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ABSTRACT The purpose of this article is to outline ways in which qualitative research has a contribution to make to research on outcomes in health, social work and education. The main questions are contextualized through a general consideration of the relationship between quantitative and qualitative methods, especially in relation to evaluative research. In the main part of the article, I draw some conclusions regarding the contribution that qualitative methodology can make to outcomes research. I illustrate how qualitative research can contribute indispensably to outcomes research in four ways: design solutions; sensitivity to the micro-processes of practice and programmes; applications of symbolic interactionism and ethnomethodology; and qualitative data analysis.

KEYWORDS: *evaluation, outcomes, process, qualitative*

Probation officers tended to 'de-couple' the *process* of supervision from the *outcomes* of someone being on a probation order. One person said that clients 'frequently get into more trouble but I don't think that in any way is a reflection on whether or not I have been effective'. There was a widespread belief that an element of luck operated in being effective. One might do 'brilliant work but if the circumstances are against you they will still re-offend'.

Two social workers, talking about their practice (Humphrey and Pease, 1992: 36) said,

'The fact that I can successfully get her to begin to think about something which might in the end turn a few little cogs in her mind is gonna be for her a success. If there is only a little, teeny chink in the wall of justification for offending then, yeah, I would probably take a little bit of credit for that'.

'You can spend a lot of time on a particular case and you feel you've done

really well, only to find out that somebody rings up and tells you they're in (hospital) on the poisons unit. So, it's very hard to evaluate or to recognise whether you're doing a good job or not'.

(Shaw and Shaw, 1997: 73, 75)

The purpose of this article is to outline ways in which qualitative research may contribute to research on outcomes in health, social work and education. It is a methodology article with a practical purpose. Large tracts of inquiry work are concerned with questions about the benefits of a given policy initiative, programme, service or intervention – benefits that are likely to be couched in terms of outcomes, merit and so on.

An appraisal of the possible contribution of qualitative research to such endeavours is important for two reasons. First, it is plausible to claim a deepened methodological strength in qualitative research in the policy field. Government departments are in some cases more open to commissioning qualitative research for evaluative purposes (e.g. National Institutes of Health, 2001). I believe it is a desirable aspiration for health, social work and education researchers to support qualitative social science research that seeks to integrate rigour (substantive and methodological) and relevance. This point will have local importance, for example in Britain, where the future of mainstream Social Policy undergraduate and postgraduate degrees is under serious threat. New alliances are necessary between 'applied' researchers and sociologists who take an integrative position on rigour and relevance. If we are to avoid reinforcing conventional wisdom about a 'horses for courses' approach to methodological choice, then we need an intellectually principled position on the relationship between different methodologies.

Second, wider developments underline the timeliness of this discussion. Government departments and research funding bodies in the USA, Britain, Australia and Canada talk routinely of 'evidence-based policy and practice'. There are widespread initiatives to facilitate the integration of research into professional practice, which are echoed in research and development initiatives that bring health and social care into working proximity. Developments in Britain such as the Economic and Social Research Council's initiative on Evidence Based Policy and Practice (see, for example, <http://www.evidencenetwork.org>) aim to be a catalyst in the development of evidence-based practice. While these are welcome developments, there is a risk that advocates of conventional outcomes research may too heavily influence debates about evidence.

A couple of cautions are in order. For one thing, this article should not, of course, be read as a claim that qualitative research is better equipped than more traditional approaches to generate foundationalist knowledge of policy and practice outcomes. This would be crass. '*Nothing* can guarantee that we have recalled the truth' (Phillips, 1990: 43). In the human sciences knowledge of outcomes – especially of future ones – is not so much informative as advisory. 'Like the cry of the backseat driver: "You'll be in the ditch in a

minute”, such communications function more like *advice*. “Consider how you would like it if things turned out this way”. . . . To receive such communications about ourselves may in a sense be informative; but it does not confront us with the “take-it-or-leave-it” claim to our assent which is the hallmark of objective knowledge. We can falsify it’ (McKay, 1988: 140–41).

In addition, I would have unwittingly provided ‘aid and comfort to the barbarians’¹ if what follows was received as a plea to replace one uniformitarian orthodoxy with another. On the one hand, a notion that only qualitative methods can examine unique, complex cases is clearly not accurate, as there is an interesting history of idiographic and ipsative quantitative methods for individual case analysis in psychology in the work of people such as Rogers, Allport, Cattell and Kelly. Likewise, I am not convinced that all forms and traditions of qualitative methodology lend themselves equally or even directly to evaluative purposes. Stake no doubt had this in mind when he remarked that ‘to the qualitative scholar, the understanding of human experience is a matter of chronologies more than of cause and effect’, and that ‘the function of research is not . . . to map and conquer the world but to sophisticate the beholding of it’ (Stake, 1995: 39, 43).

Finally, the way I approach this article probably betrays my British social science background. I talk of research and evaluation almost interchangeably for much of the article. I may be wrong – indeed, for most US readers I am certainly wrong, and evaluation is best understood as a distinct discipline. But the basic arguments are probably little affected by this.

In the first part of the article, I consider the relationship between quantitative and qualitative methods especially in relation to evaluative research. I discuss this from the position that there is a *real* but very *imperfect* linkage between philosophy and method. I concur with Greene when she concludes that ‘epistemological integrity does get meaningful research done right’ (Greene, 1990: 229). In the main part, I draw some preliminary conclusions regarding the contribution that qualitative methodology can make to outcomes research.²

Numbers and qualities

These two purposes of evaluation research, process versus outcome studies, may be best respectively addressed by qualitative versus quantitative methodologies.

(Thyer, 2000: 402)

Thyer captures in this single sentence the most common image of the relationship between quantitative and qualitative research, and links it to the related question of how such research should address issues of process and outcomes. John Stevenson expresses the position more subtly as follows:

Despite the fascination I have with the narrative complexities in the programs I have evaluated, I see this as the background for the evaluation, worthy of attention as it affects the foreground, the movement toward finding replicable means

of accomplishing desired ends. Is the qualitative salience of the process, and especially the conflicts and inconsistencies, the best guide to where attention should be paid? I see the process as seductive, and relevant only up to a point.³

The two sets of relationships (quantitative/qualitative and outcome/process) have often been expressed either in terms of a functional division of labour – what may be described as a ‘horses for courses’ approach (e.g. Chelimsky, 1997) – or in terms of a relationship of hierarchy, where aspects of one methodology are alleged to be intrinsically superior to the other. Macdonald advocates the latter position in a contribution – ironically – to a book that claims to seek a rapprochement between quantitative and qualitative approaches. In discussing the case for randomized control trials (RCTs), she concludes that it is ‘essential that *all* research designs deployed in outcomes research pay heed to the sources of internal validity that RCTs are best able to control’ (Macdonald, 1999: 101). This is a self-betraying remark, illustrating that too often the emergence of methodological pluralism has had little or no impact on outcomes research.

The roots of these debates go back at least to the work of Campbell and Cronbach from the 1960s onwards in America. Their work, and the debates between them on issues of internal and external validity, process and outcomes, and rationality, has not been sufficiently taken into account by the current generation of evidence-based evaluators and supporters of scientific practice. In some cases, their contribution has not even been acknowledged. Cronbach remarked that it is ‘rationalist to the point of unreality’ to proceed on the basis that evaluation starts from agreement on goals (Cronbach et al., 1980: 129). All social programmes, he argued, have broad and vague goals, even supposedly targeted programmes. This is not escapism, as the rationalists would argue, but reflects the nature of programmes as operating within a climate of political accommodation. ‘The first rule of the successful political process is, “Don’t force a specification of goals or ends”’ (p. 130). House correctly concludes that the traditional formulation ‘is not the world of social programmes and, in general, is not the social world at all’ (House, 1993: 135). Rather, we are engaged in ‘social inquiry for and by earthlings’ (Cronbach, 1986).

The lesson has been hard learned. Silverman, in his study of HIV counselling, still found grounds to complain of the ways in which what he calls the Explanatory Orthodoxy of counselling research leads to a focus on either the causes or consequences of counselling. This approach ‘is so concerned to rush to an explanation that it fails to ask serious questions about what it is explaining’ (Silverman, 1997: 24), such that the phenomenon of counselling ‘escapes’. Traditional formulations of causal inputs and outcomes need at the very least to be delayed until we have understood something of the ‘how’. For sound evaluative reasons we will want to ask the explanatory ‘why’ questions. ‘There is no reason not to, provided that we have first closely described how the phenomenon at hand is locally produced’ (p. 35).

The inadequacy of conventional outcome designs arises in part from the extreme difficulty of isolating inputs. Abrams' article on the problems of measuring informal care summarized the position as follows:

The resistance of informal social care to experimental evaluation has entirely to do with the problem of breaking down the intractable informality of the treatment; of reducing informal caring relationships to the sort of units, factors, events, variables, items needed if specifiable inputs are to be systematically related to specifiable outcomes.

(Abrams, 1984: 2)

The 'sheer weight and diversity of the "quasi-inputs" which appear to intervene between effects and their presumed causes' means that 'the fine web of conditions within which social action occurs is discovered in the course of the research instead of having been provided for in the research design' (p. 4). The force of Abrams' remarks should not be missed. It is not simply that experimental evaluation has technical limitations. Experimental and other comparable evaluation strategies inevitably disaggregate informal care, and rob it of its inherent systemic and holistic character. The problems stem from the intrinsic incapacity of such designs, rather than their technical imperfections.

At the heart of the question lie different 'root metaphors' of what research is basically about (Kushner, 1996). For mainstream outcomes researchers, inquiry is basically about order; for process researchers it is about conversation. Kushner suggests the key test is what keeps evaluators awake at night. For outcomes researchers it is not managing to distil the evaluation into a single unified story; for process researchers it is having only one story to tell.

We should not jump from this to the counter-conclusion that qualitative methodology has no problems of its own. Qualitative research, especially that of an interpretive cast of mind, has been criticized – for example, by Giddens who complains that interpretive sociology sits too close to philosophical idealism. Hence it is marked by:

A concern with 'meaning' to the exclusion of the practical involvements of human life in material activity.

A tendency to seek to explain all human conduct in terms of motivating ideals at the expense of the causal conditions of action.

A failure to examine social norms in relation to asymmetries of power and divisions of interest in society.

(Giddens, 1993: 163–4)

Naïve constructivism, as applied to evaluation, 'risks losing sight of culture'. Individuals daily struggle to 'reconcile their hopes with the institutional hand they have been dealt' (Kushner, 1996: 198–9), and qualitative evaluation research must address this individual/culture relationship.

Fortunately, there have been constructive, if cautious, dialogues regarding the relative merits and characteristics of quantitative and qualitative methodologies. For example, in social work research, Reid in the USA and Sinclair in

Britain have developed mediating positions. Reid seeks to 'redefine the nature of the mainstream so that qualitative methodology is a part of it not apart from it'. He regards quantitative research as strong when dealing with linkages, control, precision and larger data sets, while qualitative research is able to depict system workings, contextual factors and elusive phenomena, and provide thorough description. 'Neither method is superior to the other, but each provides the researcher with different tools of inquiry' that can be set against a single set of standards (Reid, 1994: 477).

Sinclair goes somewhat further when he says that qualitative methods are in many ways 'more adapted to the complexity of the practitioner's world than the blockbuster RCT'.

Qualitative research draws attention to features of a situation that others may have missed but which once seen have major implications for practice. It counteracts a tendency to treat the powerless as creatures with something less than normal human feelings. It contributes to an ethically defensible selection of outcome measures. And, in combination with simple statistical description, it can lead to an informed and incisive evaluation of programmes in social services (Sinclair, 2000: 8).

He turns common assumptions on their head when he concludes that:

Quantitative social work research does face peculiarly acute difficulties arising from the intangible nature of its variables, the fluid, probabilistic way in which these variables are connected, and the degree to which outcome criteria are subject to dispute. (pp. 9–10)

Qualitative researchers have also addressed the relationship between different methodologies in ways that fruitfully extend the debate (e.g. Bryman, 1988; Greene and Caracelli, 1997), and have explored the relative contributions different methodologies make to evaluation studies of process and outcomes.

It would be highly premature, however, to conclude that the debates should now be closed, and we should simply get on with evaluation research without wasting time on the profession of 'philosophical and methodological worry' (Becker, 1993: 226). For example, it is sometimes wrongly assumed that using multiple methods will lead to sounder consensual conclusions in an additive fashion. Naïve versions of triangulation arguments are sometimes resorted to in support of this argument (cf. Bloor, 1997, for a helpful discussion of triangulation as a means of judging validity). One of the most insightful discussions of the problems raised by this assumption is Trend's early classic account of an evaluation of a US programme designed to test the effectiveness of direct payment of housing allowances to low income families (Trend, 1979). In one case study, the *quantitative* data suggested that the programme was producing major improvements in housing quality. Yet all the *qualitative* data indicated the programme would fail. The major part of Trend's article records his assiduous sifting of the data in an attempt to discover a plausible explanation that did not simplistically cut the Gordian knot, either by prioritising one kind of data above the other through paradigm arguments,

or by premature syntheses. His conclusion still stands as a warning against such easy solutions:

The complementarity is not always apparent. Simply using different perspectives, with the expectation that they will validate each other, does not tell us what to do if the pieces do not fit. (1979: 83)

His advice is:

That we give the different viewpoints the chance to arise, and postpone the immediate rejection of information or hypotheses that seem out of joint with the majority viewpoint. (p. 84)

He quotes approvingly Paul Feyerabend as saying 'It seems that it is not the puzzle solving activity that is responsible for the growth of knowledge, but the active interplay of various tenaciously held views' (p. 84).

The inter-relationship of qualitative and quantitative methods is not only, nor even primarily, about choice of methods. It is about single cases or comparison; cause and meaning; context as against distance; homogeneity and heterogeneity. It entails judgements about validity and the criteria of quality in social work research, the relationship of researcher and researched, and measurement. Methodological choice is also inextricably relevant to issues of the politics and purposes of social research, values, participatory forms of research, interdisciplinary inquiry and the uses of research.

How we understand the relationship between different methodologies will, of course, be closely linked to the position taken on paradigms. Hence, we have already anticipated our likely direction in our preliminary comments on paradigms. There are three broad positions (the terminology is that used by Greene and Caracelli, 1997). The *purist* position argues that different frameworks of inquiry embody fundamentally different and incompatible assumptions about the nature of social reality, claims to knowledge, and what it is possible to know. Multi-methods at the paradigm level are not an option. The *pragmatic* position is best represented by what we have described as a functional division of research labour. The position that is likely to prove most creative for qualitative outcomes research is that described by Greene and Caracelli as *dialectical*. This position accepts that philosophical differences are real and cannot be ignored or easily reconciled. We should work for a principled synthesis where feasible, but should not assume that a synthesis will be possible in any given instance. This represents

a balanced, reciprocal relationship between the philosophy and methodology, between paradigms and practice. This . . . honours both the integrity of the paradigm construct and the legitimacy of contextual demands, and seeks a respectful, dialogical interaction between the two in guiding and shaping evaluation decisions in the field.

(Greene and Caracelli, 1997: 12)

One possible undesirable consequence is that an emphasis on the value of multiple, integrated methods may lead to a dilution of one or the other – a

lowest common denominator position. It may also lead to a tendency to treat qualitative methodology (or quantitative) in an unduly homogenous way. Stake's recent confession should not be ignored. 'I have not had much luck in using qualitative and quantitative studies to confirm each other. The criteria of the one get lost in the events of the other. Consistencies, enrichments in meaning, can be found but, I think, seldom confirmations'.⁴

Partly in recognition of this, there is a need to develop the case for a dialectical mix of methods *within* qualitative research. This will need to proceed through the development of a set of critical features of knowledge for different qualitative methodologies. A helpful starting point for this is the article by McKeganey and colleagues, in which they discuss the benefits and limitations of interviewing and observation methods as part of a study of professional decision-making when people may be offered a place in a home for the elderly (McKeganey et al., 1988; cf., Shaw, 1999: 145–6). This initial analysis needs to be extended to a full range of qualitative strategies, and tied to the critical features of the associated knowledge claims (Greene and Caracelli, 1997: 12–13).

Evaluative judgements

Qualitative applications to outcome research can be facilitated by rethinking assumptions about how evaluative reasoning proceeds.

Everyone agrees that information somehow informs decisions, but the relationship is not direct, not simple. Often the more important the decision, the more obscure the relationship seems to be.

(House, 1980: 68)

House goes as far as to say that 'subjected to serious scrutiny, evaluations always appear equivocal' (p. 72). He argues that evaluations can be no more than acts of persuasion. 'Evaluation persuades rather than convinces, argues rather than demonstrates, is credible rather than certain, is variably accepted rather than compelling' (p. 73). Evaluators have too frequently underplayed the role of judgement and hence of argumentation. This has resulted in an unduly technical, methods-oriented analysis, an over-confidence in quantification and a tendency to represent knowledge in an impersonal, objectified form. Those who fail to accept the 'compelling' conclusions drawn from the evaluation are dismissed as irrational. If results are unequivocal then those who fail to accept them are 'wrong'.

There have been several attempts to take a different approach to thinking through the reasoning process involved in constructing justified evaluative arguments. These typically emphasize the complex connection between evidence and conclusions, and commence from the differences between formal and informal reasoning. Whereas formal reasoning assumes a tight fit between the premises and conclusions within an argument, informal logic 'deals with ordinary, everyday language, where rules of inference are less pre-

cise and more contextual' (Fournier and Smith, 1993: 317). The key question is whether good but non-deductive reasoning is possible – i.e. reasoning that is not logically valid in a formal inferential sense. 'The consensus among informal logicians is that there can be logically good, but nonvalid reasoning' (Blair, 1995: 73).

This philosophical argument has direct practice implications because much evaluative reasoning is non-deductive. For example, we may sometimes engage in good all-things-considered reasoning, where there are reasons for and against a point of view but where the pros outweigh the cons. The argument by House cited above falls in that category. Also, there has been growing acceptance of the circumstances in which it may be legitimate to reason from *factual* evidence to *evaluative* conclusions, where there can be no logical relation of implication for such an argument. Finally, informal logicians have concluded that much reasoning is dialectical. Reasons for a claim are seen as a move in an argument – an attempt to persuade offered as part of an actual or possible exchange between parties who differ.

Process and outcomes

Given these more general arguments, how can qualitative research and evaluation address outcome questions? In four ways:

1. Through design solutions partly analogous to designs which entail a degree of control.
2. A shift of emphasis from internal validity to questions of external validity and generalization can lead to a greater sensitivity to the micro-processes of practice and programmes.
3. Developments in symbolic interactionism and ethnomethodology are susceptible to application to outcome questions.
4. Several approaches to analysis of qualitative data facilitate an enriched development of outcome judgements.

DESIGN SOLUTIONS

The development of case study designs, and the use of simulation methodology provide two contrasting illustrations of the development of qualitative design solutions analogous to designs, which entail a degree of control.

Qualitative design solutions have been pursued actively by those who have worked on the borders of qualitative and quantitative methodology. For example, Donald Campbell's early position was that 'one-shot' case study designs are uninterpretable with regard to causal attribution. However, through exchanges with Becker and Erikson, he came to the position that the analogy of degrees of freedom provides a major source of discipline for interpreting intensive, cross-cultural case studies. The logic entails testing theories against the predictions or expectations it stimulates, through a general process he describes as pattern matching, based on the premise that 'experimental

design can be separated from quantification' (Campbell, 1978: 197). He developed a perspective which entailed a mutual commitment to both ethnography and comparative studies.

Campbell's approach has been developed in Yin's account of case study research (Yin, 1994). Patton has also illustrated how 'well-crafted case studies can tell the stories behind the numbers, capture unintended impacts and ripple effects, and illuminate dimensions of desired outcomes that are difficult to quantify' (Patton, 2002: 152). He makes a familiar distinction between programme improvement and individual outcomes, and argues that qualitative case studies offer a method for capturing and reporting individualized outcomes. Insofar as it is necessary to understand individual outcomes – and in many classroom, school-level, health interventions, criminal justice programmes and human services interventions understanding of such outcomes is often vital – then quantitative, standardized measures will be inappropriate. Take, for example, education or human services programmes that aim at some form of individual autonomy and independence. 'Independence has different meanings for different people under different conditions. . . . What program staff want to document under such conditions is the unique meaning of the outcomes for each client'. 'Qualitative methods are particularly appropriate for capturing and evaluating such outcomes' through the use, in particular, of inductive description (Patton, 2002: 158, 476).

Simulations offer a rather different design solution for qualitative inquiry, and have the potential to provide 'a unique and innovative tool that has not yet been widely applied' (Turner and Zimmerman, 1994: 335). They have two main applications – first, as an evaluative test for service discrimination and, second, as a qualitative proxy for control within a natural setting. The first application is essentially an export from the social psychology laboratory, which offers some control over stimuli but also suffers from some of the laboratory risk of, for example, artificiality and demand characteristics. But it is the second application that is more relevant for present purposes. One particular example of simulation – the simulated client – represents an advance on the use of vignettes in policy research. Those who evaluate the process of professional practice come face to face with the invisibility of practice. How may we learn the ways in which lawyers, teachers, general medical practitioners or social workers practise? How would different professionals deal with the same case? Wasoff and Dobash used a promising innovatory method in their study of how a specific piece of law reform was incorporated into the practice of solicitors (Wasoff and Dobash, 1992; Wasoff and Dobash, 1996). The use of simulated clients in 'natural' settings allowed them to identify practice variations that could be ascribed with some confidence to differences between lawyers rather than the artefacts of differences between cases.

Suppose, for example, that one wishes to carry out a qualitative evaluation of decisions made by housing managers, medical staff and social workers

regarding the allocation of care management packages. Evaluators using simulated clients would prepare a small number of detailed case histories designed to test the practice decisions under consideration. A researcher or evaluator takes on the role of the client in the case history. The housing manager, relevant medical staff and social workers each interview the 'client' within the 'natural' setting of their work.

There are limitations to the application of simulation methods. The method needs additional resources to prepare the case material, perhaps to act the roles of clients, and to reflect on the quite detailed material that results from transcriptions of the interviews. The cost is therefore likely to be relatively high, and it requires reasonably high levels of research skills. However, the use of simulated clients has several things going for it. First, some researchers, especially insider researchers who have completed professional training programmes, are likely to be familiar with the 'family' of role-playing methods from which it is drawn. Second, other methods are not always feasible for practical or ethical reasons. Simulated clients overcome the ethical problems of seeking the cooperation of genuine clients. Above all, thirdly, it makes practice visible. It will be clear from the brief description that the method could not be a tool for evaluating particular cases, but would focus on specific *kinds* of practice.

MICRO-PROCESSES

Qualitative research also contributes to outcomes when attention is given to the micro-processes of practice and programmes. This is one plausible response to the criticism made by Abrams quoted earlier in this article. William Reid has been attracted by the potential of 'change-process' research. He does not reject the role of controlled experiments but concludes that 'practical and ethical constraints on experiments necessitate a reliance on the naturalistic study of these relations' (Reid, 1990: 130). This entails a focus on the processes of change during the period of contact between the professional helper and the client system. Rather than relying on aggregated, averaged summary measures of outcomes, this approach returns to the content-analysis tradition in social research, through a greater focus on micro-outcomes.

Reid applies his ideas to social work, although the logic applies to other forms of change-oriented professional service. A systemic view of intervention is at its root, in which professionals and service users are viewed in a circular, mutually influencing interaction. In this model 'conventional distinctions between intervention and outcome lose their simplicity' (p. 135). 'It then becomes possible to depict change-process research as a study of strings of intermixed i's and o's' – interventions and outcomes (p. 136). While Reid seeks to defend experiments, he suggests a more naturalistic stance when he says that 'averages of process variables that are devoid of their contexts at best provide weak measures' (p. 137).

A different and interesting argument for using qualitative methods as a means of understanding micro-processes has been suggested by McLeod in a thoughtful assessment of the potential of qualitative methods for understanding outcomes of counselling. He suggests that qualitative interviews are more likely to elicit critical perspectives than questionnaires, arising from the 'demand characteristics' of extended interviews. 'In terms of producing new knowledge that contributes to debates over evidence-based therapy, it would appear that qualitative evaluation is better able to explore the limits of therapeutic *ineffectiveness*' (McLeod, 2001: 178). Combined with their potential for eliciting positive relations between intervention and outcome, he concludes, not unlike Patton, that 'Qualitative interviews appear to be, at present, the most sensitive method for evaluating the harmful effects of therapy and also for recording its greatest individual successes' (p. 179).

ETHNOMETHODOLOGY AND SYMBOLIC INTERACTIONISM

Ethnomethodology and symbolic interactionism suggest more general avenues of influence on outcomes inquiry. Two examples will serve to illustrate our point. First, Miller's work enriches our understanding of the importance of context in qualitative outcomes research. He discusses ways that institutional texts constructed to explain past decisions inevitably gloss over the openness and complexity of the decision-making process (Miller, 1997). He gives the mundane example of evaluation research on a bowel-training programme in a nursing home. The evaluation consisted of counting when and how patients had bowel movements. The programme was judged to have a successful outcome if patients used a toilet or bedpan and ineffective for those who continued soiling beds. One patient had soiled her bed. However, ethnographic methods enabled the researcher to observe a nursing aide contesting the definition of this incident as 'failure', on the grounds that the patient knew what she was doing and had soiled her bed as a protest act against staff favouring another patient. This illustrates how mundane, everyday life is illuminated by observing the context of text construction. This would not have found a way into the formal outcome record. Text production in institutions is 'micro-politically organized', and this includes textual outcome records.

Second, Denzin's interpretive interactionism has also had an impact on thinking about service outcomes (Denzin, 1989). Mohr, for example, extends Denzin's argument to the evaluation of clinical outcomes in health research. She argues that the method strives to inspect the relationships between personal difficulties, experiences, policies, interventions and institutions. 'Interpretive interactionism permits intensive scrutiny of the ramifications and outcomes of various interventions' (Mohr, 1997: 284). It can

1. Sort out different ways problems are defined.
2. Show how patients experience care. What it is about interventions they find helpful or not, and in what circumstances.

3. Identify 'secondary causes' e.g. contexts, culture, and the meanings patients bring.

'Strategic points for intervention can be identified by contrasting and comparing patients' thick descriptions, and these can be used to change, to improve, or to negotiate and renegotiate interventions' (p. 284). It is valuable when 'an outcome may not be readily apparent, and . . . the intervention is something that only the patient and not the professionals can define' (p. 285).

QUALITATIVE ANALYSIS AND OUTCOMES

The fourth qualitative line of approach to methodological problems posed by conventional outcome research is rather different. Encouraged to some extent by recent developments in the realist school of the philosophy of science, fresh energy has been given to strategies for the analysis of outcomes and to cautious inferences about cause and effect (e.g. Henry et al., 1998). As John and Lyn Lofland express it:

Qualitative studies are not designed to provide definitive answers to causal questions . . . (but) it can still be an appropriately qualified pursuit.
(Lofland and Lofland, 1995: 136, 138)

Miles and Huberman are even less reserved: 'The conventional view is that qualitative studies are only good for exploratory forays, for developing hypotheses – and that strong explanations, including causal attributions, can be derived only through quantitative studies'. They describe this view as 'mistaken' (Miles and Huberman, 1994: 147), and insist that qualitative evaluation research can

1. Identify causal mechanisms
2. Deal with complex local networks
3. Sort out the temporal dimension of events.

They also argue that it is well equipped to cycle back and forth between different levels of variables and processes, and that a selective adoption of analytic induction provides a way of testing and deepening single case explanations.

Causal accounts will be local and 'now-oriented' (Lofland and Lofland, 1995: 141). Miles and Huberman develop analytic methods that address causal attribution in both single and multiple case explanations. For example, they advocate the use of field research to map the 'local causal networks' which informants carry in their heads, and to make connections with the evaluator's own emerging causal map of the setting. Such maps start from 'causal fragments', which lead on to linked building of logical chains of evidence. Such causal networks

are not probabilistic, but specific and determinate, grounded in understanding of events over time in the concrete local context – *and* tied to a good conceptualisation of each variable.

(Miles and Huberman, 1994: 159)

Patton gives an example of how the creation of qualitative matrices is especially useful for exploring linkages between process and outcome. The analytic sequence entails the development of categorizations of types and levels of outcomes and of program processes. The categories are developed through orthodox qualitative analysis. The relationships between processes and outcomes may come either from participants or through subsequent analysis.

In either case, the process/outcomes matrix becomes a way of organizing, thinking about and presenting the qualitative connections between program implementation dimensions and program impacts.

(Patton, 2002: 472)

The following extract gives an illustration of how this approach can operate.

Suppose we have been evaluating a juvenile justice program that places delinquent youth in foster homes A *regularly recurring process theme* concerns the importance of 'letting kids learn to make their own decisions'. A *regularly recurring outcome theme* involves 'keeping the kids straight'. . . . By crossing the program process ('kids making their own decisions') with the program outcome ('keeping the kids straight'), we create a data analysis question: What actual decisions do juveniles make that are supposed to lead to reduced recidivism? We then carefully review our field notes and interview quotations looking for data that help us understand how people in the program have answered this question based on their actual behaviors and practices. By describing what decisions juveniles actually make in the program, the decision makers to whom our findings are reported can make their own judgements about the strength or weakness of the linkage (pp. 472–3).

Hence, while qualitative evaluation cannot resolve the problems of causal conclusions any more than quantitative evaluation, it can assess causality 'as it actually plays out in a particular setting' (Miles and Huberman, 1994: 10).

Part of this reasoning was anticipated by Cronbach's arguments regarding causal models. Rejecting the idea of causation as events that can be predicted with a high degree of probability, Cronbach developed twin arguments. First, he argued that causes are contingent on local interactions of clusters of events. More than one cluster may be sufficient, but no one cluster is necessary. Second, he accepted that there are usually missing events or conditions that affect the outcome of a given programme, but about which we know little. He was the first theorist to produce a plausible explanation of contextual factors in evaluation. Hence, he concludes that 'after the experimenter with his artificial constraint leaves the scene, the operating programme is sure to be adapted to local conditions' (Cronbach et al., 1980: 217). Furthermore, somewhat pessimistically, he concluded that 'a programme evaluation is so dependent on its context that replication is only a figure of speech' (p. 222).

Qualitative research and evaluation share a recognition of the irony of social causes and consequences. Much of the sociology of deviance was

based on just this sense of irony, with its exploration of deviant roles as doing necessary 'dirty work'. It leads to the question of what functions are served by a particular practice that would not be served by its absence. What are the typical results of this phenomenon in this setting, and what ends are served thereby? Lofland and Lofland make the important observation that causal answers are by and large based on *passivist* conceptions of human nature. Qualitative inquiry has often steered away from causal accounts, not because the methodology is weak in that area but because of a commitment to an *activist* conception of human nature. The Loflands argue that an activist conception will lead to a focus on questions that address both structures and strategies. This will involve 'deciphering and depicting exactly what sort of situation the participants are facing' (Lofland and Lofland, 1995: 146), and understanding the 'incessantly fabricated' strategies people construct to deal with the situation.

Take, for example, Silverman's work on HIV counselling. He is right to conclude that 'it is usually unnecessary to allow our research topics to be defined in terms of ... the "causes" of "bad" counselling or the "consequences" of "bad" counselling' (Silverman, 1997: 34), insofar as such topics reflect the conceptions of social problems as recognized by professional or community groups. Nonetheless, this does not require the abandonment of causal inquiry in qualitative evaluation. Inquiry into the ways in which professionals incessantly fabricate service forms and structures does promise a better way to understand causes. The voices of human services professionals that head this article illustrate the visibility yet elusiveness of outcomes in the search for meaning in narrative accounts.

Causal networks exist at the level of the individual case. Miles and Huberman also develop ways in which ordering and explaining can be tackled through cross-case displays of data. They are confident of the ability of qualitative inquiry to go beyond the problems of inferring from association ('the weasel word of quantitative researchers', p. 222). They summarize the process as follows:

Cross-case causal networking is a comparative analysis of all cases in a sample, using variables estimated to be the most influential in accounting for the outcome criterion. You look at each outcome measure and examine, for each case, the stream of variables leading to or 'determining' that outcome. Streams that are similar to or identical across cases, and that differ in some consistent way from other streams are extracted and interpreted. (p. 228)

They spell out the steps to accomplishing this analysis, but suggest that it may not be feasible for either larger or very small samples.

An example may make this process more visible. Shaw and colleagues describe a qualitative, case study evaluation of a rural activity centre for people with learning disabilities (Shaw et al., 1992). They observed and interviewed project participants, parents, carers, management group members, key workers and other professionals. Project records were analyzed.

When describing and explaining the workings of the centre, the people who were interviewed appeared to draw on one or more of three different models of the scheme. These were a 'training for work' model, a 'personal and social growth' model and an 'education for life' model. These operated in part as causal maps which entailed an array of model-specific positions on the aims of the project, optimal target groups, desirable programme patterns, staffing requirements, future development strategies and likely or desirable project outcomes.

TABLE 1. *Causal maps: participant models for a rural activity centre*

	<i>Training for work</i>	<i>Personal/social growth</i>	<i>Education for life</i>
Aims	Credible work skills for independent/sheltered work	Personal and social growth	Alternative occupation to enhance the quality of life
Target group	Demonstrable ability to benefit; younger	Wide range of age and ability	Wide ability range; younger
Programme	Time limited stay; skill learning; assessment and review; contracts; move-one facility; integration into work	Open stay period; small project; small-group activities; counselling; liaison with carers and social work agencies	Loosely held time limits; the best learning context; interest-led contracts; community based activities and outside links; craft work and home making skills
Staffing	Education and special needs employment skills; plus volunteers	Social and group work qualifications; plus expert consultants	Education and social work qualifications; plus volunteers
Outcome	Regular throughput; work placements; normalization of work patterns; skill learning	No clear distinction between programme and outcome	Wide range of social skills; integration into community networks; change of attitudes on the part of outside community members

Finally, to return to a general point made earlier, qualitative research facilitates the *valuation of outcomes*, and is opposed to the technicalization of outcome research. While this is not exclusively the province of qualitative research, more conventional and strictly evidence-based varieties of outcome research tend to treat such issues as technical matters. This links to the broader question of value and political issues.

Evidence on effectiveness and outcomes and an emphasis on health gain and health outcome provide an apparently value-neutral, rational approach and means for rationing health and social care. Beneath the range of technical

issues in assessing outcomes are political and social values that need to be explicit.

(Long, 1994: 175)

Concluding issues

I have argued for ways in which qualitative research offers a distinctive and indispensable element in outcomes research in the fields of education, social work and health. Starting from the position that there is a *real* but very *imperfect* linkage between philosophy and method, the article reviewed the continuing grounds to doubt the adequacy of conventional controlled designs for understanding outcomes of projects, policies, practice and programmes. I then illustrated how qualitative research can contribute indispensably to outcomes research in four ways:

1. Design solutions
2. Sensitivity to the micro-processes of practice and programmes
3. Applications of symbolic interactionism and ethnomethodology
4. Qualitative analysis.

There are a number of problems and opportunities for continuing work that come into focus through this discussion. In the first instance, the focus on examining claims made in argument, rather than starting from theoretical arguments, directs our attention to how researchers (and practitioners) actually do reason. This presents a forcible case for an empirical agenda.

Second, conventional controlled designs for outcomes research tend to be linked to an instrumental model of research as having direct utility for subsequent application. The problem with this – long since recognized by writers from Carol Weiss onwards (e.g. Weiss, 1988) – is that it does not square with evidence on how research is actually used, and it misunderstands the nature of the policy-making process. It is based on a rationalistic model.

The rationalist model of policy making sees it as a series of discrete events, where each issue to be decided is clearly defined, and decisions are taken by a specific set of actors who choose between well-defined alternatives, after weighing the evidence about the likely outcome of each.

(Finch, 1986: 149–50)

Over against this, there has been development of an enlightenment model of research use. 'It offers far more space for qualitative research, through its emphasis on understanding and conceptualization, rather than on providing objective facts' (p. 154). The enlightenment model, while valuable for its realistic depiction of research use, is not a universal description of how research information use operates. For example, practitioner research is likely to proceed on a more immediate instrumental view. Empirical work on information use is equally important as work on evaluative judgements.

Thirdly, the strategies discussed earlier tend to favour an incremental approach to social change. This is also the case of the enlightenment model of

research use, which often assumes a piecemeal, social engineering position. This raises in turn the general question whether qualitative methods are especially congenial to advocacy methods in research. My view is that there is no necessary relation between the two, but in practice they have proved mutually reinforcing.

Finally, qualitative approaches to outcomes research tend to give greater prominence to theorizing, but not in the caricature of research represented by the discredited belief that 'science grows by repeated overthrow of theories with the help of hard facts' (Lakatos, 1970: 97). Finch argues:

First, that a concern with theory is quite compatible with qualitative research; second that a blend of theory and data is the hallmark of good qualitative work; and third, that this particular blend produces precisely the kind of work which is likely to make an impact upon policy because it offers theoretical insights grounded in evidence. (p. 174)

Greene (1993) specifies what this might entail. We should

- Explicate our own theoretical predispositions
- Describe locally held theories ('locally meaningful theoretical perspectives in data interpretation', Greene, 1993: 38)
- Attend to emergent theoretical issues
- Integrate substantive theory into research conclusions and recommendations.

Empirical work on evaluative judgements, developing plausible models of research information use, exploring the possibilities for interlinks between qualitative methodology, advocacy research and evaluation, and the diverse and complex relations between the theoretical and the practical, leave more than enough to be getting along with in the development of benchmark standards for qualitative outcomes research.

NOTES

1. The phrase is borrowed from Carol Weiss' reflections on how neo-conservatives latched on to liberal research critiques of education in the 1970s.
2. This article develops arguments made in Shaw and Gould (2001) *Qualitative Research in Social Work*. London: Sage, chapters 1 and 11.
3. Personal communication.
4. Quoted from a posting on EVALTALK – the discussion list of the American Evaluation Association – 26 June 2001.

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