sound as if scientists are as dogmatically attached to their theories as Popper's analytic friends were to Freud. This is wrong but interpreting falsifying evidence, though a rational process, is not as simple as Popper suggested.

Let us return to Sheldon's initial concern: social workers' indifference to refutations. In Popper's critical account of his analytic friends who thought they "saw confirming instances everywhere", he gives the impression that, to an inductivist, their observations would be genuine confirmations and that only his radical revision of scientific method can discredit them. This is not so.

On the Bayesian account, the degree of support evidence provides for a theory is relative to its

improbability given background knowledge alone. If e is predicted by the theory but is otherwise highly unlikely then it provides strong confirmation. But in the case of psychoanalysis, such strong support is hard to find. The theories do not on the whole permit specific predictions. Inferring precise consequences from the theories is complicated in particular by the conjectured role of "defence mechanisms" such as projection and denial whereby aspects of the unconscious which are unacceptable to the conscious mind are converted into a tolerable form. Hence it can for example be predicted that an unresolved Oedipal conflict will manifest itself in overt behaviour but its specific form cannot be specified. Both a display of anger and one of affection towards one's father might, for instance, be interpreted as evidence for the underlying conflict. Indeed the range of behaviour consistent with the hypothesis that someone has an unresolved Oedipal conflict is so great that, as Popper complained, whatever happens is consistent with it. Such evidence however does not, on a Bayesian account, support the theory. The probability of a particular item of behaviour is no greater given the psychoanalytic hypothesis than it is in general: the P (e/h) is equal to P(e/k).

The vague goals of many social workers present a similar obstacle to testing. Bayes theorem endorses Sheldon's complaint that fieldworkers cannot test their hypotheses while they continue to express their aims in imprecise terms. The problem with a vague prediction is that any of a wide range of results can be fitted into it.

Suppose the goal of working with a depressed client is loosely specified as "improved social functioning", then any sign of new behaviour might seem to the social worker evidence of improvement. But the probability of some change in behaviour is high whether or not the social worker is being effective so observing one small change does not offer much support to the hypothesis that the intervention is producing improvement.

Conversely, if a fieldworker makes a precise prediction such as: "the client will start to take her children to school", something which she has been unable to do for months, the prior probability of this happening without any help, $P(e/k)$, would seem lower than its likelihood if the intervention works, $P(e/h)$. If e is then observed, confidence in the hypothesis is substantially increased. Unless a theory can be tested in this way it cannot gain much empirical support.

In summary, Sheldon rightly condemns social workers' indifference to counter-evidence as unscientific. I disagree only with his use of Popper's philosophy of science to endorse his argument. This attempt to provide an account of science without any inductive reasoning has been shown to have serious faults. The Bayesian account on the other hand not only sustains Sheldon's criticisms but also provides a clear account of the impact of refutations in science.

RELATIVISM AND THE SOCIAL CONTEXT

For empiricists in social work there are distinct areas of decision-making in fieldwork. Social workers need to, first, evaluate theories and decide which, if any, is the most probable. In this area, scientific methods can help them. Secondly, they need to decide on the goals of their intervention. Then, in the light of these two decisions, they must judge which of the possible ways of achieving them is best. Here several other factors are important as well as scientific evaluation of the rival methods. Relativists however merge these areas and claim that both deciding what to do and how to do it is entirely determined by social and psychological factors.

Let me begin by clarifying the role empiricists would ascribe to scientific methods before considering the relativists' criticisms of it.

For scientists appraising rival theories, the decision is typically between theories which are rivals in the sense that they are mutually inconsistent: if one is true, the other is false. Rival theories of this sort occur in social work, a classic conflict being between psychoanalytic theories and behavioural ones. However many theories in social work are consistent and complementary. Human behaviour is generally thought to have a complex causation and different theories often focus on different strands of causation. For example, a theory which connects juvenile delinquency to family dynamics usually claims to identify a significant cause but by no means the only one.

It is compatible with theories explaining delinquency in terms of other factors such as biological processes or peer group pressures. These various theories suggest different ways of helping: focusing, for instance, on the family, the physical health or the peer group. Empirical research can help us learn about the probable effectiveness of different strategies but this alone does not determine what social workers should do. Even if research shows that a particular approach is very effective, consideration of its cost or political acceptability may lead the social worker to choose an alternative course of action. In deciding which theory to draw on as the basis for action, social workers face a complex decision.

Researchers have been censured for ignoring the essential role of values and overstating the scope of scientific evaluation in social work. Raynor accuses empiricists of having "one eye closed" (1984, p.1) because, he argues, while empirical research "may help to improve the technical efficacy of methods, it can tell us little about the desirability of the goals towards which our methods are directed, or the social functions they serve." In an article very critical of social work researchers, he suggests that fieldworkers do not use research because they "experience research as existing on some other plane, irrelevant to their real concerns" (1984, p.2). Researchers, he claims, concentrate "on technical discussion of means at the expense of consideration of ends" (Raynor, 1984, p.7). As evidence he cites two books by Joel Fischer, one called "The Effectiveness of Social

Casework" and the other "Effective Casework Practice". As their titles suggest, both books focus on questions of effectiveness, not on the social and political context of social work or on moral issues.

His accusation that Fischer and other empiricists have one eye closed — that they are blind to the complex issues of values— is however unfair. It is true that many researchers do emphasise the issue of effectiveness but this does not mean they exclude moral issues. The reason they concentrate on evaluation is simply that this is their area of competence and they do not claim to answer all questions in social work. Textbooks on research for social workers are quite clear about the scope of their subject. Tripodi (1984, p.1) begins by stating that evaluative research can provide "knowledge about the extent to which practitioners have achieved their objectives". Reid and White (1989) devote a chapter to the <u>contribution</u> that research can make, suggesting that it only a part of decision-making in social work. But that contribution is important; checking the accuracy of theories and the effects of your efforts is or should be a major concern of fieldworkers.

Even this necessary but limited role of research is denied though by many of those who adopt a relativist view of knowledge. It appears to be increasingly believed in social work that the criticisms of positivism have destroyed all of science's claim to epistemological advantage over intuitive approaches. For the relativist, scientific methods are seen as belonging to a culture which

is neither better nor worse than humanist cultures but certainly less popular in social work. Advocates of empirical research are depicted as "imperialists" (Paley, 1987) trying to extend their sphere of influence by attacking fieldworkers with phoney arguments about the need for empirical evidence and reliable evaluation.

Paley (1987) is one of the most eloquent in attacking the naturalist movement in social work. He also provides the most detailed background to his relativism. He bases his rejection of empiricism on a relativist interpretation of Kuhn's "incommensurability thesis" and Wittgenstein's theory of meaning. His argument has two strands. First, the claim examined in the preceding chapter that there is no common language and hence no shared facts against which to compare theories. Secondly, the contention that there are no shared criteria of rationality to use in judging rival theories.

Paley urges the humanist majority in social work to see the value assigned to scientific knowledge as based on cultural rather than epistemological factors. He restates the naturalist/humanist debate in terms of a power struggle between the academic world and fieldwork and incites practitioners to defend their position. He maintains that the criticisms which naturalists level at current fieldwork are based on scientific criteria but fieldworkers generally do not share their scientific beliefs and therefore need not take such criticisms seriously. I imagine that to Paley, an academic's advice to social workers to look for independent, empirical evidence of their intuitive

judgements is like a Moslem encouraging a Christian to attend the mosque.

Paley's relativism has far-reaching consequences for social work. He rules out the naturalist's hope of ever developing a body of knowledge or of being able "to identify anything that could be 'taught to future generations of social workers'" (1987, p.182). He suggests that the only useful research is sociological not evaluative, finding out what social workers do and how they appraise their work but not judging it by scientific standards since there are no rational grounds, he claims, for considering them to be a more reliable check than fieldworker's own judgements.

Howe (1987) also illustrates the extensive repercussions of adopting relativism in social work. He has written an introductory text on social work theories for students which, taking a relativist line, relegates scientific methods to being one approach among many equally valid. Therefore in presenting theories to students he does not consider any questions about their truth or probability. The decision about which theories to use is left to the individual student who "pays her money and takes her choice" (Howe, 1987, p.166). Her decision though, Howe thinks, will be influenced by her social context: "theories", he claims, "emerge as products of their time and place" (p.167).

The aim of Howe's book is to help students make an informed choice between theories by clarifying the political nature of the type of practice they lead to and

offering some guidance on their psychological appeal.

Howe classifies theories on two dimensions: first, on whether they lead to social work interventions which help to maintain or to challenge current social structures. Those, for example, which imply that problems can be resolved by altering individual or family behaviour require no changes in wider society whereas those which explain clients' problems in terms of social forces imply that what is needed is radical change in society rather than in the client.

The second dimension Howe uses to characterise theories is whether they take, as he terms it, an objective or subjective approach to understanding human actions. This division reflects the classic (and, as I have argued in this thesis, inaccurate) positivist/humanist split between studying observable behaviour and mental phenomena. Objective, scientific study, Howe claims, sees Man as "plastic":

> we are essentially passive creatures. We are natural
> and determined phenomenon just as much as the rocks,
> plants and animals around us. Our behaviour is
> therefore predictable in given situations. Plastic
> Man is programmed and can be conditioned (p.27).

The subjective approach in contrast studies the meaning of actions, what people think and feel:

> with the subjective approach, human nature enjoys free
> will. Hollis (1977) calls this self-determining
> individual 'Autonomous Man'. Such a person
> interprets, reflects, plans, decides, acts
> intentionally and is responsible for his choices.
> (p.29).

Putting these two dimensions together produces four categories of theory each of which Howe links to a type of social worker:

1. "the fixers", objective social regulators, theorists in the scientific tradition aiming at maintaining social structures.

2. "the seekers after meaning", subjective social regulators, humanists encouraging the individual to find his own solutions through re-interpreting his experience.

3. "the raisers of consciousness", subjective reformers, similar to the last category but encouraging a radical re-interpretation, such as some feminist therapies.

4. "the revolutionaries", objective reformers, Marxist social work.

While presenting social work students with the choice between these categories, Howe does not address the question which would be most useful to them, namely which, if any, of the theories are true or probable. Our values may lead us to prefer one theory and to <u>hope</u> that it is true or that a therapeutic method will be effective but the world does not necessarily conform to our wishes. The "raisers of consciousness" may challenge the social system <u>if their efforts have their intended effect.</u> The theories Howe calls the "fixers" will only help maintain social stability <u>if they work.</u>

Howe does touch on these questions but in doing so he is inconsistent. In spite of presenting them as a feature of some theories rather than a way of testing theories in general, Howe does think they have a particular merit: they lead to more effective ways of helping people. In describing changes in medical theories, he asserts that later theories are better because they "allow more

efficacious treatments" (Howe, 1987, p.11). Similarly in

discussing behavioural social work, he says:

> if social workers want to be effective they have to
> find effective cures. The rigorous and exacting
> methods of science will help social workers identify
> treatment procedures that lead to behavioural cures
> (p.59).

But who does not want to be effective? No social

worker is interested merely in studying people. The

Marxist, feminist, or Rogerian social worker all want to

act and to have some impact on the problems they have

identified. Howe's views are puzzling. Either he

mistakenly thinks of science as a narrow, positivist

discipline which cannot be extended to the subjective world

of mental experience or he holds the relativist view that

scientific method is only one way of reasoning with no

epistemological merit - but this is inconsistent with his

belief that it produces more effective methods of helping.

Whatever his thinking, Howe has produced a book for

students in which he acknowledges the practical importance

of theory choice in social work since they may lead to the

client being offered a different type of help. And yet he

leaves such a momentous decision to the individual

presenting the options at times in frivolous language,

contrasting the "car maintenance manuals" of behaviourism

with "Freudian who-done-its" and "political thrillers"

(p.94). He gives no essential role to testing theories and

evaluating outcomes is reduced to a personality trait

rather than a rational process. He even appears to see the

desire to be effective as a quirk not a universal feature

of social work.

Relativism as promoted in social work has far-reaching practical implications. Kuhn's philosophy of science is appealed to by almost all to justify their relativism but, I shall argue, it does not provide a good basis.

KUHN'S PHILOSOPHY OF SCIENCE

Kuhn's study of the history of science, as has been mentioned, led him to develop the concept of a "paradigm". Therefore the issue of theory choice is, in Kuhnian terms, how do scientists choose between paradigms and under what circumstances do they abandon one.

Kuhn's account of scientific practice is similar to the Bayesian one but he offers a very different rationale for it.

The account of paradigm choice has to be put in the context of Kuhn's distinction between two periods in science: "normal" and "revolutionary" times. Most of the time, scientists in a particular subject work within a common paradigm. Unlike Popper's picture of scientists striving continually to falsify their theories, Kuhn argues that, in a period of normal science, the paradigm is generally accepted without question and counter-evidence is always interpreted as refuting some auxiliary hypothesis. The Bayesian account says that the basic theories are tested by the experiments but, because of their greater probability, scientists rationally decide that falsifications challenge lower level hypotheses. Kuhn describes the situation more as a gentlemen's agreement to

assume the truth of the core ideas. Scientists, he says, are not trying to test the paradigm but to extend and improve its explanatory power. When they apply it in a new domain, they are involved in what he calls "puzzle-solving", using this term to indicate that they assume the truth of the paradigm and try to make it fit the data. Any falsifications will be seen as indicating a fault in the auxiliary theories. The scientist's aim is to think of adaptations in these lower level theories which will make the paradigm and the evidence consistent. The challenge in normal science is to the ingenuity of the scientist rather than the truth of the core assumptions of the paradigm:

> if it [the paradigm] fails the test, only his own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather then the current theory which is tested (Kuhn, 1970, p.5).

If scientists cannot devise a modification of lower level assumptions in the light of a falsification, they will not necessarily look critically at the central assumptions of the paradigm. They will probably leave the awkward result on one side as an anomaly and concentrate on areas in which the paradigm continues to be fruitful.

However, at times of "revolution", the paradigm is reappraised. Confidence in it is weakened perhaps by an accumulation of anomalies. People start to question its accuracy or ability to deal with the phenomena; they look around for alternative ideas; possibly then a rival paradigm is created. Some scientists will opt for the new paradigm and then a revolution will take place. At the end

of it, one of the paradigms will have gained ascendancy. A new period of normal science then begins within the victorious paradigm.

In Kuhn's early work, the decision to switch allegiance from one paradigm to another does not seem to be a rational judgement. Comparison of the rival paradigms on rational grounds is impossible for two reasons. First, as discussed in the previous chapter, he considers that the meaning of all terms changes with paradigm change. This means there is no common body of empirical evidence against which the paradigms can be judged. He therefore rules out the usual scientific practice of judging one theory better than another because it provides a better account of what we have observed.

Secondly, he claims that standards as well as meanings vary between paradigms; there are no independent criteria of what counts as a good explanation and hence comparisons cannot be made. Accepting a paradigm involves judging that it is better than its rival but, although matters of evidence and logic will influence this judgement, it is ultimately due to the psychology of the individual and the dynamics of the scientific group to which he belongs:

> as in political revolution, so in paradigm choice –
> there is no standard higher than the assent of the
> relevant community. To discover how scientific
> revolutions are effected, we shall therefore have to
> examine not only the impact of nature and of logic,
> but also the techniques of persuasive argumentation
> effective within the quite special groups that
> constitute the community of scientists (Kuhn, 1970,
> p.94).

To most of his readers it has seemed that Kuhn is claiming that paradigm choice is not rationally based.

Lakatos provides a typical reading:

> each paradigm contains its own standards. The crisis
> sweeps away not only the old theories and rules but
> also the standards which made us respect them. The
> new paradigm brings a totally new rationality. There
> are no super-paradigmatic standards. The change is a
> bandwagon effect. Thus <u>in Kuhn's view scientific</u>
> <u>revolution is irrational, a matter for mob psychology</u>
> (Lakatos, 1970, p.178).

Applying these ideas to rival methods in social work,
relativists claim that choosing between say behaviour
modification techniques and client-centred therapy cannot
be based on a rational assessment of their relative
effectiveness. Behaviourists may point out that their
approach is supported by a greater weight of empirical
evidence but relativists such as Howe would maintain that
they are judging by a criterion which is internal to their
behavioural paradigm.

Kuhn (1978, Chapter 13) has strongly objected to this
interpretation of his writings and denies saying that
theory-choice is irrational: "reports of this sort manifest
total misunderstanding." The point he was trying to make
in his earlier work, he says, is that although there are
scientific criteria for theory-choice, they do not
determine a specific answer which all rational scientists
must accept. It is possible to reject a new paradigm
without being irrational or unscientific. He cites his
argument in <u>The Structure of Scientific Revolutions</u>:

> lifelong resistance (to a new theory) ... is not a
> violation of scientific standards... Though the
> historian can always find men — Priestley, for
> instance — who were unreasonable to resist as long as
> they did, he will not find a point at which resistance
> becomes illogical or unscientific (Kuhn, 1970, p.320).

He does not, he claims, think that all standards vary
between paradigms. On the contrary, his study of the
history of science has shown there is considerable
consistency in how different scientists in different ages
and paradigms judge theories. He agrees with the
empiricist that it is possible to identify criteria which
are constant. He lists five such criteria "not because
they are exhaustive, but because they are individually
important and collectively sufficiently varied to indicate
what is at stake":

> first, a theory should be accurate: within its domain,
> that is, consequences deducible from a theory should
> be in demonstrated agreement with the results of
> existing experiments and observations. Second, a
> theory should be consistent, not only internally or
> with itself, but also with other currently accepted
> theories...Third, it should have broad scope: in
> particular, a theory's consequences should extend far
> beyond the particular observations, laws, or
> subtheories it was initially designed to explain.
> Fourth, and closely related, it should be simple,
> bringing order to phenomena that in its absence would
> be individually isolated ....Fifth, a theory should be
> fruitful of new research findings (Kuhn, 1978, p.321).

Given these changes in Kuhn's later work, the case he
is trying to present seems very close to that of an
empiricist. One difference he would claim though is that,
while the empiricist considers that the criteria for
theory-choice are rationally justified principles, Kuhn
thinks that they are values which are influential only
because of their general acceptance:

> though the experiences of scientists provides no
> philosophical justification for the values they deploy
> (such justification would solve the problem of
> induction), those values are in part learned from that
> experience, and they evolve with it (Kuhn, 1978,
> p.335).

Even this apparent difference from empiricists

disappears if we accept Newton-Smith's argument (1981, p.114). He complains that Kuhn is muddled in his thinking here in linking the justification of induction in general to the specific issue of justifying scientific principles. He argues that if Kuhn is basing his claim that science is non-rational on a general scepticism about induction then the claim follows immediately from this scepticism and all the complex argument of his book is redundant. If on the other hand he is claiming that science is not rational because particular principles are not justified then to go on to describe them as grounded in experience is contradictory since this is equivalent to saying that they are inductively justified.

In the light of all these points, Kuhn's final position appears to be empiricist. The biggest difference remaining between his and the Bayesian account is that whereas Kuhn only describes (rather vaguely) some of the principles scientists use in weighing the evidence, Bayesianism provides an explanation for them in terms of the probability calculus.

Kuhn contests the charge of being a relativist and claims that those who use his philosophy to attack the scientific enterprise misunderstand him. Therefore his philosophy provides a poor foundation for those who want to reject scientific methods in social work.

CONCLUSION

Theories taught to social workers and applied in
social work are influential in that they affect the type of
help offered to clients. Their truth or falsity is
therefore of great significance. At present, decisions
about which theories to accept as the basis for action are
mainly left to individual fieldworkers who, as I discussed
in Chapter Six, appear to reach those decisions on the
basis of limited and often biassed evidence.

In contrast, scientists judge theories according to
the empirical evidence for or against them. Scientific
methods however do not provide a way of decisively proving
or disproving a theory; science does not offer conclusive
certainty. It does though enable us to judge the
probability of our theories. By deducing empirical
consequences and checking whether they are true or false,
we collect evidence which supports or challenges our ideas.
I have presented a Bayesian account of how theories are
appraised in the light of the evidence. The more support a
theory collects, the more confidence we have in it.

While social workers hostile to science have
traditionally accepted its merits but argued it cannot be
extended to the study of human actions, there is a new and
apparently growing movement which questions the rationality
of science and hence its desirability as a model for social
work. Often based on a reading of Kuhn's philosophy which
the author himself repudiates, these "relativists" claim
that scientific methods have no epistemic validity,

reflecting only the values of a particular culture or paradigm. They quite rightly stress the importance of values in social work but overstate their case. In deciding which theories to use, social workers are partly guided by values. They need to decide which problems they should deal with, and which ways of helping are morally, politically, or economically acceptable. Scientific methods cannot answer these questions but they can help social workers judge whether the course of action they finally choose has its intended effect or whether it leads to unintended and undesirable consequences.

Some present relativism as an apparently pragmatic and liberating view. Heineman (1981) talks of choosing whichever theory is best in the circumstances. Wilkes (1981) suggests using anything that works. Fieldworkers often use similar phrases to describe a down-to-earth, practical (non-scientific) approach. But these sentiments are truisms. Of course we want to use the best or most effective methods. But the question which they leave untouched is how are we to decide? As I have argued throughout this thesis, we need scientific methods to help us reach a well-grounded judgement.

CHAPTER NINE

THE SCIENTIFIC SOCIAL WORKER

INTRODUCTION

The previous chapters have examined the debate about the role of scientific methods in social work and have argued that social workers can and should adopt a more scientific approach. This chapter discusses how social workers can incorporate scientific methods into their practice and considers what changes this would require in the currently pre-dominant style of working.

Large scale, controlled trials of social work practice have, to date, been the most prominent example of a scientific approach in social work. In recent years though. their utility has been questioned. There is widespread agreement that, at present in social work, the first priority is to get a more precise account of what social workers are doing. The first section of this chapter discusses the reasons for this conclusion.

In the remainder of the chapter, I shall focus particularly on how individual fieldworkers can use scientific methods in their work. The "single case study design" offers a way of encouraging social workers to make their reasoning explicit and to include scientific methods in their day-to-day tasks of making assessments, providing help, and evaluating their efforts. In my discussion, the

stages of the case study are analysed in relation to the changes needed in current fieldwork practices if social workers are to incorporate this method into their work.

LARGE SCALE AND INDIVIDUAL RESEARCH

In the 1960s and 70s particularly, the large-scale controlled trial was considered by researchers the most reliable source of evidence on the effectiveness of social work methods. Fieldworkers though have generally been hostile to this form of evaluation which has, for the most part, produced mainly negative results on the effectiveness of social work, results which practitioners believe (or hope) are an inaccurate measure of their efforts. But the value of conducting further trials at present is now being questioned by researchers, because of doubts not about the validity of the results but about their usefulness to social workers.

For example, the social work service evaluated in the many studies carried out in the U.S.A. is poorly defined. We know that it ranged over individual, family, and groupwork methods. In some cases, the social workers offered predominantly a counselling service; in others this was combined with practical help and advice. Unlike most British social workers who have difficulty in specifying which theories, if any, they are using, the American social workers mainly reported that their counselling was based on psychoanalytic theories. But this degree of description is of very limited value. Within each category, social

workers may vary a great deal in what they actually do. For example, "psychodynamic casework" does not refer to a single theory or a specific therapeutic method. There is an ever-increasing number of different analytic theories. Even use of the same theory does not imply a uniform therapy; there are many therapeutic techniques which each social worker might have used.

This lack of detail is a serious problem in using the results; it is difficult to know precisely what to avoid as a result of these trials. Sheldon points out the unsatisfactory nature of these studies and complains that:

> something long-term, predominantly verbal and vaguely psychodynamic" is in many studies our best understanding of what we probably shouldn't invest in again (Sheldon, 1986, p.231).

Moreover the imprecision makes it difficult to rule out alternative explanations to the general conclusion that the negative results indicate that the social work service was ineffective. For example, Wood (1978) suggests that the global results might mask examples of both effective and damaging work, the differences being averaged out in the final result. This suggestion is plausible enough to warrant investigation but this, on the available evidence, cannot be done.

Another explanation of the negative results of these trials is proposed by Strean (in Fischer, 1976). Defending the psychoanalytic theories which underpinned much of the work, he maintains that the results are evidence of poor practice not poor theory. The problem, he says, is that "psycho-dynamic theory is poorly and fragmentally utilized,

abused, and misapplied by many if not most caseworkers in their practice". Again, the available evidence is not sufficiently detailed to check this claim.

Therefore, the primary task at present is seen to be the "important task of clearly identifying the nature of the input" (Sheldon, 1982b, p.8). In 1978, Joel Fischer, a leading researcher in the U.S.A., called for a moratorium on group experimental evaluative studies while researchers concentrated instead on building up understanding of the methods of helping, a view echoed by Wood (1978).

As the discussion on current practice in Chapter Two reported, developing a clearer account of social work is not just a question of asking fieldworkers because they tend to work in an intuitive way and have difficulty in reporting their methods or goals. The problem is how to help practitioners formulate their work.

In recent years several books and articles have proposed the "single case study design" as a possible aid (e.g, Fischer (1978), Jayaratne and Levy (1979), Tripodi (1983, Chapter 7), Sheldon (1983), and Reid and Smith (1989, Chapter 6). This is a research design which studies the improvement made in an individual case. Such a design is claimed to have two functions. First it provides some evaluation of the social work intervention and helps social workers to learn from their experience. Secondly, it encourages fieldworkers to adopt a more systematic, rigorous, and explicit approach to their work.

In brief, the single case study design requires the social worker to make an assessment of the client's

problem, state the goals of social work intervention, record what help is given, and measure what improvement, if any, is then observed.

This is very similar to the type of good, reflective, critical practice endorsed by CCETSW in its guidelines for training (discussed in Chapter 2). It also resembles the "case study" which is familiar to all social workers, especially students. The typical format of a case study also involves an assessment of the problem, a description of work carried out, and an appraisal of progress made. The "single case study" differs from the standard case study though in the degree of care taken in specifying and recording the various stages of the study. The CCETSW guidelines on social work practice state that one of the areas in which social workers must demonstrate competence is in "evaluating their work" (CCETSW, 1989, p.13). The single case study specifies how this can be done using scientific standards of evidence and reasoning.

My aims in the following discussion are twofold: first, to show what changes would be needed if the single case study were adopted by fieldworkers who currently work in a predominantly intuitive way; and, secondly, to show how the empiricist view of science defended in this thesis leads to a version of the single case study which can nullify the standard, humanist objections to its use.

FORMULATING PRACTICE WISDOM

The single case study design requires fieldworkers to make their reasoning explicit, providing an assessment

of the problem, their plan of intervention, and their goals.

The scientific approach is not alone in asking fieldworkers to be clear and explicit about their reasoning. Humanist writers have also argued for its importance. England (1986) for instance, although claiming scientific testing is inappropriate, wants social workers to provide detailed case studies so that their work can be critically examined by colleagues.

However studies show that few fieldworkers do articulate their work methods in much detail. In Sainsbury's study of clients' opinions of their contact with social workers, many reported "an uneasy lack of clarity" (Sainsbury, 1980b) about the social workers' aims and methods. This vagueness arises not only from social workers failing to tell clients what they are doing and why but also because they seem not to be clear even to themselves. Goldberg and Warburton's (1979) study of the work of a local authority Social Services Department, discussed in Chapter 2, reported that fieldworkers had difficulty in describing their work accurately and in saying with any precision what their goals were or how they hoped to achieve them.

The single case study requires three main categories of information: of assessments, plans, and goals. Let us consider each in turn.

Social workers appear best able to be articulate in making assessments of clients and their problems. Assessments are a standard part of record keeping and of

the many reports social workers have to produce for case conferences or legal proceedings. Goldberg and Warburton (1979) reported that fieldworkers found this aspect of their work easiest to talk about, often indeed offering an assessment when they had actually been asked for their plans and goals.

The single case study however demands a degree of precision often lacking in standard assessments. Specifically, it wants problems specified in sufficient detail to provide an initial measure, a "baseline report", against which the outcome can be checked. If the social worker is trying, for example, to help a mother organise the children's bedtime earlier and with less friction, a hazy impression that "the children often get to bed very late" needs to be made precise and when exactly they get to bed each night should be recorded. Then in judging the progress that has been made, any change and the amount of change can be seen. The hazy assessment can only produce an equally imprecise impression that bedtime has got earlier.

But how precise should one be? Clearly it is not necessary to state the exact second the child went to bed or the number of decibels it created in the process. I shall discuss this question later.

In formulating their plans, fieldworkers have even greater difficulty in providing a detailed account. It may be that imprecise accounts of their intentions are in some cases as accurate as is feasible; some fieldworkers, particularly in long term work, seem to drift into a fairly

aimless, friendly relationship with their clients. Fisher, Newton and Sainsbury's (1984) study of long term work with people who were or had been mentally ill supports this possibility. Despite close discussion, many of the fieldworkers were unable to specify their plans as more than a vague "supportive" relationship, hoping this would improve or help maintain the client's mental health.

Even such general phrases are of course a formulation of plans but they do not meet the requirements of the single case study. Again, the problem is that they are too ill-defined. We need some way of checking that the social work help being evaluated has in fact been provided but with such an imprecise phrase, it is difficult to judge whether or not a fieldworker is being supportive. A second function in using the single case study is to encourage fieldworkers to make their practice theory explicit not only for evaluation purposes but also so that other social workers can learn from successful cases; "provide a supportive relationship" is not a clear enough instruction for anyone to follow.

Asking fieldworkers to formulate their methods is not simply asking for a description of what they do. It is asking them to say which of the many things they do they think are causally significant in helping clients. There may be disagreement. Freudians for instance would consider that the content of their interpretations was the key factor but Rogerians would dispute this, arguing that it is not what they say but the relationship within which they say it that produces change. As the discussion in Chapter

4 reported, the task is similar to that of the scientist developing a theory and deciding which of the many perceptible patterns in the world are causally related. Science does not have a mechanical procedure for generating theories; there is no clear-cut process to teach social workers. Formulating the methods of working involves conjectures not just descriptions.

Turning to the question of goals, social workers show most interest in them in the context of debating what they should be, especially addressing the question of who should decide them, the client, social worker, or society. However that controversy is beside the point here. What matters is specifying the goals the fieldworker is working towards, irrespective of how they were determined.

Again current practice appears to fall below the standards of precision required by the single case study. The evidence from research, from Goldberg and Warburton (1979) and Fisher, Newton and Sainsbury (1984), is that social workers have only vague goals, particularly in long-term work. This provokes the question of how fieldworkers can evaluate their own work at all if they have only a faint idea of what they are trying to achieve. Evaluating fuzzy goals is problematic. What should count as, for example, "improved social functioning", a goal reported by several in Goldberg and Warburton's study? Equally important, what evidence would show the goal had not been reached?

As Polansky (1975, p.188) pointed out, choosing criteria by which to evaluate practice "requires a practice

theory which is willing to make commitments about what it is hoped will be achieved". For some social workers at least, the demands of the single case study are not simply that they should state their goals publicly but that they should start to set them.

A common feature of the discussion of assessments, plans, and goals has been the requirement for greater precision and, specifically, for formulations which are testable. The changing view on empirical evidence, discussed in Chapter 7, has radically altered the potential application of the single case study design.

TESTING

The issues concerning empirical evidence examined in Chapter 7 re-appear in the arguments about single case study designs. Again, both critics and some advocates of this type of study take the view that, to the scientist, only reports on behaviour not on psychological phenomena are acceptable as evidence. From this it is inferred that, to use this research design, fieldworkers must specify their assessments, plans, and goals in purely behavioural terms. Thus Ruckdeschel and Farris (1981, p.413) for example reject single case studies because their "key element" is measurement of behaviour. They suggest instead that fieldworkers who generally work within a humanist framework should write "qualitative" case studies which can include discussion of the client's and the worker's psychological experience. The only discussion among

critics of a scientific approach of how such studies could be evaluated is England's (1986) proposal that they be subjected to critical review by other social workers.

As I argued previously, however, this behavioural requirement is hardly appropriate in social work but neither is it necessary. In the form of empiricism I defended in Chapter 7, the distinction between qualitative and quantitative data becomes blurred. Brown and Harris' (1978) study of depression in women provided an illustration of how qualitative data, about the personal significance of a bereavement to each woman, could be turned into quantitative data for analysis. Evidence, on this view, should be judged not by whether it is behavioural but whether it is reliable and valid.

This view of empiricism seems implicit in the arguments of many recent advocates of the single case study who firmly deny that it is specifically tied to behaviourism and insist that it has far wider application: "it is quite compatible with the psycho-social approach, or reality therapy, or, for that matter, black magic" Baird (1976). Geismar and Wood (1982, p.269) suggest that the single case study design is only difficult to use if the underlying practice theory is fuzzy but this, they think, is a fault of the theory not the research design.

In this account of empiricism the positivists' sharp but inaccurate dichotomy between empirical and theoretical terms is rejected; there is instead a range of more or less reliable evidence. This is a view more easily incorporated into fieldwork where practical constraints often impede the

optimal collection of information. Manuals on single case studies therefore discuss in detail the factors which increase or reduce reliability.

Bias for instance is a particular source of inaccuracy when evidence is being collected by a fieldworker who has developed a close relationship with a client. The social worker may be biassed by a wish to see progress so that the slightest sign of progress appears to be a major improvement. Clients may also be biassed in the information they give the social worker. Some may feel grateful for the attempts the social worker has made to help and so understate any continuing difficulties. In view of social work's many statutory responsibilities, many clients can have good reason to deceive the social worker. Clients for example who are suspected of child abuse and fear that the children would be taken away if the full extent of the abuse were known may well be unwilling to say they have hit their child. In the case of Jasmine Beckford, her parents presented a very misleading picture to the social workers but the social worker and her senior, who had worked intensively to help the parents, were severely criticised by the inquiry (Beckford, 1985) for their "almost naive" willingness to believe everything Jasmine's parents said to them.

Mary Richmond, who published the first textbook for social workers, warned: "the danger that commonly besets case workers is that of becoming so fond of some particular hypothesis that it will seem [to them] in no need of proof" (1917, p.98). Recognising the risk of bias in their

personal assessments though, social workers should be advised to take care that they have been accurate in recording information and have checked it against other sources, considering what the client does as well as says, and getting the testimony of others involved.

But this check on bias is useless if fieldworkers have an unreasonable psychological conviction based on little or no evidence but just wishful thinking that they are right, leading them to look only for evidence to support their hypothesis rather than judging its probability in relation to the evidence. The essential role of refutations in scientific testing was discussed in the previous chapter. In arguing for a more scientific approach, Sheldon has rightly criticised social workers for taking a dogmatic approach and dismissing counter-evidence as irrelevant. Their errors though are not surprising given that they rely on intuitive rather than scientific methods of judging the truth or probability of their ideas. Intuitive reasoning differs from scientific in paying significantly less attention to counter-examples with a consequent bias towards confirming hypotheses.

It has long been recognised that people tend to notice instances that support their ideas more than evidence which challenges it. Bacon, in the seventeenth century, noted that:

> the human understanding when it has once adopted an opinion draws all things else to support and agree with it. And though there be a greater number and weight of instances to be found on the other side, yet these it either neglects and despises, or else by some distinction sets aside and rejects, in order that by this great and pernicious predetermination the authority of its former conclusion may remain

inviolate   (Bacon, 1620).

Research   in psychology supports   Bacon's   contention.
Nisbett and Ross,   reviewing studies on the issue,   reach a
similar conclusion:

> People have few of the formal scientist's skeptical or
> disconfirmatory skills.   Once formulated or   adopted,
> theories and beliefs tend to persist, despite an array
> of   evidence   that should invalidate or   even   reverse
> them.   When "testing" theories, the layperson seems to
> remember primarily confirmatory evidence and to ignore
> potentially disconfirmatory evidence.   When confronted
> forcibly with disconfirmatory evidence,   people appear
> to   behave   as if they believed that   "the   exception
> proves the rule"   (Nisbett and Ross, 1980, p.10).

There   is   also   evidence that people   are   better   at
recognising   the   relevance of confirmations than   that   of
refutations   and   that they tend   to   mistakenly   interpret
neutral   evidence as confirming.   A problem   developed   by
Johnson-Laird   and   Wason   (1970)   provides   a   well-known
demonstration.   Four   cards   are   laid out   on   the   table
displaying respectively E,   K,   4,   and 7.   People are told
that each card has a letter on one side and a number on the
other.   They are then asked to test the rule: "if a   card
has a vowel on one side,   then it has an even number on the
other",   by   turning   over at the   most   two   cards.   Most
recognise   that   the card showing a "K" is   irrelevant   and
spot   the importance of turning over the "E" to   check   the
rule.   But   over 90% of respondents miss the relevance   of
turning over the card showing "7":   if there is a vowel on
the reverse,   the rule has been refuted.   The vast majority
choose instead to turn over the "4"   although in fact it is
irrelevant   to testing the rule since either a vowel   or   a
consonant on the reverse would be consistent with it.

A conscious effort to test hypotheses would be a major
change for fieldworkers moving from an intuitive to a
scientific approach. The dangers of failing to look for
refutations in social work are highlighted in the inquiry
into child sexual abuse in Cleveland (1988). This provides
a well publicised example of social workers, and in this
case also doctors, treating an hypothesis as irrefutable,
and illustrates how this affected their subsequent
gathering and interpreting of evidence.

The inquiry was set up because there had been a
dramatic rise in the number of diagnoses of child sexual
abuse by two paediatricians in Cleveland. In five months
in 1987, mainly in May and June, sexual abuse was diagnosed
in 121 children from 57 families. Acting on these
diagnoses, social workers removed all 121 children from
their homes while further investigations and plans were
made. The paediatricians and social workers believed that
they had uncovered a major and hitherto unrecognised
problem but the scale of the action led to public disquiet
about the reliability of the professionals' judgement and
the wisdom of taking the children into care. In response
to this concern, the Secretary of State for Social Services
ordered an inquiry chaired by Lord Justice Butler-Sloss.

Among other issues, the report criticises the way
diagnoses of sexual abuse were reached and sustained,
looking especially at the diagnostic significance of the
"anal dilatation test".

The two paediatricians in Cleveland placed great
confidence in the reliability of this test in detecting

anal sexual abuse. A positive result on this test was seen, as the inquiry report critically comments, not as "grounds of 'strong suspicion'" but as "an unequivocal 'diagnosis' of sexual abuse" (1988, p.243) The social workers to whom the children were then referred seemed to have had equal confidence in it. The senior social worker who had most responsibility for organising the social work response to the referrals told the inquiry that "the possibility of misdiagnosis had not occurred to her" (1988, p.82).

Their critics however, in the first instance the children's parents and the police and, subsequently, the general public, thought this confidence was unwarranted. The validity of the test is strongly disputed in medicine at present with little research evidence. Moreover it is, in general, used only when suspicions of sexual abuse have already been aroused by evidence from the child or others. A positive result is then taken to give some support to the suspicion. In Cleveland however the test was carried out on children admitted with other medical conditions and so a positive finding provided the first suspicion of abuse.

This confidence in the test results, as one would expect, influenced all the subsequent actions; the diagnosis was taken as a proven fact rather than an hypothesis needing to be tested. Further investigations were made but not in the spirit of testing and possibly refuting the diagnosis but to assess the family and plan future actions on the assumption that abuse had occurred. The confidence was particularly apparent in the way they

responded to criticism and requests for more proof.

The police were the first to ask for further evidence to confirm the allegation of abuse, partly to help them identify and prosecute the offender and partly because the police surgeon, like many other doctors, considered the dilatation test was suggestive but far from diagnostic of abuse. Instead of paying serious attention to her criticisms however the paediatricians and social workers rejected them and excluded her from the investigations. Indeed the Director of Social Services went so far as to send a memo to all social workers which "directed the exclusion of police surgeons from examining children referred to social services for reasons of sexual abuse" (1988, p.65).

When second medical opinions were needed in the legal proceedings, the children were referred to the doctor who was known to share the Cleveland paediatricians' view on the reliability of the test and indeed who had taught them about it. Consequently the second opinion provided a check on whether the test was positive but did not question how the result was being interpreted by the Cleveland doctors.

Social workers were especially criticised in the report for not questioning the accuracy of diagnosis in the light of the allegation that further abuse had occurred when the children were in foster homes and having no contact with the alleged offender, the implication being that a foster parent was responsible. Butler-Sloss concedes that the first time suspicion of sexual abuse in a foster home arises, the Social Services Department should take the

issue very seriously, but when, in a six-week period, three more foster homes are implicated with the dilatation test providing the only evidence, social workers should have "raised questions about the validity of the diagnosis". The families had been through a detailed appraisal and investigation before being accepted as foster parents and, Butler-Sloss thought, the probability of all these assessments having been so seriously flawed should have been weighed against the probability of misdiagnosis.

The refusal to doubt the accuracy of the test was also apparent in the "disclosure work" that the social workers did with the children. The purpose of these interviews is to investigate a suspicion of abuse, using play materials to make it easier for the children to express themselves, for it is accepted that children who have been abused may well be reluctant to talk about it. Some people have claimed, controversially, that children never make false allegations of having been abused but logically at least the possibility arises. Therefore in the interviews four outcomes are possible: 1. the child who has been abused "discloses" it; 2. the child who has been abused denies it; 3. the child who has not been abused denies it; and 4. the child who has not been abused claims he has. The inquiry found that social workers considered only the first two options; they "worked from the presumption that the children had been abused" (1988, p. 59) — making the interviews a Kafkaesque experience for any child who had not been abused. Denial of abuse was interpreted as a psychological process of blocking out a traumatic

experience, to be overcome by asking leading questions. The report makes the serious complaint that: "those conducting the interviews seemed unaware of the extent of pressure, even coercion, in their approach" (1988, p.209).

Taking a positive result of the anal dilatation test as confirming sexual abuse with certainty was, as the Cleveland Report says, unwarranted, but as soon as some degree of fallibility is admitted, the predictive value of the test falls dramatically. Few people can recognise this intuitively but the statistics involved are uncontroversial; it is a major factor in deciding on the utility of introducing screening programmes for comparitively rare diseases such as cervical or breast cancer.

Campbell and Machin (1990, Chapter 3) provide a standard account. Beginning with some definitions: the "prevalence" of a disease, or in this case sexual abuse, is the frequency with which it is thought to occur in the population; the "sensitivity" of a diagnostic test is the probability that the test result will be positive if the disease is present; the "specificity" of the test is the probability that the result will be negative if the disease is absent. Bayes theorem can be used to calculate the positive predictive value of the test, that is, the probability that a person with a positive test result has actually got the disease.

If we call the probability of sexual abuse P(A), and the probability of a positive result on the anal dilatation test P(T), Bayes theorem states that:

$$P(A/T) = \frac{\text{Sensitivity x Prevalence}}{\text{Probability of positive result}}$$

$$= \frac{P(T/A)P(A)}{P(T)}$$

Providing values for the variables in the case of child anal sexual abuse is difficult in the present state of knowledge but we can make up some figures since my aim is to demonstrate how fallibility has an unexpectedly large impact on the predictive value of a test. Being generous to those who think the anal dilatation test is very good, let us suppose that the sensitivity of the test is very good at 90%, and that its specificity is even better - 95%. The prevalence of anal sexual abuse could be set at one in a hundred - perhaps an over-estimate but it has the advantage of simplifying the maths. Before calculating the probability that someone with a positive result has actually been abused, we need to work out the probability of a positive test result. This is where intuitive reasoning generally produces an underestimation of the figure: if used on a hundred children, the test will detect the one expected positive case of abuse but, given the error rate of its specificity, it will also give a positive result on 5% of the 99 children who have not been abused. Thus the total positives is 1 + 0.05 x 99 = 5.95 and the probability is 5.95/100 = 0.0595.

Using Bayes theorem with these figures then, the probability that a child with a positive result on the anal dilatation test has actually been abused P(A/T) can be calculated:

$$P(A/T) = \frac{1 \times 0.01}{0.0595} = 0.168$$

Where the paediatricians in Cleveland were acting as if the probability of abuse given a positive result was close to 1 (certainty), allowing even a small margin of error reduces the probability to as little as 0.168. Even if the specificity of the test is raised to 99%, keeping the other probabilities the same, its predictive value is still as low as 0.5, a fifty/fifty probability that the child has actually been abused.

Accepting the weak predictive value of the diagnostic test, the police response in Cleveland was more reasonable – taking the result as grounds for investigating the possibility of abuse but not collecting evidence with a presumption that the diagnosis was infallible.

Noticing and actively looking for evidence which tests one's theory is essential in science. Acting intuitively, people tend to be bad at this – noticing mainly the evidence that apparently supports their theory and turning a blind eye to counter examples. Social workers who rely on intuitive reasoning alone are also likely to overlook or underestimate the evidence that tells against their theories. The Cleveland case is an extreme example of social workers failing to treat their theories critically but it illustrates the pervasive impact this has on further investigations: on how critical questions are treated, and on what evidence is actively looked for, what is noticed, and how it is interpreted. There is still no

consensus on how many of the children in Cleveland had actually suffered abuse and perhaps there never will be. But the paediatricians and social workers have been criticised not for making inaccurate diagnoses but for having unreasonable confidence in the diagnoses - showing blind faith rather than a critical rationalism.

EVLUATING OUTCOMES

Suppose the results of research show that after receiving the social work service either the individual, in a single case study, or a group of clients, in a controlled trial, have markedly improved. What can we infer from these results? The problem is that we cannot simply reason that if X happens after Y then X happens because of Y. Chance correlations are common: a client may win the football pools after seeing the social worker but it is unlikely that his good luck could be attributed to social work skill. Similarly, a client's mental health may improve but is it due to the expertise of his social worker or would it have happened anyway? In evaluating the outcomes of research, the issues are when and with what degree of confidence can we infer a causal relationship between treatment and improvement.

The controlled trial is generally thought to be the most powerful way of establishing a causal connection. The group of clients who have received the service being evaluated are compared with a "control" group of people who are similar in all respects judged to be relevant except that they have not had the experimental help. If both

groups show equal progress then it suggests that the service is not causally significant. If however the experimental group does much better than the control then a causal connection seems probable.

Some critics of a scientific approach in social work have claimed that this is the only form of research that science can offer and that the desire to be scientific has hindered the development of understanding in social work by discrediting other forms of research. Smith (1987, p.406) blames "positivism" for making researchers concentrate on "outcome research" rather than studying the social work process. Heineman (1981, p.374) also blames the desire to emulate the natural sciences for what she claims is the prevailing view among researchers that studies which lack "experimental manipulation, control groups, and randomization" are "not good science".

The claim that science only endorses controlled trials is also used by Ruckdeschel and Farris in arguing for the qualitative case study over the single case design. Equating the latter with a behavioural approach, they call it "quantitative" research and claim it "is not adequate to represent the reality of most social phenomena" (1981, p.418). Since, they say, you cannot scientifically generalise from either type of individual study, the qualitative study is to be preferred because it at least can provide a more accurate account of fieldwork.

Science however does not reject single case research — it has indeed long been a respected part of medical studies — but there is some evidence that social work researchers

used to undervalue it. Shyne (1963) for instance, while commending the single study design for encouraging clear thinking, asserts that no inference about causal relationships can be drawn from it.

Nowadays though, as I reported earlier, this attitude has changed with many considering that single case studies are to be preferred at the moment to build up a better picture of social work practice. But how reliable is a causal inference based on just one case?

Any inductive inference can only conclude with attributing a certain probability to causal claims. The various strategies used in research are aimed at reducing the risk of fallacious reasoning; different designs can be seen as on a continuum. The single case study can be more or less reliable, depending on how it is done and on our background knowledge of the problem and treatment being studied.

Campbell and Stanley (1963), in an authoritative analysis of research methods, suggest that the power of research designs can be measured by "how many plausible alternative explanations they rule out or render implausible" (1963, p.35). This provides a useful way of examining the strength of the single case and the controlled trial.

One alternative explanation of improvement in a client that is often plausible is that it is due to "natural history", that is, the client would have improved at this time regardless of social work intervention. It is after all well established that many problems in life are short-

lived or cyclical. Large group controlled trials are designed to test the hypothesis of natural recovery by having an untreated control group whose "natural history" can be charted. It is though the most serious limitation of single studies that it is hard to render the natural history explanation implausible. However, background knowledge can help to judge the probability of this alternative explanation. Sometimes we have a fair understanding of the natural course of the type of problem being treated and so can predict the probability of a spontaneous recovery. In effect a control is being used but an historical one not a concurrent one. For instance, research has shown that a large majority of juvenile delinquents become law-abiding citizens when they reach their twenties, with or without social work assistance; obsessional fears however tend to be chronic.

This point, in Bayesian terms, is that the degree of support that a piece of evidence e (in this case, observing improvement) gives an hypothesis depends on how much more likely e is given the hypothesis than it is on background knowledge alone. From research, it is known that the probability of improvement in a juvenile delinquent is high with or without social work intervention while in the case of obsessional fears the probability of progress if untreated is low. Therefore observing improvement in a single obsessional case will strongly support the hypothesis that the treatment was causally significant but, with a delinquent, progress only slightly supports the therapeutic hypothesis.

Another explanation which increases the probability of improvement on background knowledge alone is that it is a general effect of receiving attention. This is often called a "placebo" response; the client improves irrespective of the type of help given. Such placebo effects are well documented in medicine (see e.g. Pocock, 1983), particularly in drug treatment. In controlled trials of drugs, this possibility can sometimes be dealt with by giving the control group a "placebo", a pill made from an inert substance and administered in the same manner, so that both groups have the same experience of being treated; it makes "patient attitudes to the trial as similar as possible in treatment and control groups" Pocock (1983, p.93). Social work trials sometimes give the control group a "placebo" therapy. The control group have meetings with a therapist in similar circumstances to the experimental group but the therapist is, in fact, untrained. In this way, it is possible to isolate the significance of the theoretical approach of the trained therapists.

In single cases, the placebo hypothesis can be tested by withdrawing the treatment for a while when progress has been noticed and then re-introducing it and seeing whether there is a difference between its presence and absence. This is the so-called ABAB design, the "A" indicating the non-treatment phase and the "B" the treatment. This approach is very good but it has ethical and theoretical limitations. There would be moral problems in deliberately withholding help and thereby probably causing distress if

the problem has been very distressing and, from other evidence, the improvement is probably due to the treatment. Theoretically the design can only be used if the practice theory predicts that withdrawal of treatment would quickly lead to a perceptible reversal of progress. Behavioural learning theory does so and the design has been mainly developed by behaviourists.

While the plausibility of a causal connection between treatment and improvement in a single study may be only moderate, it may be increased by comparing it with other single cases. In judging whether X causes Y, the frequency with which the correlation is seen strengthens the inference. Fieldworkers who use the single case study extensively may find that several cases show that a similar type of problem responds to similar interventions. This would increase the probability of a causal connection. If their work is clearly formulated, they will be able to judge if colleagues are working in similar ways with similar problems and then the potential pool of cases for comparison becomes much larger. If considerable fieldwork experience supports the claim that the intervention is effective then a large-scale controlled trial would be worth doing to diminish the plausibility of the "natural history" and "placebo" explanations of the observed improvement. But such trials would differ significantly from most of those done to date: they would be testing a well-formulated method of working which, if shown to be effective, could be learnt by other social workers.

The single case study has strengths and weaknesses. It

cannot refute all alternative explanations of why the client has improved though, in some cases, it can make them less plausible. In this respect it is weaker than a large-scale controlled trial. It is also though much stronger than the way most social workers currently practice where haziness about goals and methods makes it difficult to judge even whether the client has improved.

CONCLUSION

The aim of this chapter was to illustrate how social workers could use scientific methods based on an empiricist account of science. Both large-scale and individual research have been examined. The consensus view at present is that large-scale controlled trials have limited practical value while we have such inadequate knowledge of the social work service being evaluated. The prime scientific task in the current state of social work is to study the process of helping, to turn fieldworkers' implicit practice wisdom into explicit practice theories which can then be applied with confidence based on extensive evidence. The single case study design has been proposed for two reasons: it encourages social workers to formulate their thinking clearly, and it enables them to build scientific standards of evidence and reasoning into their practice.

Discussions of the single case study echo the philosophical debates examined in this thesis. Early exponents and its current opponents consider it is

necessarily narrowly behavioural in its application, claiming that all the terms in the theories need to be testable by behavioural evidence. On this view, the single case study design cannot be used in the predominant, humanist style of social work and few fieldworkers have adopted it. However, changes in philosophical assumptions about science have significantly altered the methodology of the design. The former dichotomy between the behavioural and the psychological, or "quantitative" and "qualitative" data as it is often described in social work, is replaced by the criteria for evidence of reliability and validity. Opponents' arguments for rejecting the single case study have therefore been seriously weakened.

The aim of incorporating scientific methods into social work practice has a long history. Mary Richmond's pioneering textbook for social workers called her approach scientific in that it emphasised a clear. critical, and logical approach throughout: "there can be no good casework without clear thinking" (1917, p.99). Likewise, in the guidelines for training, the regulatory body, CCETSW continues to endorse a picture of good practice as a clearly-focused, critical process in which the social worker has: "a knowledge of both the need for and the techniques of attaining effective evaluation of the service which has been offered" (1986, p.12).

CCETSW's guidelines however do not specify what the "techniques of attaining effective evaluation" are, nor do they acknowledge the continuing controversy in social work about the place of scientific methods in implementing this

guideline. The proposals presented in this chapter give an empiricist account of how CCETSW's goals can be achieved.

Despite the enduring emphasis in the social work literature on clear, critical work, research indicates that the typical style of contemporary fieldwork is radically different. Practitioners work intuitively and are reluctant or unable to formulate their work clearly enough to permit critical review or comparison with other efforts. Nor do they show much interest in evaluation, whether in keeping up to date with and using results of studies or in systematically appraising their own efforts.

Throughout the thesis, I have been arguing that using a scientific approach does not conflict with the humanist view of mankind predominant in social work. Nor does it entail fieldworkers rejecting the empathic and intuitive skills so highly valued at present. It does require them to recognise how fallible such understanding is and consequently the need to make "practice wisdom" explicit and subject it to independent testing. At first sight, this looks like only a request that fieldworkers move from the private to the public domain but, from the picture of fieldwork gained from research, it seems that for many this would also entail a substantial change in their way of working, moving from a fairly vague, uncritical, non-goal-oriented approach to a problem-focused, critical, and purposive style of intervention.

CHAPTER TEN

SUMMARY AND CONCLUSIONS

This thesis has set out to re-examine the long-
standing debate among social workers concerning the value
or possibility of using scientific methods to develop the
knowledge base of their profession.

I began by highlighting the public significance of the
knowledge debates in view of social work's growing
importance in modern society. Legislation has given the
profession increasing powers and responsibilities in
relation to the major client groups: children at risk, the
sick, the disabled, and offenders. These powers have been
invested in social work in the belief that social workers
have, or can have, special knowledge and competence in
dealing with personal and social problems, a belief
emphasised by the 1983 Mental Health Act which stipulated
that only social workers who had undergone training could
be authorised to implement the Act's legal powers.

Social workers themselves however have to decide how
to carry out their statutory roles; it is the profession's
responsibility to determine which skills, theories and
methods help them provide an effective service to clients.
My analysis of social work's theories and skills in Chapter
3 revealed a major conflict of opinion within the
profession as to whether social work is, or should be,

based mainly on personal skills or on theories tested according to the standard methods of science — whether it should be scientific or not.

The scientific approach has always been more strongly supported by academics than by fieldworkers. The pioneers of training, for example, thought that helping in social work could and should be based on more than just common sense understanding and practical services. Mary Richmond, a leading figure in American social work, looked to science for standards of investigation and reasoning, encouraging social workers to adopt a rigorous and critical approach to assessing clients' problems and making decisions about them. Others turned to science for theoretical understanding. They believed that the social sciences offered more accurate explanations of personal and social problems and so had the potential for providing more effective ways of solving problems.

The regulatory body, CCETSW, which nowadays lays down guidelines on the content of training, endorses this scientific tradition, emphasising the public and reliable nature of social work techniques; students are advised to work in a reflective, goal-oriented, and critical way, using theories and therapies in a systematic manner, and turning to research for further empirical evidence.

CCETSW portrays an unrealistic picture however of the development of social work knowledge in the oft-repeated claim that social work has "a common body of knowledge, skills, and values". The theoretical knowledge available is not "common" since students decide for themselves which

theories, if any, to use. Moreover it is not well enough supported to claim the title of "knowledge", at least not according to scientific canons. Students take courses in, among others, human growth and development, psychology, sociology, social administration, law, and social work theories. In most of these subject areas, they will learn many rival theories, few of which have been rigorously empirically tested. In particular, it is rare to find a theory which has been much tested in a social work context since the empirical research tradition is poorly developed.

The small number of empirical studies in social work may in part be due to the low esteem in which research is held by most fieldworkers, few of whom read or use its results. Rejecting scientific methods as inapplicable in social work, many fieldworkers adopt an individual and private style of working that appears at variance with the model endorsed by CCETSW. Research studies have consistently found that, once qualified, social workers appear to make little use of theories in any conscious or systematic way despite the fact that they report that theories are influential in that they have been absorbed into their background knowledge. Such studies have reported that the personal skills of empathic and intuitive understanding are valued more than social science theories or scientific evaluation.

In this thesis, I have presented both empirical evidence and theoretical arguments against this individual style of working. Evaluative studies have produced mainly negative results, an outcome which throws serious doubt

both on social work competence and on the reliability of fieldworkers' own assessments of their efforts. Inquiries into child abuse tragedies, where a child being supervised by social workers has been killed by a parent, are another source of evidence about social work practice. Fieldworkers in these cases were severely criticised for actions which seem to be typical of current methods of working rather than instances of exceptionally bad practice. For example, they were censured for their failure to have clear goals and plans, for overlooking or underestimating important sources of information, for failing to assess the reliability of sources of information, and for being ignorant of the relevant empirical research results.

In addition to this empirical evidence, I examined theoretical reasons for questioning the reliability and scope of empathic and intuitive skills.

The ability to empathise is problematic. First, there is the difficulty of judging whether or not empathic understanding is accurate. Secondly, its value in social work is restricted in two main ways: by the constraints on our ability to empathise, and, as empirical studies show, by its weak therapeutic power.

Two defects of intuitive reasoning were particularly emphasised. Intuitive judgements are especially vulnerable to distortion because of, first, bias due to the fieldworker's close involvement with the client, and, secondly, over-confidence due to a failure to look for or consider refutations and counter evidence. Both of these

factors tend to make social workers overlook evidence which challenges their intuitive reasoning and to over-estimate the weight of supportive evidence.

This examination of social workers' empathic and intuitive skills strongly suggests that current social work practices have serious defects, but the proposal that social workers should improve their reasoning by incorporating scientific methods meets with little enthusiasm.

Conflicting opinions on the merits of science have been an enduring feature of social work but the views of both naturalists (pro-science) and humanists (anti-science) have altered as assumptions about the nature of science have changed so that there have actually been not just one debate but several.

Changing assumptions in the science debates are strongly in evidence when we look at the debate surrounding psychoanalytic theories. They have had an important and long-lasting influence in social work but in the science debate, they have, at different times, been commended and criticised by both sides. When first espoused by social workers, they were hailed as the first scientific grounding for practice. Now, advocates of a scientific approach generally attack them, claiming either that they are unscientific because they are unfalsifiable or that their plausibility has been undermined by the weight of empirical evidence against them.

Opponents of scientific social work have also altered their view on psychoanalytic theories. Initially critical

because of the scientific claims, some have now concluded that psychoanalysis is not a science. They suggest psychoanalytic theories should be classed within the humanist rather the scientific tradition. Thus they should not be judged by the scientific criteria of truth or probability, standards by which they tend to fare badly, but by their "intelligibility", whether they help social workers make sense of their clients. However I presented arguments why this subjective standard was not adequate. Social workers' understanding has public consequences, influencing the way they carry out their statutory duties and exercise their powers. The truth or probability of the theories they use is therefore of public concern and should not be ignored, leaving the theories to be judged only by whether they help social workers to have a private sensation of comprehension.

The debate about psychoanalysis has been somewhat outside the main stream of the science debate in social work. The traditional social work objections to science have been directed at a behaviourist model of science. A major criticism has been that scientific methods, they claim, can be used only in studying behaviour and not in their area of interest which is is understanding mental processes. The methodological arguments for such a narrow model of science have now been generally discredited. In examining them, I argued not only that science can encompass theories about the mind but also that there are, in fact, strong similarities between social workers and scientists in the way that they try to understand

phenomena.

Few advocates of science in social work would now subscribe to such a narrow view of science. The picture is however somewhat confused by the fact that many naturalists do champion use of the therapies this methodology has produced — behavioural modification techniques. Their reason for doing so though is because of their effectiveness not because of any unique claim to scientific status. Nevertheless, the debate about science is still in practice often conducted between humanists and behaviourists although the premises of the latter's argument have altered.

Humanists also object to a less restrictive version of behaviourism which considers that science can study mental processes but that only behavioural reports provide the empirical evidence needed to test them scientifically. This view of science, which has been very influential in research, goes some way towards meeting social work criticisms but efforts to reduce or link all psychological terms to behavioural reports have, with some justification I think, been criticised as inadequate.

In recent years this type of behaviourism has also been criticised as unduly constrained. Its underlying philosophy — positivism — has been shown to have serious flaws. Awareness of this development in the philosophy of science has produced a new debate in social work. Those who endorse a scientific approach are becoming critical of the account of science they have championed and which has underpinned much of social work research methodology. At

the same time, a new argument against science is emerging.

Traditionally, social work opponents have accepted the merits of scientific methods but claimed that they cannot be transferred from the natural to the social sphere. Now a growing number have started to argue that, in overthrowing the positivist view of science, all claims to empirical status are also destroyed. Hence scientific methods have no epistemological superiority; they are valued highly in some cultures, particularly ours, but there is no rational justification of this perceived pre-eminence.

The consequences for social work if this view of science is accepted are far-reaching. Science, it is claimed, should no longer be held up as a model. Where I have criticised the reliability of the empathic and intuitive understanding which is such a major part of current practice and proposed scientific methods as a way of improving social work, relativists contend that practice wisdom should be seen as different but not inferior to scientific knowledge. Judging theories ceases to be based on empirical testing but on individual choice, Howe (1987, p.166) going so far as to say that the social worker "simply pays her money and makes her choice". Admittedly this is a reasonably accurate account of how fieldworkers judge theories at present but the relativist claims that the process cannot become more rational.

Thomas Kuhn was the philosopher most often cited on this subject, so my defence of empiricism rested on a critical examination of the interpretation of his work which claims that different paradigms have no shared

language (and so no common body of evidence) and no shared standards (and no common way of weighing the evidence), therefore there are no criteria by which they can be compared.

This radical change on the humanist side in the science debate in social work is matched by an equally fundamental development among the naturalists. The claim that only behavioural reports constituted empirical evidence was based on the now-discredited positivist distinction between observational and theoretical terms. Behaviourism has been dominant in social work research for many decades but there are growing signs that it is being replaced by a more liberal empiricism, in which reliability and validity are the criteria used to evaluate testing procedures. Examples of research demonstrated how researchers are showing imagination and skill in developing ways of testing complex psychological hypotheses.

The question of how scientists interpret the evidence has been a major issue in the philosophy of science. A major part of the relativists' argument which I criticised is the claim that the decision to accept or reject a theory in science is determined by social and psychological factors rather than the weight of empirical evidence. As an empiricist alternative to their view, I offered a Bayesian account of how scientists judge the probability of their theory in accordance with the probability calculus.

Having concluded that social workers could use scientific methods, my final chapter examined the practical implications of trying to incorporate them into the styles

of working which are typical at present. Although I have argued that adopting scientific methods does not entail any radical philosophical change in fieldworkers' assumptions about human nature, it does lead to substantial changes in the way they work, requiring social workers to formulate clear assessments, plans, and goals, to make efforts to minimise the risk of bias, and to test their ideas more deliberately and critically.

The history of the debates about science in social work shows an ever-narrowing gap between the typical fieldworker's concern with understanding intentional human behaviour and the scientific methods advocated by naturalists. The extreme behaviourism which excluded study of the mind has been shown to be unduly restrictive. The more satisfactory account of empiricism defended in this thesis, I consider, invalidates social workers' objections to using scientific methods. The implications that these philosophical developments have for research methodology are only slowly being worked out. But change among researchers is not enough since practitioners at present pay little attention to them. Fieldworkers need to stop rejecting an obsolete view of science and consider how empiricist scientific methods can help them develop a more reliable understanding and more effective ways of helping their clients.

APPENDIX A

DATA-COLLECTION SYSTEMS

(1) E.M. GOLDBERG and R.W. WARBURTON (1979)

(2) N. RAYNES, J. WINNY, and K. MULGREW (1982)

Figure 4.1 *Case Review Form*   from E.M.GOLDBERG and
                                  R.W.WARBURTON (1979)

NATIONAL INSTITUTE FOR SOCIAL WORK

Case Review Form

Case No. _____

Case   A. _____

Surnames B. _____

Last Reviewed on _____

Last Reviewed by _____

This Review due _____

Office Use _____ 4 9

Office Use .... 5 9   This Review No. _____ 11

ACTIVITIES

Tick as appropriate

Practical services/tangible changes

	Since last Review: Actual	Before next Review: Planned
01 None			10
02 Home Help		
03 Meals-on-Wheels		
04 Day Centres/Clubs/Outings		
05 Holidays		
06 Aids/Adaptations		
07 Material improvements		
08 Financial		
09 Volunteer referral		
10 Assistance with applications		
11 Changes in living group		
12 Help with employment		
13 Temporary accommodation i.e. homelessness		
14 Permanent accommodation i.e. rehousing		
15 Admission/referral for assessment/treatment		
16 Part III: long term		
17 Part III: short-term		
18 Reception into care		
19 Discharge from Part III or care		
20 Change of legal responsibility to client		
21 Other (specify)		

PRESENT

Problems

	Tackled since last Review	Present at this Review
01 Physical disability/illness			12
02 Elderly		
03 Mental illness		
04 Mental handicap		
05 Emotional disturbance in adults		
06 Social isolation and/or loneliness		
07 Child behaviour problems		
08 Delinquency		
09 Family relations problems		
10 Family break-up		
11 Child neglect		
12 Problems in home management		
13 Financial difficulties		
14 Housing problems		
15 Employment difficulties		
16 Problems arising from residential or foster care		
17 Transport difficulties		
18 Other (specify)			29

Code most outstanding problem _____

Tick if child in care of LA _____ 34

Outside agencies in contact with social worker

	Actual	Planned
01 None			31
02 General Practitioner		
03 Health Visitor/Nurse		
04 Psychiatric Hospital/Outpatient Services		
05 Other Hospital/Outpatient Services		
06 Other AHA (Chiropody, Ambulance, etc.)		
07 Child Guidance Clinic		
08 Community Homes		
09 Educational/School		
10 Housing Department		
11 DHSS		
12 Probation		
13 Police		
14 Court/Solicitors		
15 SSD's in other LA's		
16 Voluntary Agencies (clergy, WRVS etc.)		
17 Other (specify)		

Describe present situation/reason for closure . . . .
. . . .
. . . .
. . . .
. . . .
. . . .

Code case status to be

1	Current	3	Closed
2	Dormant	4	Transferred

_____ 35

FUTURE

OPEN CASES What changes are you aiming for? Specify . . . .
. . . .
. . . .
. . . .

Code Major Change Area

1	No change intended	4	Social role
2	Major environmental	5	Behaviour/attitude relationships
3	Social/personal environment		

_____ 36

Code time before case likely to be closed

1	Up to 3 months	3	1 year	5	3 years
2	6 months	4	2 years	6	More than 3 years

_____ 37

Types of workers

	Actual	Planned
1 None			48
2 Qualified Social Worker		
3 Unqualified Social Worker		
4 Social Work Assistant		
5 Volunteer		
6 Others with specialised knowledge		
7 Social Worker plus specialised knowledge		

Social worker activities

	Actual	Planned
01 None			55
02 Exploratory/(re-) assessment activity		
03 Information/advice		
04 Mobilising resources		
05 Advocacy		
06 Education in social skills		
07 Check up/review visiting		
08 Facilitating problem solving/decision making		
09 Sustaining/nurturing		
10 Group activities			64

Code most important activity _____

Estimate no. of contacts _____

CLOSED CASES Code Reason for Closure

1	Aim achieved	6	Contact not achieved
2	Change in circumstances	7	Social Worker/Dept. withdraws
3	Client died	8	Referral to other facilities
4	Client withdrew	9	Other (specify) . . . .
5	Client left area		. . . .

_____ 38

Date of last social work contact _____ 44

Today's date _____ 50

Date of next Case Review _____ 56

Social Worker & Team _____ 60

Next social worker & team to review (if different) _____ 64

Spare 1 _____ 80   Spare 2 _____ 80

APPENDIX A

WHAT DO SOCIAL WORKERS DO? A METHOD FOR CLASSIFYING

SOCIAL WORKERS' ACTIVITIES

from N. RAYNES, J. WINNY, and K. MULGREW (1982).

APPENDIX 1   The categories below identify the type of activities in which social workers are involved.

1. Direct contact with clients

    (i)   Face-to-face.
    (ii)   Phone.
    (iii) Letter (including checking for accuracy).

2. Contacts related to clients

    (i)   Face-to-face.
    (ii)   Phone.
    (iii) Letter (including application forms, and checking for accuracy).

3. Enquiries related to client not involving another person

    (i) Action related to acquiring information related to client or preparatory to doing something for a client (e.g. reading phone messages; case notes).

4.   Record-keeping

    (i) Writing information about work done for or with client (e.g. plans; aide memories; case notes; checking accuracy of these).

5.   Reading service material (no person involved)

    (i)   Reading circulars.
    (ii)   Reading in-house material.
    (iii) Reading service information.
    (iv)   Reading research papers.

6.   Other contacts (unrelated to client)

    (i)   Face-to-face.
    (ii)   Phone.
    (iii) Letter (checking for accuracy).

7. Miscellaneous

    (i)   Taking papers to and from secretary/filing.
    (ii)   Walking from one room to another.
    (iii)   Moving paper from one side of desk to another.
    (iv)   Delivering messages and taking telephone messages from colleagues.

8. Travel

    (i)   Going to and from visits to clients.
    (ii)   Going to and from health centre.
    (iii)   Going to and from agencies.
    (iv)   Going to and from meetings in local authority area.

9. Non-productive contacts (e.g. wrong numbers, no reply, person out)

    (i)   Client (phone).
    (ii)   Client (face-to-face).
    (iii)   Colleagues (phone).
    (iv)   Colleagues (face-to-face).
    (v)   Relatives (phone).
    (vi)   Relatives (face-to-face).
    (vii)   Neighbours (phone).
    (viii) Neighbours (face-to-face).
    (ix)   Friends (phone).
    (x)   Friends (face-to-face).
    (xi)   Other (phone).
    (xii)   Other (face-to-face).

APPENDIX 2   Purposes of activities

1. Giving/receiving/obtaining/discussing/information about social services.
2. Arranging/facilitating use/access to these.
3. Giving/receiving/obtaining/discussing/information about other services.
4. Arranging/facilitating use/access to these.
5. Giving/receiving/obtaining/discussing/information about client's history; practical situation; relationships with people; opinions, needs; feelings; resources,service use; future plans, with anyone other than a colleague (e.g. client, relative, friend).
6. Giving advice and guidance to client, colleague, relative, other.

7. Giving/receiving/obtaining/discussing/information about a client's history; practical situation; relationships with people; opinions; needs, feelings; resources; service use; future plans (client's, colleagues, own); with a colleague.
8. Acting as an 'aide' for a client (e.g. filling in a form).

9. Facilitating working relationships between colleagues (e.g. arranging dates of meetings).
10. Giving/receiving/obtaining/discussing/matters related to own working conditions (e.g. hours of work, salaries, expenses).
11. Giving/receiving/obtaining/discussing/administrative matters relating to client or clients.
12. Supervision.
13. Giving instruction to, and receiving information from a secretary or receptionist.
14. Identifying what the social worker and client can do and arranging contacts between social worker and client.
15. Discussing work-related events, and feelings about these with colleagues.
16. Record-keeping. Writing information about work done for or with client (e.g. plans, aides-memoire, case notes; checking accuracy of these).
17. Travel. Going to and from visits to clients. Going to and from health centre. Going to and from other agencies. Going to and from meetings in local authority area.

APPENDIX 3 Issues arising

A. Relationships outside family.
B. Physical health.
C. Medical service.
D. Social service.
E. Family.
F. Education.
G. Utilities.
H. Emotional health.
I. Housing.
J. Finances/DHSS.
K. Leisure.
L. Work.
M. Legal.
N. Criminal.
O. Household management.
P. Combinations.
Q. Unclear.

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