Quality in Qualitative Research

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A variety of conceptions of qualitative research exist, with competing claims as to what counts as good quality work. Rather than opting for the criteria promoted by one variety, "paradigm," "moment," or school within qualitative research, practicing researchers can learn valuable lessons from each one. This is because social research is a craft skill, relatively autonomous from the need to resolve philosophical disputes. At the same time, methodological awareness is a valuable mental resource in research studies. It can be acquired by exposure to almost any intelligent methodological discussion, whether from positivist, naturalistic, constructivist, or postmodern paradigms, as well as from careful consideration of research studies done by others. Particular techniques developed originally to fulfill the requirements of particular paradigms can often be used for other purposes and from within other paradigms if need be. This is illustrated in a case study of triangulation.

A lot of effort has been expended by methodologists over the years, trying to give some guidance to qualitative researchers in improving or judging the quality of qualitative research. You could say that all methodological writing is ultimately directed at such a goal, because the idea of writing about how one can do research is presumably aimed at giving other people some good ideas on how they might proceed with their own studies. Explicit discussions of quality in social research, though, began from concerns designated with words such as *validity* and *reliability*, developed within the quantitative or scientific tradition, and then moved on under the pressure of critique from the qualitative research community. At first, this led qualitative methodologists to spawn new terms that either substituted for the scientific language of earlier periods or added new ideas to them. More recently, with postmodernist perspectives in fashion, the whole issue of whether we ought to be trying to generate criteria for judging the quality of research has become controversial. Maybe we should be letting a thousand flowers bloom, people say. The result

Author's Note: Martyn Hammersley and David Silverman assisted me in the development of these ideas, both by the examples they set in their own methodological writings and by providing their time generously in commenting on related manuscripts. Norman Denzin, Paul Filmer, and Yvonna Lincoln also helped me improve the quality of this piece with some perceptive comments on earlier drafts.

Qualitative Inquiry, Volume 5 Number 4, 1999 465-478

is that practicing researchers now have to find their way through a mass of conflicting positions, and methodology is in danger of getting a bad name. I want to suggest a way through, following in the spirit of an earlier era, when the call to produce grounded theory empowered researchers concerned to reclaim their craft from the "theoretical capitalist(s)" of the day (Glaser & Strauss, 1967, p. 10). The appeal to scientific foundations made by Glaser and Strauss may no longer be as easily sustainable, but the need for a new conceptualization of the relationship between qualitative social research, theory, and indeed philosophy is as pressing now as it was in the 1960s.

Philosophy is often presented as underpinning the craft of social research, being an arena where various attempts at providing foundations for judging truth claims have come and gone, yet present day opinion seems nowadays, paradoxically, to conclude on antifoundationalism as itself being a philosophical foundation for social research. I think it is time for social researchers to exploit this paradox, by breaking free from the obligation to fulfill philosophical schemes through research practice, while remaining aware of the value of philosophical and political reflexivity for their craft. A confident view of social research as a craft skill could then emerge, relatively autonomous from social theory or philosophy, yet drawing on these arenas of discourse as a resource. The search for overarching criteria for judging quality under this vision is thereby held at a distance, and the elusive nature of quality (we somehow recognize it when we see it, but we cannot prespecify it with methodological rules) is preserved. This also means that we do not have to abandon skills developed under one paradigm because another paradigm has come along.

It is worth summarizing some key shifts in criteriology, which is a kind of offshoot of broader debates in the philosophy of science, to show how I have reached this conclusion. Then I shall make some statements about philosophical positions and foundationalism, after which I shall use the example of triangulation to demonstrate how social researchers might operate within a conception of research as a craft skill in a way that preserves a commitment to producing good quality research.

CRITERIOLOGY

Qualitative creation mythology, in the modernist phase of qualitative inquiry, emphasized difference by making overdrawn contrasts with the supposed "positivism" of quantitative work. In the methodological debate about quality criteria, this, initially, involved substituting new terms for words such as *validity* and *reliability* to reflect interpretivist conceptions while retaining a sense that social researchers in both traditions shared similar scientific orientations. A typical example here is LeCompte and Goetz (1982) who argued that techniques for establishing validity and reliability should be

somewhat different in, say, an ethnography compared with an experiment. They drew up a scheme intended as a qualitative parallel to Campbell and Stanley's (1966) influential account of validity and reliability in quasi-experimental designs, inventing new concepts such as *internal* and *external reliability* to legitimate a degree of difference that nevertheless allowed them to advocate pragmatic combinations of quantitative and qualitative work.

Qualitative criteriologists since LeCompte and Goetz progressively moved away from modernist commitments, leading to conceptions of validity and reliability very far removed from positivist or realist perspectives, as constructivism shifted into postmodernism. The urge to generate criteria for judging good quality studies seems irrepressible in these methodological writings, perhaps in part due to the requirement that social researchers impress the worth of their efforts on skeptical audiences, such as research-funding bodies.

A sometimes bewildering variety of new concepts arose. For example, Altheide and Johnson's (1994) review of interpretivist positions on validity identifies "successor validity, catalytic validity, interrogated validity, transgressive validity, imperial validity, simulacra / ironic validity, situated validity, and voluptuous validity" (p. 488). A glance at Kirk and Miller (1986), though, shows Altheide and Johnson to have omitted from this list "apparent," "instrumental," and "theoretical" validity. Additionally, Kirk and Miller (1986) demonstrate the ease with which new forms of reliability can be conceptualized, dividing this into the "quixotic," the "diachronic," and the "synchronic." This proliferation of concepts reflects the difficulties that qualitative methodologists, committed to creating some overarching system for specifying quality, have had in making their ideas stick. This is in marked contrast to parallel authors in the quantitative tradition where a consensus around certain ideas (for example, the distinction between validity and reliability, or between internal and external validity) has been more easy to sustain. In qualitative research, the project of criteriology experiences particular contradictions because of the difficulty in regulating and constraining an endeavor whose guiding philosophy often stresses creativity, exploration, conceptual flexibility, and a freedom of spirit. Additionally, though, conceptual proliferation is a marker of the paradigm shifts and crises of legitimation and representation, which have characterized "moments" in the recent history of qualitative methods (Denzin & Lincoln, 1994).

The work of Lincoln and Guba reflects these more recent shifts. Lincoln and Guba (1985) argue that establishing the trustworthiness of a research report lies at the heart of issues conventionally discussed as validity and reliability so that four questions have, from within the modernist paradigm, been asked of research reports, namely their truth value, applicability, consistency, and neutrality. Truth value, though, assumes a "single tangible reality that an investigation is intended to unearth and display" (Lincoln & Guba, 1985, p. 294), whereas the naturalistic researcher makes "the assumption of multiple

constructed realities" (p. 295). Applicability depends on generalizing from a sample to a population, on the untested assumption that the "receiving" population is similar to that of the "sending" sample; the naturalistic inquirer, on the other hand, would claim the potential uniqueness of every local context, requiring empirical study of both sending and receiving contexts for applicability to be established. They are similarly critical of the other two conventional criteria. Consistency, they say, depends on naïve realist assumptions; neutrality depends on an artificial separation of values from inquiry. They criticize LeCompte and Goetz, whom they identify with such questions, for their dependence on axioms such as "naive realism and linear causality" (Lincoln & Guba, 1985, p. 293).

Instead, Lincoln and Guba (1985) propose their own four-point criterion list for naturalistic inquirers. A concern with credibility should replace truth value and "the most crucial technique for establishing credibility" is through "member checks" (Lincoln & Guba, 1985, p. 314). Transferability should replace applicability, or external validity as conventionally conceived. Dependability is proposed as a replacement for consistency, or reliability as conventionally conceived, to be fulfilled by peer auditing procedures. Auditing is also useful in establishing confirmability, a criterion designed to replace the conventional criterion of neutrality or objectivity. Auditing is an exercise in reflexivity, which involves the provision of a methodologically self-critical account of how the research was done. Trustworthiness is always negotiable and open-ended, not being a matter of final proof whereby readers are compelled to accept an account. This, Lincoln and Guba (1985) claim, "stands in marked contrast to that of conventional inquiry" (p. 329), which claims to be "utterly unassailable" once relevant procedures have been carried out. This is, in fact, a rather overdrawn contrast if we consider the fallibilistic spirit of Campbell and Stanley's (1966) account of threats to validity and reliability.

The criteria offered by Lincoln and Guba in 1985, however, depend on a contradictory philosophical position, because the belief in "multiple constructed realities," rather than a "single tangible reality" (Lincoln & Guba, 1985, p. 295), which lies at the heart of the constructivist paradigm, is not consistent with the idea that criteria for judging the trustworthiness of an account are possible. Relativism does not sit well with attempts to establish "truth," even if the term is placed in inverted commas.

Acknowledging this problem, then, in later work (Guba & Lincoln, 1989, 1994), a fifth criterion, "authenticity," is proposed as being consistent with the relativist view that research accounts do no more than represent a sophisticated but temporary consensus of views about what is to be considered true. In detailing the components of authenticity, Guba and Lincoln (1989, 1994) reveal a sympathy for political conceptions of the role of research that was already evident in their earlier commitment to the value of member checking. Authenticity, they say, is demonstrated if researchers can show that they

have represented a range of different realities ("fairness"). Research should also help members develop "more sophisticated" understandings of the phenomenon being studied ("ontological authenticity"), be shown to have helped members appreciate the viewpoints of people other than themselves ("educative authenticity"), to have stimulated some form of action ("catalytic authenticity"), and to have empowered members to act ("tactical authenticity"). Of course, the view that fairness, sophistication, mutual understanding, and empowerment are generally desirable is itself a value-laden, culture-bound position that a Foucauldian deconstructionist might very well enjoy taking apart. It represents an attempt to pull back from the relativist abyss, but the substitution of political goals as foundations for research is problematic in a world where there is no fixed consensus on the desirability of particular goals (Hammersley, 1995). A softer version of political commitment might involve researchers simply offering readers a reflexive account of their politics and leaving it to the democratic process in wider society to resolve clashes of interest. Nevertheless, it is hard not to agree with Guba and Lincoln's (1994) following conclusion: "The issue of quality criteria in constructivism is . . . not well resolved, and further critique is needed" (p. 114).

These authors, then, along with many others in the qualitative social research community, have traveled on a path beginning with a rejection of positivist criteria and the substitution of interpretivist alternatives. Dissatisfied with the limitations of these, constructivism has been embraced, introducing an element of relativism. Political versions of the value of research have then been imported to save facing the logical implications of relativism, which seem to threaten a nihilistic vision and abandonment of the research enterprise. What is a practicing social researcher to make of all this? How can these inconclusive debates become a resource for researchers rather than a source of frustration and negativity? Before turning to this, I want to consider some more purely philosophical concerns.

PHILOSOPHICAL MOMENTS

At a philosophical level, discussions of the problem of foundations for knowledge are also inconclusive. Nevertheless, philosophical positions are sometimes claimed as being in another sense "foundational" for research practice by criteriologists. Thus, empiricism is claimed as foundational for modernist paradigms, antiempiricism is proposed as foundational for constructivist and postmodern views, and so on. I propose here to examine just one attempt to provide foundations for research practice in a philosophical position, the "subtle realist" conclusion of Hammersley (1992), together with some criticisms of this. I do not propose subtle realism as a solution (though it has attractive qualities as a pragmatic compromise between several

extremes), but want to use it to illustrate the limits of any approach that expects research practice to conform precisely to a philosophical position.

Subtle realism involves maintaining a view of language as both constructing new worlds and as referring to a reality outside the text, a means of communicating past experience as well as imagining new experiences. Hammersley (1992, 1995) presents this as an adequately grounded place for social researchers seeking a middle way between the various paradigm positions that are nowadays available. Like analytic realism (Altheide & Johnson, 1994), Kantian soft or "transcendental" idealism, and critical realism (Bhaskar, 1989), it is a marker of an approach to social research that takes the view that, although we always perceive the world from a particular viewpoint, the world acts back on us to constrain the points of view that are possible. The researcher treading this middle way is continually aware of the somewhat constructed nature of research but avoids the wholesale application of constructivism to his or her own practice, which would result in a descent into nihilism. Research, then, constructs "transitive objects" such as the concepts of social science, to represent the real (Williams & May, 1996, p. 85). Knowledge is always mediated by preexisting ideas and values, whether this is acknowledged by researchers or not. Yet, some accounts are more plausible than others, and human communities in practice have created reasonably firm grounds on which plausibility can be judged, whether or not these grounds can be supported in some ultimate sense by means of philosophical reasoning. Judgments about the plausibility of research accounts inevitably involve a temporary subscription to the view that language is referential to a reality outside the text. This is a long way from a simple correspondence theory of truth, but it contains elements of this. Neither does it claim that truth solely lies in the consistency of claims with some other set of claims, though this can legitimately be an element in judging truth claims. It involves opposition to the pure constructivist view that states there is no possibility of knowing a real world that exists separately from language.

At the heart of the advocacy of subtle realism lies the idea of a research community with agreed standards of judgment for the plausibility, credibility, and relevance of research reports. Distinguishing claims from evidence, providing the strongest evidence for more important claims, and exposing the judgments of the researcher for readers to scrutinize are all methods for addressing the standards applied by a community of critical peers. In arguing for this, Hammersley (1992) pursues an argument similar to that of Popper (1972), who claimed the authority of an imagined "third world" of objective knowledge, humanly constructed but, by virtue of being a joint endeavor of a community of scientists, having an existence independent of the biographies of individual scientists. Hammersley (1992) is also similar to Popper (1963) in advocating a fallibilistic approach, regarding "truths" as provisional until

there is good reason for contradictory versions to gain support. Hammersley is therefore firmly in a postpositivist camp.

As is well known from the criticisms of Popper that have emanated from radical epistemological and political positions, reliance on norms of communal assessment has the potential to support a rather conservative approach. We can observe that this community of researchers is not in fact an imagined thing of the mind, but a reality. Particular people do concrete things in the world and call them research. These people come from particular cultural backgrounds and bring specific, exclusive prejudices to bear in the standards that they maintain. In practice, the social research community is no different from the rest of society in its divisions of status and power, acting at times to oppress and silence particular groups who are unable to influence the discourses of social research (Harding, 1986). Hammersley's stress on whether findings are consistent with knowledge that is currently accepted in the relevant research community ("plausibility") initially looks rather dubious in this light. Against this, though, the advocate of subtle realism might point to the role of evidence in testing theories for both credibility and plausibility, which exerts a persuasive force on the research community and can result in revision of accepted wisdom and the eventual overthrow of dominant paradigms.

Yet, this is itself assailable, as it rests on assumptions about the evidentiary basis of what constitutes evidence. How should we make contact with an external reality that affirms or disaffirms claims? Is not all observation fundamentally driven by preexisting theoretical assumptions? To present subtle realism as a foundational basis for social research practice seems inadequate in light of these questions. Conventionally, there can be a turn to further philosophical work at this point, perhaps to constructivism or postmodernism, often claimed to be epistemologically nonfoundational, but nevertheless presented by some (for example, Denzin, 1997; Dickens & Fontana, 1994) as a new rationale for research practice, suggesting a foundationalist habit of thought with which I believe researchers should break.

The widespread appeal of postmodern, political, and constructivist conceptions of research is based on some fundamental dissatisfactions with the scientific world view. Quality does matter in qualitative research, but the modernist headings of *validity* and *reliability* no longer seem adequate to encapsulate the range of issues that a concern for quality must raise. The constructivist critique of criteriology has led us to see that "quality" is a somewhat elusive phenomenon that cannot be prespecified by methodological rules, though their reconstitution as "guidelines," to be followed with intelligence and knowledge of the particular research context, may assist us in moving toward good quality work. A major threat to quality is in fact the idea that research must be carried out under the burden of fulfilling some

philosophical or methodological scheme. Practicing social researchers can learn to do good work from a variety of examples, done within different "moments," without needing to resolve methodological disputes before beginning their work. At the same time, the quality of qualitative research is enhanced if researchers engage with philosophical and methodological debate, so that the pursuit of quality becomes a "fertile obsession" as methodological awareness develops and feeds into practice (Lather, 1993).

The idea of a self-critical research community acting together to produce positive knowledge for the benefit of others retains its appeal for many researchers. The continuing desire to participate in a shared language, constructing and negotiating standards for judging quality, incorporating political and cultural differences, always involves an act of trust in the judgments of others, though this can be made easier by the application of certain methodological procedures. These procedures (discussed in more depth in Seale, 1999), are based on this view of a research community existing as a key audience for social researchers concerned about the quality of their efforts. They include techniques such as the peer auditing described by Lincoln and Guba (1985). Acceptance of the researcher's case can then partly depend on the capacity of the researcher to expose to a critical readership the judgments and methodological decisions made in the course of a research study (Swanborn, 1996).

TRIANGULATION: A CASE STUDY OF A CRAFT SKILL

To illustrate an approach to research practice that cuts through inconclusive methodological disputes, retaining a conception of research as primarily a craft skill, I shall discuss a particular one of these skills. Triangulation describes a set of techniques that arose initially within a crudely realist paradigm. Attempts have been made to restrict its use to this paradigm by people who mistakenly believe in inevitable logical connections between paradigm positions and techniques. More enlightened methodologists have perceived that it has a place within a variety of paradigms. The next step in this logic, which I believe researchers should take, is to claim it as a valuable craft skill, relatively autonomous from any paradigm position.

The idea of triangulation derives from discussions of measurement validity by quantitative methodologists working with crudely realist and empiricist assumptions. Campbell and Fiske (1959) argued that "In contrast with the *single operationalism* now dominant in psychology, we are advocating . . . a *methodological triangulation*" (p. 101, emphasis in original), and proceeded to outline their ideas for the convergent and discriminant validation of measurement instruments. Subsequently, Webb, Campbell, Schwartz, and Sechrest (1966) used the idea to advocate multiple operationalism, the use of

several methods at once so that the biases of any one method might be canceled out by those of others. Its use in qualitative research, though, was first advocated and then popularized by Denzin (1970) whose textbook has been through several editions (1978, 1989) in which the original concept was modified. Other textbook definitions at times contain distant echoes of this background in the quantitative research tradition, as where Hammersley and Atkinson (1983) describe it as a method whereby "links between concepts and indicators are checked by recourse to other indicators" (p. 199). The term itself is designed to evoke an analogy with surveying or navigation, in which people discover their position on a map by taking bearings on two landmarks, lines from which will intersect at the observer's position. If only one landmark were taken, the observer would only know that they were situated somewhere along a line. Triangulation used in this way assumes a single fixed reality that can be known objectively through the use of multiple methods of social research (Blaikie, 1991). Many might feel that it is therefore a technique impossible to employ without also taking on modernist philosophical commitments within a positivist, or at least postpositivist, paradigm together with a commitment to constructing a single true version.

Cicourel (1964, 1974) offers the most extreme vantage point from which to view triangulation from a different paradigm position. His own critique of the technique is typically paradoxical, in that he proceeds by enthusiastically advocating the advantages of "indefinite triangulation" (Cicourel, 1974, p. 124). This rhetorical ploy (that I think could usefully be read now as a joke, though this was probably not Cicourel's intention) proceeds by showing that what he means by this is in fact the precise antithesis to the consensus on truth sought in conventional triangulation. His illustration comes from his own practice.

> The triangulation procedure varies with the research problem. When gathering information on language acquisition in the home setting we left a tape recorder for about one hour during lunch. A transcription of the tape was done by a typist who had been instructed to render a verbatim record. Then the transcript, the first version of this scene, was read by the mother while she listened to the tape; her comments produced another version of the interaction. The typist was next asked to listen again to the tape and to describe what she thought was "going on," correcting her original transcript as she deemed necessary. In this elaboration and correction a different version of the scene was always produced. My phonetic transcription of the tapes created still another version. . . . The reader could now say that we should have simply combined the different versions to produce the "best" one possible, but the point is that different versions could have been produced indefinitely by simply hiring different typists and providing the mother with different transcripts. (Cicourel, 1974, p. 124)

This is an amusing little demonstration of the constructivist objection to realist tendencies in discussions of triangulation. As with postmodern views, it suggests that every reading of a text is likely to produce a new interpretation, with no version assuming privileged status. Cicourel's example leads Blaikie (1991) to claim that triangulation therefore only makes sense from within a

positivist framework. Blaikie fails to see any irony in Cicourel's example ("But the question arises as to why it should be called triangulation" [Blaikie, 1991, p. 130]) and argues that the technique "has no relevance for genuine interpretivists and ethnomethodologists" (Blaikie, 1991, p. 131) because it necessarily involves subscription to inappropriate ontological and epistemological positions. I argue, by contrast, that researchers should question this claim that there is a necessary connection.

Bloor's (1997) objection to triangulation leads to a further questioning of its supposedly inevitable philosophical connotations, though it also offers an opportunity to locate this skill within a subtle realist paradigm. Bloor (1997) says that even if all the different methods in a methodological triangulation exercise converge on the same thing, apparently agreeing with each other, how can we know that they are correct? Perhaps some hitherto unthought-of method would reveal something different. In fact, this problem is analogous to that of induction: How can we reliably reason on the evidence of past experience that the sun will rise tomorrow? Logically of course, we cannot. Taken at this level, the objection to triangulation as a validation exercise is also unanswerable. Yet, we operate in the world all the time on the basis of what it is plausible to believe, and it will do us little good to assume that the sun will not rise tomorrow. A pragmatic, subtle realist might answer Bloor by saying that triangulation exercises can add to the credibility of a particular account as a part of a fallibilistic research strategy in which evidence is sought for central claims. Thus, we can move from a crude to a subtle realist paradigm as a potential set of background assumptions for triangulation. Can we go further toward Cicourel's view?

Silverman (1993) presents an argument that moves triangulation toward a constructivist paradigm. At first, he makes the subtle realist point that triangulation exercises can deepen understanding as a part of a fallibilistic approach to fieldwork, although being themselves no guarantee of validity. In a similar vein, Cain and Finch (1981) argue that multiplication of methods can help deepen understanding of different aspects of an issue. Dingwall (1997) begins the move toward constructivism by saying that triangulation offers a way of explaining how accounts and actions in one setting are influenced or constrained by those in another. Silverman (1993) eventually has no problem with this use of triangulation, finally saying that it can help "to address the situated work of accounts" rather than "using one account to undercut the other" (p. 158). This version of triangulation, then, which is now very close to Cicourel's view, gets away from the idea of convergence on a fixed point and accepts a view of research as revealing multiple constructed realities, something that triangulation, now conceived as the revelation of difference, is well suited to expose.

Flick (1992, 1998) completes this move toward what might be called a soft constructivist version of triangulation, deriving this from a study in which a

conversation analytic study of psychological counseling was complemented by interviews with counselors to elicit their accounts of what they tried to achieve in their practices. Flick (1998) argues that, used in this spirit, "Triangulation is less a strategy for validating results and procedures than an alternative to validation . . . which increases scope, depth and consistency" (p. 230).

In Seale (1999) I show, further, the uses of triangulation exercises in generating material for discourse analytic studies, thereby improving their coherence and fruitfulness, suggesting that triangulation can be used for work that is located within a poststructuralist, if not quite postmodern paradigm. It is not hard, too, to conceive of triangulation exercises enhancing the quality of politically driven research projects, whose emancipatory or enlightening effect is enhanced by the elicitation of multiple perspectives on, or constructions of, a phenomenon. That such shifts can occur in the discussion of just one of the many techniques available to qualitative researchers supports the more general point that particular craft skills do not have to be linked inextricably to particular philosophical or paradigm positions. I generalize this point in Seale (1999) to incorporate discussion of both established and newly formed qualitative research skills. These include member checking, accounting for negative instances, analytic induction, the uses of numbers, using low inference descriptors, the grounding of theory, deconstructive approaches, reflexive accounting, and new textual forms of reporting, as well as others.

CONCLUSION

Methodological writing is of limited use to practicing social researchers, who are pursuing a craft occupation, in large part learned "on the job," through apprenticeship, experience, trial, and error rather than by studying general accounts of method. Methodological discussions of the quality of research, if they have any use at all, benefit the quality of research by encouraging a degree of awareness about the methodological implications of particular decisions made during the course of a project. Intense methodological awareness, if engaged in too seriously, can create anxieties that hinder practice, but if taken in small doses can help to guard against more obvious errors. It may also give ideas for those running short on these during the course of a project. Reading and discussing such methodological ideas, then, is a sort of intellectual muscle-building exercise, time out in the brain gymnasium, before returning to the task at hand, hopefully a little stronger and more alert.

This is intended to be a rather pragmatic and skeptical orientation, reflecting the view that people learn how to do research through apprenticeship experiences, fortunately possible to have by reading others' work rather than actually going and sitting at their feet (although this also can be useful). Any contemplation of other people's research work, if it involves thinking

seriously about its strengths and weaknesses, can be this kind of vicarious apprenticeship experience. But additionally, purely methodological writing may help to structure this experience a little more, focusing on particular themes that writers believe to be of importance when considering how to produce good quality research.

I have tried to show that people often make strong claims that philosophical, political, or theoretical positions ought to lie behind—indeed ought to determine—the decisions that social researchers make "on the ground" so that quality is underwritten by adherence to a particular position. This is even the case with postmodernism, though writers occupying this "moment" in qualitative work sometimes try to present themselves as being almost entirely permissive. I see things differently: Research practice, in fact, should be conceived as relatively autonomous from such abstract and general considerations.

In treading along this path, I hope carefully and with due consideration of the great variety of conflicting positions that exist, it is possible to benefit from just about any of the key methodological discussions on how to ensure quality in social research. This includes so-called "positivist" methodology, neopositivism, political perspectives, constructivism, postmodernism, and others I may not have listed. What I would like to see is some sense of there being a community of social researchers who have respect for the strengths of a variety of positions within that community, appreciating the need also to develop research skills taken from a number of genres (quantitative as well as qualitative, in fact), in much the same way as artists learn how to paint, draw, or sculpt in a number of different styles. Then, the development of one's own "style" can build on a series of principled decisions, rather than being the outcome of uninformed beliefs. Such are the ways in which a research community might work.

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