Mind, Culture, and Activity

Criteria for the Quality of Inquiry

Willem L. Wardekker

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ARTICLES

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Willem L. Wardekker

Department of Education
Vrije Universiteit

Researchers working in Cultural Historical Activity Theory (CHAT) need to develop criteria for the adequacy of the processes and products of research that is founded on the central tenets of that theory. Criteria like validity and generalizability are not universal, but have been developed from a specific theory of knowledge use. This relation is explained both for the nomological and the interpretive paradigms, and suggestions are made for the development of criteria in the CHAT paradigm on a similar basis.

PARADIGMS AND CRITERIA

Faced with the problem of having to judge research reports submitted for publication, journal editors in the fields of educational, psychological, and sociological research find themselves more and more in an awkward position (cf. Donmoyer, 1996; Stronach, Hustler, & Edwards, 1997). It is a position, moreover, that is paralleled by the one in which judges of research proposals are often put. In "an era of paradigm proliferation," as Donmoyer calls it, there is no easy way to judge the quality of research because each paradigm claims to have its own set of criteria for that quality. To make matters worse, only the standard nomological or "quantitative" paradigm can boast well-defined and more or less consistent criteria, whereas researchers working within the interpretive or "qualitative" paradigm either refer to widely different criteria related to a host of sub-paradigms (see, e.g., Denzin & Lincoln, 1998, or Potter, 1996, for a broad but not nearly exhaustive overview) or just claim that "accepted" criteria of validity, reliability, and generalizability do not strictly apply, without offering alternatives. This situation not only poses problems for the judges of research, but also for the researchers themselves, because it is difficult for them to foresee according to which set of criteria their work will be assessed. The procedure Donmoyer proposes (having texts judged by a wide range of reviewers) may be the only practical one under the circumstances and has the virtue of being democratic, but it can hardly contribute to a solution to this problem. And indeed, there may not be a generally acceptable solution as long as the problem is formulated in this way, because reaching a solution presupposes accepting the existence of different paradigms—something that runs counter to the tenets of the

Requests for reprints should be sent to Willem L. Wardekker, Department of Education, Vrije Universiteit, Van der Boechorststraat 1, 1081 BT Amsterdam, The Netherlands. E-mail: wl.wardekker@psy.vu.nl

nomological paradigm in the first place. But if we cannot hope to convince everybody, we can at least try to show that the present situation is not the only possibility.

The "paradigm wars" cannot be stopped by adopting a practical, democratic, or pragmatic stance. We need to formulate the problem that keeps the camps divided and then find a way to re-formulate it in terms that would allow us to see some kind of synthesis. This is a task that Habermas took upon himself in trying to show that critical theory could provide such a synthesis, but his analysis seems flawed (cf. Miedema & Wardekker, 1999). I try to show, without being able to give a full account, that cultural-historical activity theory (CHAT) might be able to do better in this respect.

This would in the end solve a big problem for those researchers who consider themselves members of the community basing its work on CHAT. Their situation is even more complicated: in this group, some researchers keep to the standards of the nomological paradigm, although in many cases with some uneasiness; others adopt a form of interpretive research, for example in the narra-tive tradition; whereas some feel that CHAT researchers would do best to develop their own set of criteria, that is, to position CHAT as a paradigm in its own right. My position does not coincide with any of these; rather, I intend to point out the possibility that we might understand all of these positions as different instances of one principle. Such a step, if realized, would not make the "par-adigm wars" go away, but it would make them less interesting and instead point our attention to-ward different problems and difficulties in the realm of research methods.

The following, then, is offered in the hope of providing an incentive for further discussion in which we reconsider the presuppositions of such criteria as validity and generalizability to find reformulations and starting points for new specifications of criteria. The gist of this contribution is that concepts like validity, reliability, and generalizability do not have a fixed meaning and do not by themselves define the scientific character of research. Rather, they form part of a paradigmatic discussion in which their seemingly objective character is used to exert power. Although I will fo-cus on research in education, specifically research that intends to be of help in changing teacher practices, the implications are not restricted to such research. I hope that others will join in by spelling out such implications.

HOW TO COMPARE PARADIGMS

Normally, we talk about the "paradigm war" in social studies as waged between two positions: the nomological and the interpretive paradigms. I will conform to this usage, but we will have to ask what exactly we mean by speaking about paradigms here and how to understand the difference be-tween the two.

I have assumed that using the concept of paradigm is valid for the social field, even if its origi-nator, Thomas Kuhn, would say that in our field of study there are no paradigms, just a confusing number of schools of thought. For him, probably, behaviorism and cognitive psychology would have qualified as different paradigms, except for the fact that neither of these theories ever could be considered "the" way of thinking in the social field. Others will indeed maintain that there are many theoretical paradigms, but not all of them claim specific methods and criteria for research quality. If I speak of two paradigms here, I use the term *paradigm* in the field of social studies in a stricter sense than Kuhn did. To be able to speak of paradigms *in this sense*, it will not be enough to point out a difference in theoretical principles, even if these entail some specific criterion (such as,

in certain branches of cognitive psychology, the claim that a good theory must lend itself to computer simulation).

But neither is it enough to claim a methodological difference. Sometimes the "paradigm wars" are equated with a controversy between quantitative and qualitative methods. Although there is some truth in this, it does not reach the core of the controversy. Methods may be chosen according to the implications of a research question. Paradigms, however, are ways of thinking that generate certain types of research questions and therefore lead to different interpretations of the results obtained by such methods (Smagorinsky, 1998). I would like to reserve "quantitative" and "qualitative" for research methods in which the categories into which the data fall are or are not prespecified by the researcher. There is then no necessary connection between paradigms and methods: Qualitative methods may be used within a nomological paradigm, for instance, and I rather think that both quantitative and qualitative methods may be used in CHAT-based research. It is not the character of the methods themselves that defines a paradigmatic difference, but the way in which the results are interpreted and used.

Often the difference between the nomological and the interpretive paradigms is seen as an opposition between explanation and understanding. I do not think that is adequate. Such a distinction is rather superficial: It tells us there is a difference, but does not explain why. Logically, it does not seem necessary, as it is perfectly possible to see beliefs and reasons as "causative" factors in an explanation of behavior (see, e.g., Bhaskar, 1979). It seems directed more against behaviorism than against the dominance of the nomological paradigm as such. This is probably one of the reasons so many people now maintain that there is no need for a strict opposition and the paradigm wars ought to be over.

Indeed, if the difference were situated only in either the intentions of the researcher in producing a certain type of contribution to a "body of knowledge" or in the way human behavior is theoretically understood as either caused by external factors or guided by beliefs (surely, it can be both, dependent upon the situation), there would be no reason to speak of two competing paradigms any more. But the paradigmatic difference as I understand it runs deeper. In my view, the difference between paradigms is not based on how research is done nor on what is researched, but on why it is done. It is based on two different views of how the results of research may contribute to better human (cultural) practices, on two views of the relation between knowing and acting—in short, on two different views of learning in which research results are the input of the learning process. Or, to put it differently, it is based on two views of how potential users of research results may be convinced and what this will mean for their future behaviors. Even the idea of contributing to a body of knowledge as a goal of inquiry is to some extent interpreted differently within the two paradigmatic views, because it rests on a specific idea of what potential users need. These views, which I explicate, in their turn are part of an ongoing controversy about how to define and study mankind—as primarily natural or primarily cultural—that has given rise to "two psychologies" (Cole, 1996) and that ultimately rests on a Cartesian dualism. It is exactly because CHAT theory offers a third view of this learning process between research and practice that it can aspire to bridge the gap between the two. This implies that evaluating CHAT research on the basis of criteria developed within one of the other paradigms exclusively means that the full potential of that theory cannot be actualized.

In the following I explicate the learning theories implied in these paradigms and what these mean for the criteria for "good" research and for good research reports. I need to emphasize that these explications do not in any substantial way rest on an analysis of what researchers actually do

or how they understand what they do. Rather, I try to reconstruct the logic behind what is considered good research from a certain point of view. At the same time, I will point out some of the less desirable consequences of these points of view, as made visible by the reconstruction.

THE NOMOLOGICAL PARADIGM

The *nomological paradigm* rests on a rational, or rather a logical, image of mankind and its (everyday) practices. It says that people want to, or at least should want to, act in the way that is the most effective in a given situation and with given aims. *Acting* means creating the conditions that enable a natural law to work in a way that will produce the desired results. Thus, a teacher creates conditions under which learning processes, in a way, must happen. If students do not learn, that means the right conditions for those processes were not created in that situation. Some of these conditions may be in the teacher's sphere of influence; some are not. But generally, the more the teacher knows about the situation and the processes she desires to happen, the more she will be able to create the right conditions. Competent and effective actions are supposed to be based on adequate knowledge. This knowledge, in most cases, is distributed. The teacher herself does not need to know everything about learning processes; she can leave the details to educationists and textbook writers. All she needs is some recipes for acting in specific situations and the ability to determine what the relevant characteristics of a given situation are. And even that ability may be distributed, for instance among a testing service.

In such a view of practice, no attention is given to its historical and cultural aspects, which construct the situation in a specific way and thus guide actions. If anything, these are viewed as obstacles to rational behavior. Practice is conceived of only in terms of individual or collective ends and the rational means to reach these ends. Better practice is practice that is guided by better knowledge of pre-existing laws that are independent of human action and better ways to handle constraints and restrictions that apply in a specific situation, constraints that may be human constructions, like laws, funding problems, and the like.

Knowledge is the operative concept in this approach. The effectiveness of actions depends on the adequacy of our knowledge of two things: first, of the natural laws that can be put to work for us so we may obtain our goals, and second, of the actual conditions in a specific situation that need to be manipulated. In both cases, empirical research is meant to deliver the most adequate knowledge possible. *Adequate* here means giving an accurate description of laws, forces, and conditions as they are (at least, as far as we can know). Research reports focus on convincing the reader that the results are accurate in that sense; that the world could not be different from the description given here. Note that knowledge is considered to be a commodity that is transferable from one situation to many others (cf. Cobb & Bowers, 1999).

What is considered an adequate product of research, then, is determined by our notions of the way it could and should be used in practice. Thus, the criteria for good research ultimately refer to the use that is (or rather, ought to be) made of its results, even in the nomological paradigm—that is, they rest on an image of good learning and good acting, on what Bruner (1996) called a theory of mind. This is also true of the interpretive paradigm—although its image of good learning and acting is different. But in both the nomological and interpretive approaches, this foundation of methods and criteria is seldom made explicit. Neither, in the nomological paradigm, is this image of the good research user itself subjected to scientific scrutiny, for reasons that I discuss.

To the nomological way of thinking, such an explication is not even really necessary. For its underlying theory of mind says that individual rational people act according to the knowledge they possess. They can act differently, for example on the basis of neurotic compulsion or of unanalyzed feelings, but that is irrational behavior that is itself in need of an explanation. They can also understand public knowledge differently, but in that case they make an error, as there is really only one adequate way to interpret knowledge correctly. In that sense, adequate knowledge is thought to be objective, that is, independent of personal interpretation. Knowledge is considered to be best when it is valid without restrictions as to time or place, which means that "good" knowledge is useable in many cases that were not foreseen when the original research was done. Production and utilization of knowledge can be separated. The practical usefulness of knowledge is thus implied in the model and not really a criterion. The attention can be transferred to the mode of knowledge production: if the production process is adequate, the resulting product will be too—hence the emphasis on validity, reliability, and generalizability. Such criteria are concerned with knowledge production, and their discussion is limited to the circle of co-researchers, but ultimately they are based on a theory of the mind of the knowledge user and aim at establishing the rationality of using the results. They focus on ensuring the context-independency of knowledge exactly because such knowledge is deemed to be the most useful.

The separation between production and utilization is reflected in the separation of the empirical and the practical. Research is concerned with the empirical: a situation created for the purposes of research in which natural laws and their conditions will become apparent. If research is done in an educational situation, for instance a classroom, this does not by itself make the research practical or practice-oriented, because the researcher sees only those aspects of the classroom that are relevant to the research question. To the nomological researcher, a classroom is not a practice situation, but a place where data are to be found. Such research does not, to paraphrase Lewin (as cited by Cole, 1996, p. 225), take its point of departure in the subject's life space; instead, it changes that life space to fit the requirements of a predefined set of research conditions: it makes an experiment happen in a non-laboratory situation. The distinction is essential to good research in the nomological tradition, for if teachers were to begin to alter their practice in the course of the research on the basis of its results, or even of the fact that research is being done, this would "contaminate" the data, which would then become useless (Smagorinsky, 1995). Data should describe the world as it is; they should be objective in the sense that they are the product of the state of the world and of nothing else. Most methodology is there to ensure that this really is the case, and it is not accidental that most methods books describe the controlled experiment as the paradigmatic case of research. The greater part of the research report is meant to convince other researchers that this condition of objectivity has been rigorously observed. The way research reports are structured (say, along the lines of the American Psychological Association's scheme) ensures that the discussion is focused on the validity of the data, not on the practical use of the theory.

Of course, educational research does not escape the pressure to justify itself exactly by its practical relevance. But the classroom as a practice situation often comes into focus only after data have been gathered and conclusions have been drawn. At that moment, implementation projects may be started to show teachers what, according to the research results, a rational way of conducting educational practice is. Such projects are almost by definition one-sided because the researcher knows something he wants the teachers to learn and use. The premise of such projects is that teachers will want to act rationally and will thus adopt the researcher's knowledge. If they construe the situation differently, that just goes to show how irrational human beings can be. If re-

search results are not used, this does not reflect on the criteria for knowledge production and only in a limited way on the content of theories; instead, innovation strategies are devised to make utilization happen. Teachers can, by definition, never be more rational than researchers. This also makes for a severe limitation in those cases where research is done under the "ownership" of practitioners—either in the form of "practice-oriented" research by researchers hired on a project basis or of research done by teachers themselves within the nomological paradigm. Such research, which has no option but to take the problem definition of the practitioners involved for granted whereas still couching it in terms of general laws, will often produce no more than short-term problem solutions without wider relevance (Edwards, in press).

If teachers cannot be more rational than researchers, neither can pupils be more rational than teachers. It is clear that this way of thinking, this theory of mind, also has influence on the curriculum. It indicates that pupils should acquire models of the world as it really and objectively is—that is, as science says it is. Recent innovations based on (a Piagetian brand of) constructivism, in which self-regulation is the central concept, do not change this; they just say that such models have to be acquired by actively constructing them, not by passively receiving them, and that the conditions for this construction vary according to individual development. Still, the preference in the curriculum for teaching objective knowledge that allows for only one correct interpretation and precisely because of that can be used in many situations remains intact.

Both in research and in teaching, however, this way of thinking in terms of generalizability as a first criterion of knowledge has come under attack. The concept of ecological validity has proven to be very complicated. The work of Cole and associates (Cole, 1996; Newman, Griffin & Cole, 1989), who were virtually unable to make the same psychological task happen in different (experimental) contexts, gives rise to the idea that context is not just the specific circumstances in which to look for the working of general natural processes. Instead, context is what defines the importance of aspects of a situation. However, within a "natural psychology" (as opposed to a cultural one), the concept of context proves very elusive, because in reality what is important in a situation is dependent on the interpretations of the actors involved. Unless we have good reasons to believe that the interpretations of different actors are essentially the same (and the work of Cole and associates casts doubt on exactly this supposition), this means that providing them with decontextualized information will not automatically coordinate or "rationalize" their actions. The image of the "rational" user that is behind the paradigm clearly needs some refinement. However, this cannot be done with the means for theory-development that this paradigm has at its disposal, for these can only lead to a general theory of interpretations, so that the argument becomes circular.

Essentially the same problem comes to light in the concept of self-regulation. Self-regulation in fact tells pupils to act in such a way that they become subject to the laws of learning. Thus, pupils are at the same time seen as subject to natural (learning) laws, and (in their role as users of scientific information) as rational actors who can use natural laws to their advantage. In other words, human agency is assumed, but goes unexplained. (For a Deweyan critique of the dualism inherent in the concept of self-regulation, see Prawat, 1998.) Rationality and agency are not categories that can be subsumed under the concept of general law.

This problem is recognized within the paradigm. It is, in essence, a consequence of the fact that it has been transferred from natural science (where it is perfectly adequate) to the study of human beings, where the researcher, the subject, and the user can coincide. The standard solution is to point at the difference between the context of justification and the context of use, with research being done only in the former. A different formulation for the same principle is that the paradigm as-

sumes a methodological determinism, but not an ontological one: in theories, human actors are regarded as if determined by laws, but we don't have to believe that they really are. As can be inferred from the above examples, however, this solution is inadequate.

THE INTERPRETIVE PARADIGM

The interpretive paradigm rests on a totally different image of how research contributes to better practice. This is because its theory of mind is different. The theory implicit in the *interpretive paradigm* is of human beings as interpreters and constructors of a meaningful world. Humans think about themselves, about other people, and about the world and attach meaning to everything they encounter. These meanings guide their practices. They are essentially individual constructions, although coordinated with those of others through common elements. People do not discover the world, as in the nomological paradigm, but rather construct it. Of course, this construction is not seen as totally free; it has to take into account the physical properties of the world and the constructions of other people in the same cultural group. Also, it has a history and is built up through personal experience and meaning-making. Still, meaning is always bound to the individual and to specific situations; within the interpretive tradition, supra-individual culturally and historically formed narratives (including the "body of knowledge" as produced by scientific inquiry) are mostly seen as just so many possible sources of elements for individual construction processes. Among other things, this implies that "knowledge" in the traditional sense does not guide actions unless and until it becomes "contextualized" within the (re)construction of a complex of meanings.

Within the paradigm, opinions differ on how exactly people construct and maintain meanings and complexes of meanings and on how to reconstruct these in research. This has given rise to a host of sub-paradigms, for example, phenomenology, ethnography, and narrative research. Each of these has its own rules and criteria for securing the quality of the "data," that is, the researcher's interpretation of meanings. This situation reflects the fact that the whole paradigm itself rests on an interpretation of what is human about human beings, and this basic question allows for differing answers. In other words, it reflects the self-reflexivity of the paradigm, a property we found missing in the nomological paradigm. This implies that there cannot be the same kind of discrepancy between the image of humans as created in theory and the image of the potential user. But it also implies that what the nomological paradigm traditionally considers (good) research also rests on an interpretation, and there can be multiple interpretations, each with their own methods and criteria. In a sense, therefore, it is against the very nature of this paradigm to suppose there could be common criteria, even between the sub-paradigms, but it is possible to discern guidelines for research that are based on the nature of the way meanings guide our actions.

Given this situation, it is (according to the interpretive way of thinking) impossible for another person (e.g., a researcher) to convince anybody by purely "rational" means (in the sense of the nomological paradigm) that he or she has a better story about the world and that actions should be guided by that story rather than the one the first person has constructed for herself. The rational and objective knowledge as produced by the nomological paradigm will only be incorporated into a person's construction of the world if that person is able and willing to integrate it, to give it a meaning within that construction. In other words, nomological knowledge can at most guide actions in a heuristic way. But nomological knowledge is not privileged in this respect; other sources may be just as influential. In fact, whereas the nomological paradigm tries to convince potential

users that the world could not be any other way than it is depicted in the research results, the interpretive paradigm tries to show the user that her interpretations of the world could be different, without asserting that any particular interpretation is "better" in a general sense. The criterion for whether a particular insight will influence future behavior is not its truth or objectivity, but whether a person feels that a particular interpretation of the world contributes to his or her own practical wisdom or *phronesis* (cf. Kessels & Korthagen, 1996); thus, its impact is dependent on the person's life history. Learning is not a purely cognitive phenomenon; rather, it is directly connected to the development of personal identity. Note that this is a very individualistic picture of learning that does not make a lot of provisions for the grounding of learning in cooperative practices. As a consequence, it may tend towards relativism.

What research in the interpretive paradigm does is to find out what meanings people construct, how they construct them, and how these constructions guide their actions, with the intention of providing "heuristic schemes" that people may or may not use. Research does not contribute to an ideally ever more consistent "body of knowledge," but to a relatively loosely ordered collection of such interpretive schemes. Such schemes should offer new ways of seeing and interpreting familiar events (Edwards, 2001), but it is not implied that a new way is also a better way. The only people who can decide whether it is "better" are the people who do, or do not, incorporate the schemes offered into their own life histories. Therefore, the criterion for the adequacy of such schemes can never be that people actually use them, because this is up to the meaning-making of actual people and may differ from situation to situation. As in the nomological tradition, then, the actual use that is made of the products of research in practice does not play a significant role in the research itself. Criteria are based instead on projections of what is most likely to be of value to potential users. Hence, the "heuristic quality" of the research report, its power to enlighten people by making them really understand the narratives that were the object of study, can be an important criterion. The pleas for "thick description" in research reports, for instance, are based on such projections. The research process itself, on the other hand, is guided by the idea of neutrality; that is, the outcome should be an understanding of the interpretations of the research subjects in which the interpretations of the researchers play as small a role as possible (although they may, in some sub-paradigms, be considered an adequate or even necessary starting point). As in the nomological tradition, the ideal is for the researcher to be transparent or invisible. The researcher is never a participant in the sense that by her actions the narratives must not be contaminated. A paradoxical consequence is that, although the intention of the research is to provide people with heuristic schemes in an area chosen and delineated by the researcher, and although the researcher may be considered an expert on such schemes, nevertheless the schemes offered as a product of the research can never be those of the researcher himself. Most of the methods in the interpretive paradigm (as far as they go) are intended to insure this form of neutrality and distance.

In interpretive research, then, the concepts of validity and objectivity can be used, but their meaning is restricted to a methodological one: the researcher's rendition of the subjects' stories should be adequate, but this does not imply anything about the quality of those stories themselves in terms of the effectiveness of behavior guided by them. For that reason, many researchers in this tradition prefer to speak of "adequacy" as the sole criterion for research. One consequence of this is that an "outside" critique of interpretations, such as a critique of ideology, is impossible. This contributes to the tendency towards relativism that is inherent in the paradigm, and it is the main point that the "critical" paradigm tries to remedy. I will not go into a discussion of that proposed

paradigm here (but cf. Miedema & Wardekker, 1999) because I think that CHAT offers better possibilities in that respect.

In sum, the interpretive paradigm manages to avoid the type of inconsistency that we saw the nomological paradigm lead to. However, it does so at a cost: it lacks the possibility of explaining why people come to certain forms of interpretation and meaning and not to others, especially in view of possible (natural or social) constraints; and therefore it equally lacks a means to criticize interpretations from an outside point of view. As a consequence, potential users are largely left to their own devices: research offers no guidance as to what could be "better" interpretations; users are seen as totally free in adopting meanings and therefore as responsible for their own practices. Put differently, the paradigm is unable to offer a realistic theory of the learning of meanings, including the meanings offered by its own work.

THE POSSIBILITIES OF CHAT

Vygotsky himself already intended to find a way to combine the "two psychologies" of his day (Vygotsky, 1997). Although we would now express the basic tenets of both paradigms and their opposition in a different way, I think that the way of thinking that is present in his theories and more recently in CHAT does indeed give us a possibility to look at the problem in a fresh way. As CHAT is very much a theory under construction, it is impossible for me to give an account of it that covers all of its currently existing varieties. I refer to what I see as a promising interpretation, but I have no doubt that additions and changes will be necessary or even that totally different interpretations exist, which may also necessitate different inferences in the area of methods.

Central to my rendition is the "genetic approach" (cf. Wertsch, 1985). Here, the human mind and human individual personality are seen as the result of a development process in which culture and its history, represented in and mediated by cultural artifacts, are primary. Humans develop into individual personalities whose actions are guided by identity narratives by learning to participate in communities of practice that are related to historically developed and developing activity systems. The nature of this learning process, both in terms of the affordances and constraints offered by specific activity systems and in terms of the ways individual learners give meaning to their participation in them, is of paramount importance for the development of human agency.

This theoretical view of development and learning is, of course, also valid for the situation in which potential users learn from work done in the scientific community. Thus, whereas in the other two paradigms this kind of learning is not (and cannot be) reflected within the paradigm itself, in CHAT the way users are convinced of the quality of research and its "products" becomes itself an issue that is open to theoretical and empirical discourse. CHAT theory can become self-reflexive in this way. It does not need to rely on a preconceived image of a mythical generalized user. Instead, it can adjust the criteria for research (and for divulging its results) according to its theory-driven image of how learning processes are enhanced.

This does not mean that in CHAT we can use either nomological or interpretive categories and criteria according to what a research situation seems to demand. Let me remind the reader that the discussion is not about whether in some situations people (potential users, say) have more or less freedom to construct their own views of the world. Instead, it is about the question of whether an adequate world view is based on "objective" knowledge or on culturally held (and possibly relativistic) meaning systems. And although I certainly do not want to give the impression that all

CHAT-oriented researchers agree, or should agree, on this issue, I think that, based on research done within this orientation, a strong case can be made for the statement that CHAT does not fully endorse either view of learning. In fact, we can probably say that both are at best incomplete because they imply an abstract view of human beings, abstract in the sense that in both views humans are seen as individuals unrelated to actual human activities and the communities in which these activities are practiced. By relating "adequacy" to the concepts of activity, practice, and community, we can establish a way of thinking that ultimately can overcome the limitations of both "paradigms." Doing this leads to a rather pragmatistic twist in our thinking about the aims of social research.

People interpret the world in specific ways not just because that interpretation is "objective" or because everybody else thinks the same way. Interpretations are held because they enable us to participate in specific practices that have meaning for us. Interpretations are changed if and when they enable us to participate more fully and thus at the same time produce a better practice. As Rogoff (1995) said, learning always takes place on three planes at the same time: the community, interpersonal, and personal planes.

The criterion for what constitutes a "better" practice cannot be whether the participants have learned to do as the researcher told them. Neither, on the other hand, can it be that all participants feel satisfied with the outcome, even if it is important that they are. Nor is there a logic of development for practices, some sort of absolute direction or end point as in Piaget's theory, against which to measure the outcome. So what sort of criteria do we have? How can we escape the danger of relativism inherent in the interpretive paradigm?

Every practice may be seen as a culturally structured way to reach the specific ends of an activity, or in other words to satisfy specific human needs. This has to be done under certain constraints, some of which are culturally imposed, some dependent on the situation, and some of a physical nature. A better practice may be defined as one that succeeds in reaching its ends in a more complete way while still meeting all of the constraints. Of course, we should keep in mind here that every activity is complex as to its ends. Moreover, both the ends and at least some of the constraints are cultural co-constructions and either they themselves or the way we understand them may be changed, for instance as a result of a scientific research project. As a consequence, there is no easy or quick way to determine whether a particular practice has improved in this way, but we can usually find at least some indications. On the other hand, most solutions create side effects and new constraints. Especially, we should not look for quick and easy solutions because generally those will not satisfy all of the constraints as perceived by the participants. That is exactly what happens when the recipes that are the product of traditional nomological research are presented to a practice: the practitioners will often feel that such a recipe is either only a partial solution to their problem or does not take all the constraints into account (cf. Robinson, 1998).

As in the nomological and the interpretive paradigms, this image of what constitutes the aim of research cannot in itself be a criterion for good research: that would require us to know before the fact that a positive change in practices will occur as a result of doing a research project. Criteria for the quality of research must aim at optimizing the impact of research, but even the best thought-out projects may not have the intended consequences. I do not feel capable to propose a fully adequate set of criteria that might be used in designing and evaluating research projects or programs at this moment. However, in the following, I offer some explorations into possible lines of thinking about such criteria. Whether this will make journal editors any happier remains to be seen.

DEVELOPING CRITERIA FOR RESEARCH

Even if research is focused on the personal plane, we should be aware of the community level of activity and of the dynamic character of that activity as the context of the personal plane. In other words, research is not about a stable and objective world or about stable narratives of it, but it is always about change and learning and its relation to actions. In the other paradigms, change as a result of learning comes after the research has been done; in CHAT (or at least, in my interpretation of it), learning and the resulting change are the object of the research, and a state of the world, or of a person, at any given moment is interpreted as the result of change and development. Thus, the product of research is not knowledge in the sense of a product that can be transferred to other persons and situations; it is an understanding of the change processes in a specific situation that may or may not have implications for other situations. Knowledge is a mediational means for focusing our attention on specific aspects of a practice.

Research is focused on practical activities organized in activity systems. It should take into account both the historical and the actual dynamics of that practice. In other words, it should be both genetically and contextually adequate. As these dynamics are the result of meaning-making discourses, research should be sensitive to the socially constructed character of activity structures. Much of the standard educational research does not do this. For example, lack of motivation in pupils is often attributed to a lack of competence or even a flawed character structure of the pupils, instead of to specific qualities of the school system. Such theories are not contextually adequate. I also quote Gudmundsdottir's (2001) paraphrase of Bakhtin: "School practice is overpopulated with the intentions of others. Forcing it to submit to one's own intentions is a difficult and complicated process. Every action and every word carried out within a role (be it as teacher, as pupil, or as researcher) is only half the individual's and half the tradition's." This implies that our knowledge of activity systems is contextual, as opposed to the decontextualized (or rather, seemingly decontextualized) knowledge in the nomological paradigm. However, this contextualization is not limited to the actual context-as-experienced, as in the interpretive paradigm. In CHAT research, the context includes the global, political, and historical aspects of the activity system under study. The way in which we try to change the activity should take account of this.

From all this, it becomes evident that research cannot be seen as the disinterested study of a practice. Instead, the results are the product of the interaction between two practices, the research practice and the practice under study. This draws our attention to the fact that change is not only induced after a research project has ended. Whenever a researcher enters a practice with her instruments, it creates a potential for learning and change. This implies that both the results of measurement and the direction in which the practice is going to change are co-constructions of the researcher and the practitioners, mediated by the institution and its history. In a sense, research is always a dialogue between a researcher and the respondents: they form a "community of inquiry," whether they realize it or not. This makes the researcher co-responsible for both the research results and the practice changes. In that sense, no objectivity is possible, and all research constitutes an intervention in a practice. As Smagorinsky (1995, 1998) showed, an idea about the desired direction of change is always already implicit in measurement instruments. This does not mean, however, that this idea is the only one possible. There is no pre-given or logically ideal *telos*, which implies that any implied *telos* is the co-responsibility of the co-researchers.

That "data" are a co-construction leads to the idea that transformational, collaborative research (see, e.g., Cobb, 1998) may be seen as the paradigmatic form of research, just as the controlled ex-

periment is the paradigmatic form in the nomological paradigm, because the essential elements are made explicit there. Especially important is that this type of research tries to use the dialogue between the researcher and the practitioners to its fullest extent. Whereas in other forms of research the instruments limit the dialogue to specific prescribed forms and topics, in transformational collaborative research the dialogue is the method. We should note that the term *dialogue* has a wider meaning here than usual. It implies not only talking, but also acting together, and in it the wider cultural and historical contexts of both the activity system and the participants resonate, so that it is probably more accurate to speak of a polylogue. Moreover, the goal of this polylogue has not been one-sidedly determined by the researcher who wants to assemble data, but it is about possible development of a practice. Its intention is to establish a "discursive rationality" in which alternative practices are discussed and tried out.

This, however, does not automatically mean that all research projects should have the form of transformational collaborative research. Cole (1996, p. 256), for instance, mentions that in some cases, an existing activity structure may be strictly enough regimented to enable us to do quasi-experimental research that is still ecologically valid. However, I doubt that this is the case in education (but more so in countries with a very centralized education regime, such as the National Curriculum in England and Wales). Also, in many research forms members of the community of practice may be present only in a virtual form, as partners in a virtual polylogue. But even in such cases, it means that we should never regard data as objective descriptors of an independent world. It also means that we, as researchers, cannot be indifferent to what happens to a field of practice after we leave. Having ideals about that practice on the basis of a critical consciousness is essential to research. As I argued elsewhere (Wardekker, 1998), a critical view of existing practices and an ideal concerning good practice that transcends the existing are integral parts of "scientific concepts." Research is inherently ethical.

The introduction of research instruments into a practice, including dialogue between the researcher and the practitioners, is itself change-inducing. The results of using these instruments are co-constructions. Therefore, as Smagorinsky (1995) made clear, instruments and their use should be adequate for the way the participants construct their practice and for the possibilities they have of using the instruments as elements in a Zone of Proximal Development (ZPD). Note, for instance, the comments on Luria's well-known project among the peasants of Uzbekistan (Smagorinsky, 1995; Wertsch & Tulviste, 1992). Luria's conclusion, that these peasants functioned on a lower level of intelligence because they had not been involved in important cultural changes, is flawed according to these comments; it is an artefact of the research instruments that were outside the cultural sphere of the respondents, and thus outside their ZPD, so no adequate co-construction could take place.

The aim of research is not to find "objective" knowledge about a practice or about the natural laws that govern that practice, knowledge that others then can use to develop their practice by transferring it to their own situation. Instead, the development of practice by way of establishing a "discursive rationality" is the aim of the research project itself, and researchers should be aware of this. Therefore, co-establishing an adequate setting for the project is an essential part of research to which a lot of time and attention must be devoted. Also, conceptual, attitudinal, and behavioral change should be monitored, and adequate categories for the description of changes should be developed. This should lead to a deeper understanding of the change process as one of the project results. Finally, a convergence of interpretations of the change process among all participants (including the researchers) may well be an indication of a successful project. The influence of re-

search in practice is most evident in transformative collaborative research, but even if the dialogue between researchers and practitioners takes a more virtual form (as in theoretical work), the researcher cannot be indifferent to practice change. This is not a moral exhortation; instead, it follows from the very nature of the relation between research and practice.

The aim of research, however, is not purely to change a given practice. We want changes to endure beyond the limits of a research project; we also want changes to have some impact outside the specific research setting. Therefore, I conclude with a number of suggestions to realize this.

1. To be able to work toward a better practice and still evaluate the results, researchers need to strike a balance between participation and distantiation. They have to be stimulating partners in a dialogue, but at the same time they must not be carried away by their enthusiasm. The French sociologist Touraine, for instance, has developed a method in which he splits up these functions over two persons, one the participant and the other the analyst (cf. Dubet & Wieviorka, 1996).

2. The participants in a change process should be able to maintain and further develop the resulting changes in a practice after the researchers have left. That is, the new practices should become part of their zone of actual development. Sustainability (Cole, 1996) is a criterion in the evaluation of research projects.

3. Ideally, the results of a project should not be limited to just the situation in which that project took place. Although results are not transferable in the classical way, or "generalizable," they ought to have "generative power." This power depends, among other things, on the balance between results and investments, the question of whether in other situations the same or at least recognizable constraints apply, and the way in which the results are made known to others.

4. Publications about research projects should make clear in what way these questions and criteria were met and how the researcher made sure of that. They should also offer enough ideas and heuristics so that others may be inspired to try something along the same lines. However, publications are probably not the most effective means to generate "transfer" to other practices. Changes in practices spread according to an "ink blot" model, as more people become acquainted with them and use them as positive heuristics. Publications generally do not have much power to influence the identity narratives of people in that way. They can, however, draw attention to possible sources of inspiration.

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