

## Theory, method, and analysis in research on the relations between peer collaboration and cognitive development

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### **Abstract:**

It is important to make explicit the theoretical framework within which research is conducted because one's choice of theory constrains the choice of what to study and which methods to use for doing research. From a perspective informed by the writings of T. Kuhn (1962), S. C. Pepper (1942), and L. T. Winegar (1997), the author assessed the extent to which each of the articles in this special issue related theory to methods and to analysis. The author identified limitations in each of the articles, caused primarily by the use of methods or analyses that did not seem well suited to the theory of choice. However, each of the articles also had great strengths, and, taken together, they further our understanding of the complexity of the relationships between peer collaboration and cognitive development. They also provide some stimulating and productive ways of studying the collaborative processes themselves.

**Keywords:** Cognitive development | peer collaboration | worldviews

### **Article:**

Angela O'Donnell, in her overview of this special issue, wrote, "The objective of this special issue is to illustrate how the theoretical framework one adopts with respect to understanding peer learning and collaboration has crucial influences on what is studied and how" (p. 3). John Richters (1997), in a provocative and insightful article, used the Hubble telescope as a metaphor with which to castigate psychology for being insufficiently concerned with conducting research in which the theory being espoused has clear links both to the methods being used to collect data and to the ways in which the data are analyzed. O'Donnell's is thus a timely call for a greater concern for theoretical, methodological, and analytic consistency. My feeling is that, unlike in real life, the marriage of incompatibles should be immediate grounds for divorce.

Why is there such a concern? As early as the 1920s, Vygotsky realized that the development of new theories in psychology required the development of new methods. The reason is that

theories have links to different conceptions of the world, the way the world works, and how to understand that world—in short, different worldviews or paradigms (T. Kuhn, 1962). That discussion, at least in terms of its impact on psychology, can be traced back to the work of Pepper (1942), who described four “world hypotheses”—formism, organicism, mechanism, and contextualism—that deal with scholarly approaches to knowledge. Each of these positions comes with its metaphor for how development occurs, such as the holistic unity of the developing organism or the mechanism of a watch.

In the United States for the second half of the 20th century, greatest interest was shown in the worldviews of organicism and mechanism, corresponding to the theories of Piaget on the one hand and behaviorism and social learning theory on the other. Deanna Kuhn (1978), for example, focused exclusively on organismic and mechanistic approaches to the discipline when asking whether there was one psychology or two. Overton (1984) questioned whether those two approaches “provide the only possible basis for research programs in psychology” and answered “no” to that question. However, he went on to argue that neither formism nor contextualism could “form the basis for a scientifically viable research program” (1984, p. 217). By contrast, Altman and Rogoff (1987) argued that each worldview has its corresponding domain in psychology. They linked *formism* (renamed a “trait” worldview) to the theories of Freud and Erikson and *contextualism* (renamed a “transactional” worldview) to Gibson’s ecological theory and to Vygotsky. Moreover, Altman and Rogoff pointed to the different units of analysis, approaches to causation and development, views on the nature of the observer-observed relationship, and the foci of study associated with each worldview.

Recently, Guba and Lincoln (1994) argued that “questions of method are secondary to questions of paradigm, which we define as the basic belief system or worldview that guides the investigator, not only in choices of method but in ontologically and epistemologically fundamental ways” (p. 105). They examined the ontology, epistemology, and methodology associated with positivism, postpositivism, critical theory, and constructivism. Those whose worldview is *positivist* or *postpositivist* believe that reality is essentially real and knowable, albeit probabilistically knowable from a postpositivist perspective. By contrast, *critical theory ontology* holds that what counts as “reality” is “shaped by social, political, cultural, economic, ethnic, and gender values; crystallized over time” (Guba & Lincoln, p. 109). What Guba and Lincoln call *constructivist ontology* is associated with the view that reality is relative, related to the specifics of the context.

Guba and Lincoln described similar differences of epistemology, in which *positivist* and *postpositivist* worldviews hold an essentially dualist and objectivist position on knowledge, whereas *critical theory* and *constructivist positions* on epistemology hold that research findings are necessarily subjective, mediated by one’s values or coconstructed by the researcher and the participants in that research. Those alternative views on the nature of reality and how one can know that reality are reflected in the types of methods favored by researchers who hold those different worldviews. According to Guba and Lincoln, positivist and postpositivist researchers rely on methods that are essentially experimental and manipulative in an attempt to falsify hypotheses. By contrast, researchers whose worldviews are critical theoretical or constructivist use methods that are dialogical, hermeneutic, or dialectical. The aims of positivist and postpositivist researchers are those of explanation, prediction, and control, whereas the aims of

critical theory and constructivist researchers have more to do with arriving at greater understanding, both for researcher and participants, so that reconstruction and emancipation might follow. Goldhaber (2000) made essentially the same argument in his discussion of Pepper's (1942) mechanistic, organismic, and contextualist worldviews and argued that the methods and analytic techniques relevant to a theory that fits within one of these paradigms are inappropriate for use by those whose work falls within a different theoretical perspective.

It seems clear, therefore, that the worldview that one adopts has enormous implications for one's notion of reality, the type of theory that one finds appealing, the methods one uses, the way in which one analyzes and interprets the data, and so on. Not surprisingly, discussions of methodological and analytical differences cannot make much headway until differences at the paradigmatic and theoretical levels have been clarified. A good case in point is that of Piaget, whose constructivist approach was reflected in a "clinical method" that used talking with and questioning children as a way of uncovering their understanding of reality. By contrast, many of his North American critics used methods stemming from a positivist worldview to try to "disprove" Piaget's theory (Goldhaber, 2000).

From my perspective, the most helpful approach to these issues is Winegar's (1997) discussion. His position is that the term *research* should be considered more broadly as the focus on multiple interrelated levels, including metatheory (similar to paradigm or worldview), theory, methodology, method, analyses, and data. First should come the explicit focus on metatheory:

It is my position that any scientific practice will be more successful if its practitioners direct at least some of their energy toward the explicit consideration of metatheory, rather than letting such assumptions remain solely implicit as the unreflective consequences of particular practices. (1997, p. 14)

Given the previous discussion of worldviews, it is not surprising that Winegar argued that metatheoretical predilections constrain the choice of theory that one finds satisfying. A metatheoretical view that holds that reality is essentially knowable in the course of careful experimental control and manipulation would not set well with a theory that sees bidirectional influences between researcher and participants.

Perhaps the most important distinction drawn by Winegar is between methodology and method. Winegar, drawing on Danziger (1988), argued that the former term should be reserved for the explicit consideration of the types of methods that need to be used given the theory of choice:

That is, methodology operates within metatheoretical assumptions about both the phenomenon of interest and the nature of theories appropriate to the study of such a phenomenon. . . . Rules of practice of methodology include faithfulness of articulation between theory and phenomenon and elegance in translating theory-phenomenon into a particular investigative method. (1997, p. 21)

In other words, when O'Donnell laid out her objectives for this special issue, she was essentially thinking methodologically, focusing on the relations between the theory these authors were using

on the one hand and the methods they were therefore constrained to use, on the other hand. That is precisely what Winegar is calling for—an explicit link between theory, method, and analyses.

## **Commentary**

With this discussion of the links between metatheory, theory, methods, and analysis behind us, I turn to the five substantive articles in this issue. However, before proceeding I should, in keeping with Winegar's suggestion, make explicit my own metatheoretical position. I subscribe to a constructivist (Guba & Lincoln, 1994) or transactional (Altman & Rogoff, 1987) worldview, and the theories in which I have set most of my recent work are sociocultural and ecological (see, e.g., Hogan & Tudge, 1999; Tudge, 1992, 1997; Tudge, Hogan, & Etz, 1999; Tudge, Putnam, & Valsiner, 1996; Tudge & Scrimsher, under review). This theoretical framework clearly has implications for the types of methods I view as important and the types of analyses I think are most relevant to those methods. I hope that my bias in this regard still allows me to evaluate the extent to which alternative methods and analytical strategies are appropriate to other theories, based on other paradigms. Having said this, my first goal is to examine the fit between theory, method, and analysis in each of these articles. My second goal relates to the extent to which our understanding of peer collaboration has been enhanced by these articles.

## **Theory**

What are the theoretical frameworks adopted by these authors, and what are the implications for the methods that were used? Two of the articles were set within a constructivist perspective that derives from Piaget. Golbeck and Sinagra clearly referred to the theory they used to frame their research:

From a Piagetian constructivist perspective, the symmetrical nature of peer relationships presents an ideal context for promoting the development of thinking. Peer interactions offer a context for the disequilibrium of thought, the search for logical coherence, and the transformation of ideas leading to new understanding or development. (p. 22)

As they pointed out, research on collaboration set within a Piagetian perspective has focused on peer interaction rather than adult-child pairings, a view consistent with Piaget's views on the relative merit of the former for cognitive development.

Samaha and De Lisi also focused on peer collaboration (as opposed to peer tutoring) involving peers of equal status rather than partners who have different levels of competence. This view of peer collaboration "seems to be more illustrative of Piaget's (1932, 1970) view of social interaction than of Vygotsky's concept of the zone of proximal development" (p. 2). Rather than elaborating on the theoretical foundation of their work, they set their research within the context of other recent studies of peer collaboration requiring formal reasoning, spatial perspective, balance scale tasks, reading, and math. As they pointed out, the authors of much prior research have examined the active nature of the reasoning among peers, implying a discrepancy in ideas and an interest in engaging with partners to resolve those differences. Though Samaha and De Lisi did not relate those ideas directly to Piaget, that approach to collaboration accords well with his view of disequilibrium and its cognitive resolution. Essentially, their goal was to assess the

extent to which collaboration among peers would lead to improvement in reasoning. Moreover, because the literature had suggested differential effects for male and female students, Samaha and De Lisi wanted to know whether gender played a significant role.

Van Boxtel, van der Linden, and Kanselaar discussed differences between cognitive constructivist (individualistic) and sociocultural perspectives (viewed as related to social processes) on collaborative problem solving, and although they did not cite Vygotsky directly, the sources linked to the sociocultural perspective have been influenced by Vygotsky. The cognitive constructivist perspective was derived from an information-processing paradigm that focuses on “individual cognitive activities and development and stresses the need for elaboration” (p. 57). Their research, however, was set within a sociocultural framework, with a focus on the negotiation and coconstruction of meaning. That coconstruction is not simply the construction between partners, but between partners and a mediational tool—a textbook. The authors did not view the outcome as elaboration and learning, but rather as an appropriation resulting from engagement in shared activity.

Hmelo, Nagarajan, and Day also described themselves as being influenced by sociocultural theory. They wanted to describe the ways in which knowledge “is constructed by group members as they interact with each other and with cultural artifacts” (p. 37) and were interested in understanding how “learning involves mediated action in a context” (p. 37). Hmelo and her colleagues were also interested in seeing the ways in which an adult “facilitator” might be used and might help the group members.

Chinn, O’Donnell, and Jinks were not explicit about the theory that informed their research and in fact framed their research as an investigation of ways of analyzing the quality of group interactions that combine approaches from sociolinguistics and from peer learning. The authors’ focus was on the structure of peer discourse rather than on the effects of conflict and its resolution (the emphasis of the other articles in this special issue). Chinn and his colleagues were the only authors to relate their work to a broader metatheoretical level (argumentation in science), but the research itself does not seem to be guided by any specific theoretical framework.

## **Method**

All of the studies reported in these articles were well conceived, designed, and carried out. Given my earlier discussion of Pepper (1942) and Winegar (1997), however, my interest was in examining the extent to which the methods fit well with the theoretical foundation of each study, rather than commenting on the details of the methods themselves. In the case of Golbeck and Sinagra, the task itself was Piagetian, and a good deal of Piaget-inspired research has used similar methods. The participants were tested individually; they then worked again on the problem, with access to containers half-filled with liquid so that they could be actively involved with the objects rather than simply working at the abstract level. Some of the participants worked individually, whereas others worked as pairs. Finally, the participants were tested again to ascertain the extent of improvement. One problem in the design, from a Piagetian point of view, is that it is not clear whether quantitative changes in degree of horizontality constitutes a developmental change, which would be more easily shown by qualitative change. A second

problem is that although Piaget indeed argued that peer interaction is likely to be more effective in bringing about cognitive development than adult-child interaction, its effectiveness is not the result simply of equality of status. It stems from the fact that children of equal status are free to argue with one another and, potentially at least, therefore experience cognitive conflict and the cognitive restructuring associated with development. Without differences in ideas, there can be no cognitive conflict, and therefore no possibility of restructuring. Golbeck and Sinagra's design, it seems to me, would have been closer to a Piagetian design if it had encouraged some degree of cognitive conflict, for example, by consciously pairing participants whose pretest thinking on horizontality differed. Nonetheless, one might expect that some of the pairs would have differing ideas and that therefore cognitive conflict would be generated.

Samaha and De Lisi also used a three-part design, consisting of pretest, experimental phase, and posttest, and, like Golbeck and Sinagra, they conducted the entire study within a single day. Four groups were created for the experimental phase, allowing for a control group that was tested individually and three groups of 4 or 5 children (male, female, and mixed-gender groups). The groups were asked to reach consensus and then to write, individually, their justifications for the group's selection. The members of the control group, tested individually, were provided feedback. Again, the authors made no attempt to ensure that cognitive conflict would be created by grouping children who, at pretest, had shown different abilities. However, given the nature of the problems, differences in judgment and reasoning might have been expected. As was true for Golbeck and Sinagra's research, it is not clear to me that Samaha and De Lisi's outcome measures captured development in the sense that Piaget used that term.

Van Boxtel and her colleagues also used a three-part design, although that design differed from those of the first two studies in a number of ways. First, the time between the phases was far greater; the pretest and experimental phases were separated by 3 weeks, and the posttest took place 1 week after the experimental phase. This design also did not feature a control group of students tested individually—all participants were paired, virtually all as same-gender dyads. The authors were interested in the learning of concepts related to electricity, and they were mainly interested in what occurred in the collaborative session—particularly the use of a mediational device, a textbook. This design is most clearly relevant to sociocultural theory, in which one would expect cultural tools to play a significant role. Moreover, unlike in Piagetian theory, in which development should be shown by qualitative change, the quantitative changes that van Boxtel and her colleagues measured seem more appropriate, particularly given the longer intervals between sessions.

Hmelo, Nagarajan, and Day's study was different in that it focused almost exclusively on what happened during the group session. Of the two groups selected for comparison, one had scored low initially (on average), whereas the other had scored high. Given that the authors had based their research on a sociocultural foundation, the decision to examine simply the group session makes sense. The focus on the group's use of a cultural tool (a computer) also fits nicely on a sociocultural foundation, as does the use of a more competent other (the facilitator). On the other hand, the study (like that of van Boxtel et al.) did not seem designed to allow more competent and less competent students to work together, which would have been a design characteristic more representative of a sociocultural perspective.

The research reported by Chinn, O'Donnell, and Jinks was part of a larger study encompassing five sessions, although this article focused on group discussions during the second session. The design allowed for analysis of individual conclusions drawn by the students as well as the groups' discussion of conclusions supposedly written by other children of the same age. The design allowed scores on their individual conclusions to be related to the quality of their group discussion. Chinn and his colleagues did not set their research within any specifiable theory, however, which makes it difficult to say whether there was a connection between theory and method.

## **Analysis**

In keeping with the goals of this commentary, I must consider what would count as an analytic technique relevant to one theory or another. Goldhaber (2000) discussed this issue well, linking analysis of variance and regression analysis (useful for assessing the effects of independent variables) to theories stemming from a positivist or mechanistic worldview; clinical interviewing and pattern analyses to theories (such as Piaget's) that fit within an organismic worldview; and ethnography and discourse analysis to contextualist theories such as Vygotsky's.

Although Golbeck and Sinagra's article is framed in Piaget's theory, and the task and design are nicely linked to the theory, the analyses are purely positivist and do not seem well related to Piagetian theory. The authors used a traditional analysis that treats each participant as the unit of analysis, ignoring what actually occurred in any specific dyadic discussion. One problem, from a theoretical point of view, is that the analyses do not allow us to know the extent to which either cognitive conflict or its resolution was implicated in cognitive development. A more appropriate analysis would presumably have been one in which dyads that featured cognitive conflict and its resolution could have been distinguished from dyads in which there was either no conflict or conflict but no resolution. In other words, the one thing that is key to this part of Piaget's theory (the fact of cognitive conflict and its resolution in discussion) is lost from sight. Raising interesting points, gesturing, talking about the water level in relation to the external environment, and so forth, might all be important aspects of the task, but it is unclear whether these issues were raised by one partner for the benefit of another, were raised by one person in cognitive conflict with his or her own expectations, were raised by someone's partner and then simply imitated (as Bandura might have suggested), or what.

The same issues are apparent in Samaha and De Lisi's article. There is no obvious way in which the analyses they conducted are related to Piaget's theory. Their analyses also involved a repeated measures analysis of variance that treated the individual as the unit of analysis, thus effectively preventing the authors from analyzing statistically the very things that were of interest (theoretically speaking). In all groups but the all-female group, clear improvement occurred during the experimental phase (but not at the posttest) in the judgment scores, and all groups improved during both the experimental phase and the posttest for the explanation scores. Unfortunately, we do not know why there was that pattern of improvement. It might have been because more competent members of each group simply demonstrated better responses that were then imitated. It might have been that less competent members of each group came to understand better under the guidance of one or more competent members. It might have been that cognitive conflict was generated and then resolved within some groups and not in others. In other words,

the analyses could equally well support Bandura's, Vygotsky's, or Piaget's theory, although the authors believe that their findings support "versions of social constructivism based on Piagetian theory" (p. 19).

Van Boxtel and her colleagues used analyses similar to those reported by the other authors—namely, analysis of covariance and paired *t* tests, with the individual as the unit of analysis. Those analyses do not seem at all appropriate for sociocultural theory. However, the authors also go into a good deal of detail in their discussion of dyadic interaction in the course of using a textbook to assist them. In this case, the qualitative analyses are highly appropriate to the theoretical framework being used, showing the coconstructive processes occurring in one dyad with and without the assistance of the cultural tool provided for that purpose—a textbook. The richness of this approach magnifies the limitations of the quantitative approach adopted, in which no account is taken of what occurred during the collaborative phase itself.

Given Hmelo, Nagarajan, and Day's goal of examining the ways in which a joint problem space is constructed, the qualitative analyses do an excellent job of showing the construction process and how that process differed in the two groups examined (initial low prior knowledge and initial high prior knowledge). From their base in sociocultural theory, the authors appropriately demonstrated the ways in which the computer tool and the human facilitator both helped to mediate the students' discussions. Comparisons of ideas, negotiations, questions, suggestions, elaborations, and self-reflection can all be examined in the transcripts displayed and provide good indications of the coconstruction of knowledge.

Chinn, O'Donnell, and Jinks' analyses were also primarily qualitative, focusing on the structure of discussions with particular emphasis on the complexity of the structure. As is true of the article by Hmelo and her colleagues, those analyses allow for assessment of discussion at both the group and individual levels, so that it is possible not only to see what each group member adds to the discussion but also to derive measures of the complexity of the group as a whole. Chinn and colleagues then used those measures to assess "outcome" measures of the students' individual postdiscussion conclusions. However, the subtlety of the more qualitative measures of discussion complexity was followed by correlational tests in which no account seems to have been taken of group membership and in which the individual was treated as the unit of analysis.

## **Discussion**

I have been somewhat critical of these articles, but this is because I have held them to a standard typically not asked of most empirical reports—to show a close connection between theory, method, and analysis. I have done so partly because of my belief that such a connection is an important part of the research enterprise. But I was also interested in O'Donnell's objective for this special issue, namely, "to illustrate how the theoretical framework one adopts . . . has crucial influences on what is studied and how" (O'Donnell, p. 3). Most of these articles, individually, fall short of meeting the objectives, but that may be a function of the fact that O'Donnell intended that the collection of articles as a whole would illustrate the objective. In fact, I was interested to note that there was a good deal of uniformity in the ways in which the data were analyzed. All authors provided some fairly traditional (positivist) analyses that did not seem appropriate to the theories on which the research was based as well as some very interesting

qualitative analyses that focused on the processes of collaboration. I am not convinced, however, that one could examine any of the analyses and determine directly from them the theoretical foundation of the study. That was not true of the methods themselves; the two Piaget-based studies focused exclusively on peer collaboration, whereas the two studies based on sociocultural theory examined not only collaborative partners but also aspects of appropriate cultural artifacts as well as the role of the more competent adult.

Moreover, all of the articles succeeded in broadening our understanding of the effects of peer collaboration on cognitive development. I discuss those benefits from the perspective of one of my metatheoretical presuppositions—namely, that to understand development one must consider the interrelations among interpersonal, individual, and contextual factors.

### *The Interpersonal Level*

At the interpersonal level, most of the articles provide wonderful evidence for why it is so helpful to examine the processes of interaction that take place during peer collaboration. It must be clear to all researchers in this area that studies that only discuss the results of collaboration tell us nothing about the mechanisms involved (collaborative processes) that might explain those results. By contrast, these authors focus primarily on the mechanisms, particularly at the level of discourse.

For example, Hmelo and her colleagues examined the ways in which joint problem spaces were created in the course of discussion. Chinn and his colleagues illustrated the benefits of examining the complexity of the argumentation structure itself. Their analyses clearly show how greater collaborative argument complexity is linked to posttest gains and that elaboration might be the key. Providing relatively simple points in support of or critical of a partner's argument was not as helpful as going beyond what a partner had said, adding elements from one's own thinking. That linked nicely with Hmelo et al.'s discussion of evaluative and interpretive statements. Van Boxtel and her colleagues also showed the ways in which simple reading from the text was far less helpful than elaborating on the information that was gained.

By contrast, it is noteworthy that Golbeck and Sinagra found (or at least reported) very little evidence of cognitive conflict, let alone complex argumentation or elaboration of ideas, and perhaps that helps to explain why there was no benefit gained from peer collaboration. Samaha and De Lisi, on the other hand, found that the students who had provided more sophisticated explanations during the collaborative sessions provided better explanations (although not judgments) at the posttest.

In these articles, argument or sociocognitive conflict was less useful than the process of negotiation, of collaborating in such a way that joint agreement or intersubjectivity was attained. Goals had to be mutually agreed on and worked toward; however, Hmelo and her colleagues provided some excellent examples of students coconstructing their understanding of the problem in the course of discussion. The dyads discussed by van Boxtel and her colleagues also indicated that "engagement in each other's ideas and reasoning" and building on the contributions provided by the partner was linked to cognitive gain (p. 71). Conflict by itself was of no value.

### *The Individual Level*

However, to make sense of collaborative problem solving (the interpersonal level), one must take into account what each individual brings to bear on the situation. In some cases, it simply was not clear why some participants were more goal oriented than others, some more willing to collaborate and negotiate than others. However, Hmelo et al. noted clear differences in the ways in which members of the high-prior-knowledge group differed from those in the low-prior-knowledge group. Specifically, the former referred more often to prior and conceptual knowledge when solving the problems, were more goal oriented, asked more questions, and made more evaluative statements. They were also far more likely than those with low prior knowledge to try explicitly to work on achieving consensus—all things that were related to task success. Clearly, their higher knowledge base meant that they were able to approach the problems and attempt to solve them in a way different from those who had less of a base to start from. Their findings relate nicely to those of Chinn and his colleagues, who found that individuals often benefited from the collaborative process, even if the reasoning used was constructed individually, without help from others in the group. Unfortunately, because Hmelo et al.'s groups were homogenous with respect to initial knowledge, it was not possible to determine the ways in which the students with more knowledge might have helped those with less knowledge.

The studies reported by Golbeck and Sinagra and Samaha and De Lisi draw attention to a different personal characteristic—gender. Golbeck and Sinagra found, for example, that the women-only dyads did not benefit from collaboration (at least in terms of the task they were working on) as much as did the men-only dyads; women paired with men also did poorly. Women, in fact, performed better from pretest to posttest when working alone than when collaborating. Samaha and De Lisi, similarly, found that girls-only groups performed worse, at least in terms of judgments, than did boys-only groups during both the collaborative phase and at posttest. Both sets of authors noted that female students might have been more interested in establishing affiliative relations with their partners than in trying to solve the problem assigned.

### *The Contextual Level*

It is thus clear that the personal characteristics that dyadic or group members bring with them to the collaborative sessions themselves are as important as what goes on between dyad or group members in the collaboration itself. The final factor is the contextual—by which I mean not only the proximal context of the setting itself but also the broader sociocultural context. For example, if it is the case that the different performance of the female participants in the studies just mentioned was due to their affiliative tendencies, it is only by reference to the sociocultural milieu that one can understand why that might be true. Change the broader cultural context, and there is no reason to expect the same type of differences (Tudge, 1989). It is hard to ignore the impact of context, as Golbeck and Sinagra noted when discussing male-female interactions that resulted in the female participant appearing uncomfortable.

Samaha and De Lisi dealt with context in a different way. Having commented on the fact that “virtually all [research on peer collaboration] has been conducted with nondisadvantaged school children” (p. 7), they used a predominantly Hispanic and disadvantaged set of participants. They

are to be commended for examining the effects of peer collaboration on groups that have been little studied, particularly as disadvantaged students may be less likely to be involved in cooperative groups in school than are those with more financial and social advantages. The results indicated that for the most part, peer collaboration was beneficial, but it would have been useful if the authors had indicated whether the degree of benefit from collaboration was similar to what would have been found among students from more advantaged groups. Research using the same method but a different population is needed if we are to know whether the effects of collaboration are similar for children from different cultural milieus.

Several of the authors incorporated the sociocultural world explicitly in their research by the tools that they included to assist the collaborators. Hmelo and her colleagues, for example, noted that “computer tools provide opportunities to study the role that social factors and artifacts play in learning” (p. 37). Hmelo et al. showed the ways in which the computer helped to structure and guide the students’ thinking and also briefly discussed the role of the human guide (facilitator), representing the more competent social world, who also explained things if necessary. I would have liked to see some discussion of the fact that this task, a simulation of a clinical testing of an anticancer drug, had real-world significance, particularly to the medical students who were involved. The task contrasts with those more typically used in collaborative problem solving, in which there is a much less obvious reason to be motivated to solve the problem. Such a discussion would have allowed a better sense of the ways in which individual factors (prior knowledge and motivation) in conjunction with aspects of the sociocultural world (in terms both of tools and meaning) helped make sense of what occurred interpersonally.

The work of van Boxtel and her colleagues is set within a sociocultural framework, and they pointed out that this framework “provides us with an understanding of concept learning as social and situated processes in which knowledge is coconstructed and mediated by symbols and artifacts” (p. 58). In this case, however, what was interesting was the fact that the cultural tool that was used in the study (the textbook, potential source of cultural knowledge) was actually an impediment to learning, at least to many of the students. Although van Boxtel et al. did not write about it in these terms, their study was a nice illustration of the fact that from a Vygotskian perspective, internalization or appropriation is far more than a simple copying or imitation of some aspect of the world. Instead, it involves a process of making the information, skill, or concept one’s own, thereby transforming it in the process (depending on the individual’s past experience, knowledge, etc.). Although the group of dyads that had access to a textbook was not sufficiently distinguished in this way, I am confident that the dyads that did better were those who, like Aniek and Bregje, made the information their own in the course of elaborating on it. Students like Monique and Yvette, by contrast, who simply used the text as an “authority” and copied sentences without thinking or discussing them would not be expected to have made the information their own. In other words, they had not appropriated the information in the way in which Aniek and Bregje had.

Taken as a set of articles, therefore, the research discussed here helps make clear that interpersonal, individual, and contextual factors must all be considered if we are to understand the relations between collaborative problem solving and cognitive development. This is a very worthwhile goal to have accomplished, and there should be no doubt that our understanding of these issues has been markedly increased.

Is this sufficient? I think that O'Donnell's overview raises an issue that may be more important-to show the ways in which our theories influence what and how we study. Our field needs more theoretically driven research in which care is taken to match methods and analyses to the theoretical demands. In other words, I think that we need to spend more time undertaking what Winegar (1997) called the "methodological" work of the research process. To do otherwise is to fall prey to an empiricist stance in which variations in findings, no matter how interesting, cannot be related to each other in a coherent whole. What makes findings interesting is not simply whether they support or call into question results reported by other researchers. A whole variety of reasons could be held responsible-from variations of task to differences of age of participants. What surely matters is that patterns of similarities or differences in findings can be related to each other in some coherent manner. Coherence, as Winegar argued, is the realm of theory.

If that is the case, why do we see so few published articles in which clear connections are drawn between theory, method, and analyses? Authors rarely make explicit their metatheoretical presuppositions, and the theoretical foundation of their studies often receives little more attention than a few citations of the theorist or theorists who have presumably been influential in the authors' thinking. Is this because theory is considered unimportant? I think not. I think that there are two main reasons. First, graduate students in education, psychology, and child or human development are not encouraged to give more than cursory attention to issues of theory and the application of theory in methods and analysis. By contrast, far more attention is given to classes in research methods and statistical analyses. Second, the journals in which we write tend to be bimodal; they either allow a focus on matters of theory, with little or no space allowed for analyses of data (see, e.g., *Human Development* or *New Ideas in Psychology*), or encourage detailed discussion of methods and analyses but provide little encouragement for authors to go into theoretical depth. Moreover, the latter journals, focusing primarily on empirical work, are far more numerous, and the vast majority of the most prestigious journals come from their ranks.

The authors of the five articles I have been discussing are to be commended for the fine work they have done increasing our understanding of the relations between collaboration and cognitive development. Moreover, they illustrate nicely the connection between theory and method; different theoretical perspectives do indeed lead scholars to approach the issue of collaboration differently. Perhaps it is time, however, to take more account of the fact that the statistical techniques that we are trained to use have implicit theoretical and methodological connections that may not fit with the theories or methods that we want to use.

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