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Preface

Conceptions and conceptual change have become a dominant paradigm in science education ever since the seminal study on the topic was published in a science education journal. The framework fundamentally is concerned with supposed mental frameworks human beings are said to develop for understanding, among others, scientific phenomena. Since then, however, numerous other theories and disciplines have emerged that not only challenge the conceptual change paradigm language but also the conception of language as a neutral means for reading out and accessing contents of (solipsistic) minds. Discourse analysis, discursive psychology, cultural-historical activity theory, theories of language and multiliteracies, social semiotics, and so forth all challenge the usefulness of thinking about (a) human activities in terms of the public confrontation and interaction of individual human conceptions, and (b) learning as conceptual change, a change that requires the reconfiguration of mental structures.

The purpose of the 2008 Springer Forum, held on March 29, 2008 in the Graduate Center of the City University of New York, was to establish a broader context for rethinking conceptions and conceptual change, a context that takes into account more recent developments in thinking human activities, such as cultural studies, cultural sociology, discursive (social) psychology, and so forth. The participants were asked to articulate in their papers new ways of thinking about conceptions and conceptual change—that is, new theoretical perspectives—or to provide case studies of conceptions and conceptual change that question the very idea of mental structures that can be identified independent of human activities in which they are mobilized for the purposes at hand. We subsequently extended our focus by thinking about how the dominant psychological paradigm could be expanded to include sociological theories of knowing and learning in science education specifically and in science, technology, engineering, and mathematics education generally.

Following the very successful Forum, a success to a large extent brought about by its highly interactive nature, we planned to reproduce the interactive nature in this book that was planned as one of the Forum’s outcomes. Furthermore, together with Ken Tobin I conceptualized this book for graduate level courses and as a scholarly volume for a wide range of academics and policy makers in science education interested in overcoming the gulf between sociological and psychological approaches in and to science education. I had set as my specific goal to make sure that language and style are appropriate to address the diversity of the intended audiences.

All contributors to this volume have endeavored to produce texts that contribute to the effort of overcoming the still existing divide between sociological and psychological approaches to science education research and practice. From very
different positions—gender, culture, race—they provide valuable insights to re/uniting approaches to doing theory and method in our field. As an ensemble, the contributions constitute a rich array of thought from which new forms of science education can emerge.

A book such as the one presented here is impossible without the assistance of others. Most importantly I am grateful to the continued support Springer Verlag generally and Harmen van Paradijs particularly have provided to their endeavor of making cultural studies of science education an acknowledged and legitimized way of working in our field. This book also is the first of a new Springer series entitled, as the journal associated with this endeavor, Cultural Studies of Science Education. Ken Tobin and I have been the founding editors of this series, as we have been the founding editors of the journal. I am also grateful to the contributors, who willingly subscribed to the goals that we had framed for the Forum and for this book. Finally, I am grateful to all those individuals and organizations who have contributed in making the original Springer Forum not only possible but also a success. Above all I want to thank Ken Tobin for his continued support and encouragement, which facilitated and accompanied my career for the past 20 years.

Victoria, BC  
September 2009
Chapter 1

ReUniting Sociological and Psychological Perspectives in/for Science Education An Introduction

Wolff-Michael Roth*

[C]onsciousness, provided that we do not lose sight of its content, is not just a psychological phenomenon but also, and above all, an ideological phenomenon, a product of social intercourse. (Vološinov 1976, pp. 114–115)

How we think about phenomena of interest is a function of the cultural and historical position of the field in which the phenomenon of interest appears—in science education phenomena of interest include learning concepts, teaching concepts, teacher and student identities, and motivation. Take the following excerpt from an interview between a graduate student of science education, interested in conceptions and discourses about natural phenomena, and another student at her university. The two have come together, as part of a largely tacit social contract, to produce an interview that has as its content the way in which the interviewee (Mary) thinks about diurnal and seasonal changes. The interviewer utters what Mary clearly hears as a question, but the specific nature, as indicated in her own turn, is not clear to her (turn 02). The question concerns the specific position of the sun in the sky at the moment of the interview. Mary offers a possible hearing of the question, which we might gloss as, “So you are asking me ‘Why is the sun in the sky?’”; the interviewer affirms this hearing (turn 03). After a brief pause, Mary offers an answer, where the position in the sky is explained by the facts that it is daytime and that the sun is moving.

01 I: um (0.48) and do you think why the sun is over there? its in the sky?
    (0.90)
02 M: why? the sun is in the sky?
    (1.12)
03 M: why? the sun is in the sky?
    (0.19)

* W.-M. Roth, University of Victoria

As a result of about 30 years of research in the field, science educators have come to see in such answers evidence for the presence of a misconception in Mary’s mind. She thinks what we can gloss as “the movement of the sun across the sky,” which is a way of talking about this phenomenon typical to Western culture prior to Galileo Galilei, who, in an acrimonious debate with the (Catholic) Church, attempted to convince others that it was the rotation of the Earth that led to the impression of the sun’s movement across the sky. In the science education literature, largely conducted from psychological perspectives, such interview excerpts are used to postulate individually held mental frameworks and (mis-, alternative, naïve) conceptions. Because Mary uttered the words in this manner, it is easy to be convinced that she thinks what she said and, if we accept that a computer metaphor for the mind, that there are structures in her mind that make her say what she says. But is this the only way of thinking about the episode? Is turn 05 evidence for a phenomenon that has to be analyzed using the individual (mind) as the unit of analysis?

It turns out that there are many reasons why we might want to think very differently about this situation than from a traditional individual psychological perspective—though such ways of thinking have not yet taken hold widely in our field. Thus, Mary responds to questions about phenomena that she has not or little thought about before, as she says to the interviewer a little later in the conversation. If she has not talked and thought about such phenomena before, she cannot have a mental framework or conception, which, according to conceptual change theory, is the outcome of an intentional construction. How then can Mary talk about something that she has not yet thought about and has no framework for? One answer lies in the nature of language, which provides us with resources to speak about phenomena even if we have never talked or thought about them before. A second reason for thinking about this excerpt in a different way comes from the fact that Mary does not just talk to herself. She addresses the interviewer, with whom she has entered into a social contract by promising to participate in and complete an interview. With this social contract comes that she responds to make the interview a success, that is, that she speaks in a manner and presenting content such that the interviewer understands what she is talking about. What she says therefore is explicitly directed to the interviewer for the purpose of helping this other person understand what she, Mary, has to say about the position of the sun in the sky. That is, Mary inherently presupposes that the interviewer hears and understands what she has to say. This means that her way of talking inherently is presupposed to be understood and therefore a realization of a cultural possibility of talking
rather than an idiosyncratic, solipsistic, highly singular way of talking. In fact, the very phenomenon of language is premised on the presupposition that another person can hear and understand, understand and hear. Even when the very first word was uttered in the history of humanity, the speaker presupposed to be heard and understood. Speaking, in fact, presupposes hearing, and hearing presupposes understanding (Heidegger 1977). We talk only because we can presuppose the intelligibility of what we say, even if at times it turns out that this presupposition was wrong for one or the other reason. That is, even in its brevity, this analysis already points to the fact that the proper unit of analysis might be language and social interaction, the domain of sociology and social psychology, rather than that of individual (cognitive, developmental) psychology.

There is then a tension between the fact that language offers us ways of talking about relevant phenomena in inherently shared and intelligible ways, on the one hand, and the fact that such talk is always delivered in concrete and singular manner by an individual. How are we to think this tension? How are we to think knowing, learning, teaching, or instruction in and from this tension? How are we to think if we decide that it should not be from one or the other side of the apparent abyss between the psychological and the sociological? How are we to think about science education phenomena without reducing the differences to the hegemony of the same that comes with a master discourse? Are there ways of thinking | speaking about learning where the difference between the sociological and psychological becomes undecidable even in the undeniable presence of both? We may then have to look not only for a dialectical sublation (Aufhebung) of the opposites in a synthesis that arrives with a new unity but also for new ways of thinking. What would such new ways of thinking be? How do you bootstrap out of the old ways of thinking in dichotomous manner typical of classical logic—A, not-A, and tertium non datur [a third is not given]—into new ways of thinking that accepts and is based on difference as such? These new ways of thinking, if we admit hints from philosophies of difference (e.g., Derrida 2003a, b), my take us into domains where the process of thinking no longer fits the computer metaphor. This metaphor makes decisions computable, that is, it suggests that you can make fully informed decisions about choice alternatives. The notion of undecidability, which allows us to understand diverse human experiences—including forgiveness, democracy, law, giving and gifts, time and temporality, the relationship of thought and language, and memory—takes us beyond computability and into the domain of chaos and catastrophe theory, where future events, even if they are only seconds away, can no longer be predicted with accuracy.

Introduction

Psychological approaches to knowing and learning have dominated science education since its beginning, as evidenced in the importance that behaviorism, cognitivism, Piaget’s developmentalism, or constructivism have had on theorizing
salient phenomena. These approaches have almost entirely focused on establishing the mental as an individual phenomenon to the exclusion of sociocultural, cultural-historical, sociological, or anthropological moments of human life generally and learning specifically. More recently, a number of other theories and disciplines have emerged that challenge language as a neutral means for reading out and accessing contents of (solipsistic) minds. Discourse analysis, discursive psychology, cultural-historical activity theory, theories of language and multiliteracies, or social semiotics all challenge the usefulness of thinking about (a) human activities in terms of the public confrontation and interaction of individual human conceptions, and (b) learning as a solely psychological phenomenon (e.g., as conceptual change, a change that requires the reconfiguration of mental structures).

Two recent papers in particular present alternative perspectives on conceptions and conceptual change, one grounded in cultural studies (Roth et al. 2008) and the other one in discursive psychology (Roth 2008b). The common tenor of these papers is this: if there is anything like the psychological that exists independently of the cultural context, then it always constitutes a social achievement in which society and culture generally and researchers specifically have an active part. The psychological therefore cannot be legitimately attributed to individual minds, especially because the very linguistic resources for producing them in talk are cultural rather than individual possibilities. But of course, there cannot be a culture and the associated collective consciousness without the individuals and their consciousness that constitutes it. The two aspects, the sociological and the psychological, go hand in hand when it comes to understanding social life generally and learning in and of the sciences specifically.

Thinking the two perspectives differently, sociology and psychology, is the tenor of the message in the Vološinov quote that opens this introductory chapter. With this book, we aim at beginning such a rethinking of psychological and sociological approaches in and for science education. That is, prior to and outside of science education there have been approaches to studying knowing, learning, mind, consciousness, and so on that embodies the dual approach, the sociological and the psychological. In fact, the very etymology of the word consciousness, from Latin con-, with, and sciēre, to know, points us to the social at the very center of what it means to think, reflect, and be aware of. The purpose of this edited collection is to establish a broader context for rethinking science education generally and knowing and learning in science specifically. This context takes into account more recent developments in thinking human activities, such as cultural studies, cultural sociology, or discursive (social) psychology. The chapters and discussion forums articulate new ways of thinking about knowing and learning, that is, about new theoretical perspectives—or provide detailed case studies of important issues related to science education that question the very idea of the psychological that can be identified independent of human activities in which they are mobilized for the purposes at hand. The ultimate purpose of each chapter and the edited collection as a whole is to prepare the ground upon which sociological and psychological perspectives in science education can be reunited to provide more encompassing theoretical frameworks that allow us to capture the complexity of science learning.
as it occurs in and out-of schools. As a whole, the book moves theorizing into new and uncharted terrain. But we do not and cannot claim to give a final answer: such an answer is forever on the remove, forever withdrawing, and therefore forever to come. This is the underlying idea of dialogism, where ideas are in continuous exchange, continuously working and reworking each other, therefore undergoing continuous change for the purpose of self-improvement. If there were the hope of achieving a single answer, this would be the death of dialogue and the arrival of a mono-logical—both in the sense of one logic and one discourse—way of understanding human nature. To anticipate such unrealizable hopes, Ken Tobin invites us in chapter 2 to think beyond the hegemony of any single narrative about knowing and learning in science education.

Learning Theories Across the History of Science Education

A recently published history of the National Association for Research in Science Education shows how this science education organization emerged in the course of the twentieth century (Joslin et al. 2008). The paper reports, among others, on the struggles that the journal of the organization, the Journal for Research in Science Teaching, had problems with the quality of the articles generally and the theories and methods specifically. In his contribution to the collaboratively written history, Roger O. Anderson notes that at the time, science education researchers began to draw on theories and methods in cognate fields including “cognitive developmental psychology, learning theories, scientific epistemology, and theories of assessment and evaluation” (p. 179). It was at this stage, especially because the unit of analysis and teaching in science education was viewed in terms of the individual, that knowing and learning came to be framed in terms of the dominant psychological theory of the day. These included, in historical order, behaviorism, developmental psychology and Piagetian stage theory, David Ausubel’s reception learning paradigm, information processing, and finally radical constructivism followed by social constructivism. Associated with the psychological theories were the methods of investigation also adopted from psychological research: experimental and quasi-experimental studies focusing on the relationship between pedagogy (e.g., teaching method, curriculum materials), on the one hand, and achievement levels, on the other hand.

The latter part of the 1980s witnessed the emergence of qualitative observational and ethnographic research methods perfectly suited to the emergence of (radical) constructivism on the scene. The ethnographic research designs gained in importance during the 1990s within a new generation of researchers in science education associated with an increasing role of researchers from diverse backgrounds heretofore ill represented in the science education community: the research community now included increasing numbers of women, visible minorities, individuals of different culture, or race. However, despite the increasing differentiation in the nature of the participants, the main paradigm concerning the locus of knowledge
remained to be the individual mind. Moreover, the dominant paradigm emphasizes forms of thinking rooted in metaphysics and the Greco-Roman forms of logic and rationality. It is not surprising, therefore, that postmodern scholars would find delight in the associations created by term *phal-logocentrism* (e.g., Derrida 1992), which points to the historically dominant influences on philosophy: male (phallus), language (*logos*), and logic (Gr. *logos*). This influence continues in the way science is practiced today, including psychology and sociology. But despite the changes in Continental philosophy in particular, research continued with the traditional in-the-head orientation that has dominated thinking about epistemology. Thus, we witnessed research that seemed to focus on cultural variation but that read like it had a psychologically oriented conceptual change orientation. That is, the individual (differential) psychological approach dominated even when researchers outwardly displayed a discourse about culture as the relevant unit for understanding knowing and learning.

The orientation to and focus on learning from a psychological perspective was also observable in the, at the time most radical theoretical shift toward communities of practice and situated cognition. Typical for this orientation is the conceptualization of learning in communities of practice, which is said to occur as the individual *is moving from the periphery to the core of the community*. That is, the theory concerning communities of practice and the trajectory of learning with *legitimate peripheral participation* still focused, at least in the research realized, on the individual as the unit of analysis (Roth and Lee 2006). The community of practice was taken as a more or less stable box with a periphery, where the cultural practices were less authentic than in its core, where the leading practitioners and the most characteristic and community-defining practices were to be found. Learning was still thought to occur in the individual thought to become competent and in so doing moving from the periphery to the core.

A very different way of thinking about knowing and learning was offered in the latter part of the 1990s in a series of studies concerning the *discourse* found among high school students concerning knowing, learning, beliefs, epistemology, and so on (e.g., Roth and Lucas 1997). The approach was grounded in a relatively new sub-discipline of psychology that came to be known under the name of *discursive psychology*. The fundamental idea of this approach is to take discourse as the unit of analysis rather than the individual. The individual then is thought to realize possibilities that exist at the collective level. The need for such a shift in thinking comes from the fact that in talking, an individual student or teacher participating in a research project addresses the researcher or a research assistant. But in addressing this other person, the form and content of the discourse is automatically adjusted to the social situation and made what is—without reflection necessary—to the listener. Any stretch of discourse, therefore, is characteristic not of individual participants but of the coparticipants in the communicative effort and the type of social situations in which they take part. Thus, from this perspective it is to be expected that a conversation between a high school teacher and his student changes when it takes place in the context of a lesson that is part of their physics course or when it takes place in the context of an interview that the teacher
conducts as part of his research on knowing, learning, or epistemology. Just as the introductory quote states, consciousness is a function of ideology and social interaction—and all three, consciousness, ideology, and social interaction use language as their ground, material, and tool for their realization (e.g., Roth in press). If a high school student talks about a hand and arm transferring a force to a baseball that a pitcher throws (a “misconception”), then, from this perspective, we ought to understand it as a shared phenomenon realized by this student but understandable by many others speaking the same language. This student merely realizes a widely recognized possibility of English to explain the flight path of a baseball.

In this shift that I describe in the course of last two paragraphs, from psychology sociology (social psychology), the unit of analysis changed from the individual to culture or a cultural dimension (discourse). We can easily understand why for educators such a shift is difficult to understand and make. Whereas researchers can use a cultural lens for understanding, teachers still are confronted with the task to educate and test individual students. Even when students are allowed to learn in collaborative settings, the institutional requirement of evaluating learning at the individual level orients the endeavor of science educators to the individual. This is the case even though ways of talking are shared, so that the talk used to realize “(mis-, alternative, naïve) conceptions” constitutes a cultural possibility. This cultural possibility is not eradicated in the teaching of an individual child or student, allowing us to understand why “misconceptions” are so resistant to the endeavors of those science educators interested in eradicating them. The discourse approach provides us with an answer to a question that conceptual change researchers have been unable to provide.

The foregoing suggests that to understand what students say and write we need to study culture, even though we know that it is the individual who, especially when reflexively pointing to him/herself using an indexical “I” or “me.” There are other reasons why science educators may have to shift perspective to include cultural and sociological perspectives not only in their theoretical frames but also in their teaching. In an increasingly complex world, an increasing division of labor defines any task. Whereas it was still possible for many individuals to repair the engine of their cars into the 1980, an increasing computerization and the associated change in car engines makes it almost impossible for the person without specialized competencies, tools, and instruments to conduct a repair. Similarly, environmental problems, for example, are so complex that it takes not only scientists and engineers but also individuals with many different competencies to solve. Thus, the question whether to put a water main to an outlying area of a municipality cannot be solved by drawing on hydro-geologists and town engineers (Roth 2008a). There are many others involved, the fire department, the local people with their local ecological knowledge, politicians at the municipal, provincial, and federal levels, the regional health authorities, lawyers, and so on. The different members do not need to know what others know, even in its most basic and rudimentary form. But the participants in the decision-making process need to be able to evolve a form of discourse that allows them to settle the issue. It has
therefore been suggested that scientific literacy ought to be rethought in terms of a collective praxis, something people do together in real time of the relevant here and now (Roth 2007). The question then becomes this: If we want to teach for competent participation in collective practices, what do we need to change in the classroom? The present book has been conceived in part as an opportunity for providing the ground for thinking about how to change not only our research foci but also for rethinking how we might think about curriculum design so that it does not privilege the psychological to the detriment of the sociological. It is only when sociological thinking accompanies psychological thinking that we can re/structure science education to address the needs of humanity as a whole rather than only the needs of (a few) individuals. The various real and imagined crises facing humanity require more than we have done in the past; and science educators need to do more or rather differently than producing the forms of scientific (il-) literacy that they have produced in the past.

Social-Psychological Approaches

Outside science education specifically and education generally discourses that focus on the collective nature of knowing and consciousness have existed for a long time. Thus, in contrast to Immanuel Kant, whose theories underpin the work of Jean Piaget and (radical) constructivism, G.W.F. Hegel (1806/1977) suggested already at the beginning of the nineteenth century that consciousness, and therefore knowing and learning, is a collective dimension. The term consciousness literally means knowing together, refers us to forms of knowing that are shared between people and within a culture. Anything we can think is shared and always is articulated in terms of a shared language. The term self-consciousness therefore denotes a consciousness of the self that always is in terms of the other, in terms of a shared consciousness mediated in and by language. Thus, any “action has double significance not only because it is directed against itself as well as against the other, but also because it is indisvisibly the action of one as well as of the other” (p. 112). Any two interlocutors “recognize themselves as mutually recognize one another” (p. 112). The work of Hegel is important because it influenced, in part via Karl Marx, a number of scholars whose work has begun to influence science education only in more recent years.¹ These scholars include the members of the Bakhtin circle (Mikhail Bakhtin, Valentin Vološinov, and Pavel Medvedev) and a lineage of social psychologists ranging from Lev Vygotsky via Alexei Leont’ev and Alexander Luria to the present day.

One can observe influences of the Bakhtin group on science education on the part of some scholars who attempt to rethink science education in terms of the theoretical notion of discourse. The term—etymologically deriving from the Latin

¹ For a general review of this literature see Roth and Lee 2007; for a review of this literature pertaining to science education alone see Roth et al. 2009.
ReUniting Sociological and Psychological Perspectives

discurrere, to run back and forth, to converse (itself deriving from Latin conversare, speaking with)—at its root, requires multiple individuals involved in a give and take, involved in a passing back and forth of language. The importance of the Bakhtin group derives from the fact that it conceives of language generally and the individual word specifically as something inherently yoking speaker and listener (e.g., Bakhtine [Volochinov] 1977). The individual word cannot ever be attributed to the speaker but always already is characteristic of two. Moreover, the individual word never can be understood on its own but always as part of a language (and therefore culture) as a whole. It is part of a web of signifiers that constitutes differences (Derrida 1967). This understanding of language is captured in and denoted by the adjective ideological, which itself is set in contrast to the material aspects of everyday human life. As the introductory quote states, every psychological phenomenon is an ideological phenomenon, and therefore, inherently, a sociological phenomenon. But realized in and through individual tongues—etymologically, language derives from Latin, lingua, via the French, langue, tongue—and ears, language is a material phenomenon through and through. This inseparability between the material and the ideological is evident especially in Bakhtin’s (1984) analysis of Rabelais’ Gargantua and Pantagruel. Therefore, relative to science learning, Bakhtin’s work implies that the body and mind can be separated as little as psychology and sociology, a point made relative to science education in detailed analyses of concept mapping sessions (Roth et al. 2009). Rigorous applications and developments of the work of the Bakhtin group remain to be conducted within our discipline, but at this instant they promise a lot to the endeavor of including both sociological and psychological perspectives.

Despite living at about the same time, there is little evidence of interaction between the thinking and theorization that occurred in the Bakhtin circle and the budding social psychology that Vygotsky and his students and co-workers evolved.2 Most fundamentally, attempting to articulate a rigorous Marxist psychology, Vygotsky (1978) held that any higher psychological function has existed in and as social relation. That is, anything that science educators historically have attributed to the individual (mind) is, following Vygotsky, observable in social interactions, which have been the object of sociological studies. Whereas some science educators acknowledge the provenance of higher psychological functions from social relations, they tend to seek the functions in the individual rather than in social interaction. Moreover, it became evident especially in the work of his students and followers that the social relations, psychological functions, and relevant forms of consciousness are a function of the collective activity. Thus, it can be expected that thinking about the physics of flying in the context of a school lesson versus thinking about the physics of flying in the context of a real competition flying model planes will be different; this expectation bears out and has been tested empirically (Leontjew 1982). The phenomenological sociologist Alfred Schutz

2. Vološinov (1976) references an article by Vygotsky. But I am not aware of any other points of contact between the Bakhtin group and the psychologist. But there are several texts in which Vygotsky articulates his Marxist position; and it is in this that he has a lot in common with Vološinov, who apparently introduced Marxist thinking and theory into the Bakhtin group.
(e.g., 1996) arrived at the same expectation based on somewhat different intellectual grounds (European phenomenology). Thus, the forms of consciousness are a function of the project at hand; our apperceptions are a function of the project, which realizes our situation-specific, contingent interests in a (personal) world “organized around the self who lives and acts in it” (p. 13). The world of everyday life, in terms of which we understand our experiences, always already “existed before we were born; it is given to our experiences and interpretations” (p. 26). That is, consistent with Vygotsky, Schutz articulates individual consciousness as concrete realization of collective consciousness.

There is more to Vygotsky than his focus on the development of (scientific) thinking. One part of his theory that has merely begun to be of interest to science educators is the integral role of affect to cognition. Thus, whereas in most psychological theories affect is theorized as a factor external to cognition that affects and generally diminishes cognitive powers, Vygotsky (1986) suggested that affect and cognition are irreducible moments of the same phenomenon. If it were not like this, we would not be able to understand how emotion and cognition could mutually mediate and influence each other such as when success in a cognitive task increases emotional valence and negative valence (e.g., potential punishment) decreases cognitive performance. Most importantly, together with the theory of the origin of higher psychological functions, we can understand that an understanding of emotions and emotion talk is itself a cultural sociological phenomenon. Emotion as phenomenon and construct that is part of the dialectic of the individual and collective is central to a relatively new sociological domain: the sociology of emotions (e.g., Collins 2004). Focusing on the interaction between individual and collective affect, mediated by rhythmic phenomena made available in voice, tapping with pens, rocking body parts, or emphatic gestures has contributed to science educators understanding of learning in complex culturally diverse settings (Roth and Tobin in press).

A better understanding of affect will also lead science educators to a better understanding of the relationship between thought and language, a topic particularly salient in chapter 9, “Thinking and Speaking: A Dynamic Approach” (Roth, this volume). Why might thinking and speaking continually push on and develop? Why should anyone endeavor to change his or her thinking, that is, why should anyone endeavor to learn anything? This is precisely the problem Vygotsky (1986) framed in saying that the separation of the study of thought and affect, a weakness characteristic of “traditional psychology … makes the thought process appear as an autonomous flow of ‘thoughts thinking themselves,’ segregated from the fullness of life, from the personal needs and interests, the inclinations and impulses of the thinker” (p. 10). Affect and emotions have their source in and are mediated by, as Vygotsky’s successor Alexei N. Leontjew (1982) points out, the collective, entirely practical endeavors and projects. This is precisely the

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3. In dialectical theories, moments are identifiable structures within some phenomenon that cannot exist on their own (Roth and Lee 2007). They are a function of other moments and of the whole. Moments therefore are different from elements, which are parts that can be used as they are to build the whole. Moments do not exist apart from the whole.
perspective that Schutz also takes when he emphasizes that the social actor will consult emotions and affect as much as rational deliberation in making a decision in and about a situation; moreover, the situation itself, by means of the actors practical interests in it, is the root of emotions and affects. Again, an integrated approach to which both sociological and psychological ways of theorizing contribute or find their place will offer science educators a much more extensive and holistic tool for researching and teaching science than any individual approach taken in and by itself.

**Structure of This Book**

The body of this edited collection consists of the papers and plenary talks presented during the Second Springer Forum. Each paper or paper set was followed by a discussion of the central features of the text. To capture the dynamic aspects of the original discussions, each chapter or chapter set in this book is followed by a forum in which scholars discuss, based on the videotapes of presentations and discussions, the salient issues raised. As a whole, this book captures the dialogic and interactive nature of the Forum from which the papers and discussions derive. It therefore also represents the ways in which scholars do much of their theorizing work.

As I introduce each of the three parts of the book with a brief text and summaries of the chapters, I outline here but the structure of the book. It begins with an overarching chapter entitled “Tuning in to Others’ Voices: Beyond the Hegemony of Mono-logical Narratives” in which Ken Tobin exhorts us to get out of the traditional practices that forced us into intellectual silos that then confronted each other in sometimes acrimonious debates and to begin new practices modeled on dialogue and dialogism. The chapter sets the stage for three parts within and across which authors engage in wrestling with questions surrounding the problem of sociological and psychological approaches. Part A of the book centers on the three plenary talks presented at the Forum by Greg Kelly, Anna Stetsenko, and myself. Yew Jin Lee and Donna DeGennaro, Regina Smardon and Wesley Pitts, and Eduardo Mortimer and Pei-Ling Hsu are, respectively, the discussants of the three main papers. In Part B, I collected together chapters in which the authors contribute articulations of positions and perspectives on the theme of this book. The authors include Kathryn Scantlebury/Sonya Martin, Michiel van Eijck, Jean-François Maheux/Wolff-Michael Roth/Jennifer Thom, and Chris Emdin. Giuliano Reis provides a commentary on this chapter set as a whole. In Part C, I collected chapter contributions that focus on science agency and structure across the life span. Katherine Richardson Bruna, Maria Varelas/Justine Kane/Christine Pappas, Edna Tan/Angela Calabrese Baron/Miyoun Lim, and Maria Rivera Maulucci contribute chapters to this section. Jennifer Adams/Christina Siry/Koshi Dhingra/Gilian Bayne discuss, literally, the four contributions to this third part of the book. I contribute an epilogue to conclude the book.
References


Chapter 2

Tuning in to Others’ Voices: Beyond the Hegemony of Mono-logical Narratives

Kenneth Tobin*

I will learn from you, but can you learn from me? With so much being published, what criteria are used to ascertain what is relevant to a scholarly project? A dichotomy that is central to research in science education typifies an all too familiar standpoint—I am speaking, but are you listening to me? We have all endured the experience at professional meetings of a critic who fails to address the capital in a paper or paper set and instead rails against what has been done as she launches into an exposition of her preferences. (To avoid cumbersome constructions I use feminine pronouns whenever male or female pronouns might have been used.) The authors of the papers being critiqued experience violence because the merits of their work are not explicated and the critic reifies her own standpoint as she presents alternatives. I regard such practices as dangerous to the welfare of science education.

In a recent rejoinder to several critics Anna Stetsenko (2008) addressed what each of three critics had contributed to her understanding of cultural historical activity theory based on review essays each had authored. Instead of reiterating her stance and tearing apart critiques, Stetsenko addressed each point of criticism and each alternative that was presented, showing how difference can be used as a foundation for further learning. In so doing she endeavored to build coherence around difference and go beyond what any of the previous texts had done. That is, her original paper and the three review essays became a foundation on which further learning could build. In authoring her rejoinder she addressed the points she considered most salient in the three review essays and seemingly asked what more is there? She did not assert that her position was right and theirs wrong, and she did not create an alternative that excluded the positions of her critics. Instead, Stetsenko created something more, a rendering that included her own initial articulations, points raised in the review essays, and fresh ideas that build from the dialogue. Though she may not have been thinking of Kwame Appiah (2006) when

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she wrote her paper, the approach is reminiscent of having built cosmopolitanism based on difference, whereby each of the authors was respected for her difference and Stetsenko’s final paper emerged as one that acknowledged the value of others’ standpoints and did not seek to persuade them to accept her standpoint. Stetsenko showed that she was able to learn from difference.

In this chapter I use an autobiographical approach to address issues that are central to the purposes of this book, including the role of theory in explicating research issues, the methods used, and the values attached to theoretical products and transformed practices. I also discuss ways in which psychological models for teaching and learning science have saturated scholarship in science education and I identify pathways for moving forward, not by establishing dominance over our former oppressors, but by creating scholarly alliances with them. Such an approach sets the stage for learning from the different standpoints presented in the remainder of the book.

**Reflexively Choosing My Own Theoretical Frameworks**

Throughout the 1980s I searched for ways to theorize teaching and learning to teach science. In some respects my doctoral work at Georgia interrupted a process that began with Jean Piaget in the mid 1970s and embraced radical behaviorism during the time in which I studied for my doctorate. However, on my return to Australia I took a position as a science teacher educator. The theories I used for teaching were inadequate for my roles as a scholar and as a professional. At the time I was interested in better understanding teacher learning, especially how to use conceptual knowledge to improve teaching. This concern became paramount and in a period of 15 years I moved through an array of theories, employing them as a basis for my science teacher education programs and framing research questions and methods. There were many contradictions that demanded solutions. For example, reductionism, based on a tendency to describe social life in terms of variables, greatly expanded the scope of research and focused on better understanding the connections between the variables. However, a methodology that employed statistics necessitated at least some effort to conform to the assumptions underlying the validity of statistical tests. An example is the assumption that the subjects involved in research are independent and that their learning (or changes in the dependent measures) is attributable to and represented by the relationships included in the statistical model—that is, by the variables and relationships between them. Also, the focus on variables prevented me from researching issues, such as macrostructures, that were of most importance in science education. These included the impact of national and state policies, international “movements” such as those associated with the curriculum reforms catalyzed by the launch of Sputnik, and an increasing emphasis on external (high stakes) examinations. Accordingly, I began to use ethnography to study some of the issues in science education that transcended building models in terms of variables.
In a search for viable theory to make sense of teaching and learning it became clear that the methods I used to do research had to get into step. The theory of method, that is, methodology, presupposed research foci, and to an extent that I had not fully realized, the research foci presupposed the methods of investigation. Methods and foci were dialectically related, as were the theories on which they were built. Hence, changes in either methods or foci would perturb the whole and be evident in all parts of it.

Given the dialectical relationship between research foci and research methods it is no surprise that in my 35 years as an educational researcher there is connectedness between foci and method. In making this claim I do not want to create an impression of a mechanistic lock-step situation in which a change in focus produced a change in method. The relationship is much more subtle and at any stage there are contradictions, probably associated with other dialectical relationships that allow me to enact research being both conscious and unconscious of what I do. In such circumstances it is not surprising that my methods as a researcher would reflect what I have done in the past and would be responsive to the goals of a study and unfolding exigencies arising in doing the research. Before examining the relationships between research foci and methods in more detail I digress to briefly discuss some social theory that is germane to my goals for this chapter.

**Dialectical Perspectives on Research Foci and Methods**

The complexity of any social activity is such that, in trying to cope, knowledge comes to hand in resonance with structures of the fields in which participants are engaged. When events unfold as intended much of what happens occurs without particular awareness and it is only in a situation where breaches occur that an actor becomes aware and consciously enacts repair rituals. There are several theoretical frames that address social life in ways that cohere with my standpoint on the conscious | unconscious dialectic. For example, Pierre Bourdieu’s theory of habitus and Alfred Schutz’s stocks of knowledge at hand illuminate ways in which resonance occurs between knowledge at hand and the dynamic structures of a field to afford the enactment of culture. As I have explained elsewhere, fluency occurs when culture is enacted in ways that are timely, anticipatory, and appropriate (Tobin 2007). In these circumstances participants have a sense of the game and the unfolding encounters in a field produce capital. Encounters involve interaction and transaction, each of which presupposes the other; hence, interaction and transaction are dialectically related and integral to capital production in a field.

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4. I theorize production to always involve reproduction and transformation. That is production is equivalent to reproduction | transformation.
Setting aside tendencies to invoke either–or reasoning and to organize variables in causal, temporal sequences, I began to examine social life in terms of an array of coexisting entities that were constitutive of a whole. Sometimes, as in the example of patterns of coherence and contradictions, these entities might be thought of as recursively linked opposites, and in other cases, such as schemas and practices (i.e., culture), they are recursively linked, but not regarded as opposites. The adoption of a dialectical view of social life affords a more holistic way of thinking about experience and dispositions to consider questions associated with presupposition of coexistence and complex, recursive models depicting relationships among social entities (setting aside models that include unidirectional, causally linked variables, especially if the implication is that one set of conditions must precede another). The dialectical perspective opened up different possibilities within our urban science education project. As a way of thinking, co-presence raised questions that otherwise may never have arisen. For example, if in any social encounter a person is simultaneously a teacher and a learner, questions arise about how individuals are simultaneously both. A more complex picture then arises from our research. Similarly, if agency and passivity are dialectically related then encounters can be viewed in terms of co-presence of these entities and simultaneous consideration of other salient dialectical relationships, including interaction | transaction, reproduction | transformation, and individual | collective.

**Synchrony, Asynchrony and Lag**

A reflexive approach to what is to be done in a study affords synchronies between the methods employed and foci. However, what is planned and what is enacted necessarily differ and the dynamic structures of the research field, methods enacted, and research foci are mutually constitutive (i.e., co-related). In a context of my research in urban high schools my concern is to improve the lives of social minorities through science education. How this manifests in particular urban schools, for example in the Bronx of New York City, is to understand how to create productive learning environments in science classes characterized by differences that are often regarded in terms of categories having salience for students, such as race, ethnicity, native language, and social class. Similar categories also apply to teachers, who are usually significantly older than students. The focus for our research is the creation of solidarity in different social fields associated with science education (e.g., whole class discussions, small groups, labs). Hence, my approach to research involves broad foci and a history of having been involved continuously in similar studies for many years.

When we meet as a research group to consider what to do next an initial consideration is what we have done in the past and what we know of what others have done and learned. What is on the table is an extant literature, an evolving understanding of what we have learned from our ongoing projects, the motives of the research squad, and goals of individual researchers. The questions we have are
broad: what more is there to do and know? In an important sense the approach we adopt is ethnomethodological in that we attempt to learn about social life by looking beyond the equilibria associated with what Ann Swidler describes as “settled times” to probe the culture that presents itself in “unsettled times” as breaches occur, efforts at repair are enacted, and capital production reveals reproduction and transformation of macro-, meso-, and micro-culture.

In our consideration of what we know already we turn to an extant literature that has been produced from standpoints that differ greatly from ours. We regard it as important to incorporate diverse forms of scholarship that might have salience to our projects. What is regarded as salient is left to the researchers who comprise our squad. Not surprisingly, the theories and research we find most appealing is associated with scholars most closely aligned with our foci and methods. For example, our projects are informed significantly by the work of Wolff-Michael Roth and his coworkers such as Yew Jin Lee (e.g., Roth and Lee 2007). What we appropriate from their scholarship usually concerns methodology and theoretical frameworks from the genre of social and cultural theory. The substantive foci of their research rarely involve urban education and issues of social justice as primary. However, their written and spoken theories and methods are central to our work and are discussed in the context of what more needs to be done in almost every research meeting. Other criteria include foci and concern issues such as urban education, cogenerative dialogues, and coteaching. We know where to look within a network of scholars for ongoing research we regard as relevant to our projects. Mostly the researchers in this network have been close collaborators for many years and have now created their own research squads in cities and countries around the world.

New theoretical insights are continuously sought in endeavors to shine different lights on social life. The search is expansive and intensive with general reading in cultural studies, sociology and associated areas being supported by focused reading and understanding of particular scholars whose work is considered relevant. For example, my efforts to expand radical constructivism to include “the social” and a better understanding of action led me to the work of French scholars such as Pierre Bourdieu and Paul Ricoeur. During a decade of collaboration with Roth I expanded the list of theoreticians I sought to understand, gradually including them in a bricolage of theories that comprise a dynamic framework. Colleagues within my own institution also have played a significant role in expanding the theories I use in my research. For example, Elijah Anderson’s *Code of the Street* (Anderson 1999) introduced me to the salience of respect in my research, and Randall Collins’ work afforded me including theories of emotions and emotional energy as central parts of my frameworks for studying teaching and learning. Eventually I realized how important it is to reach out to others to learn about fields, such as cultural sociology, and I enrolled in some of the graduate classes in theoretical sociology taught by a colleague. The courses I took allowed me to navigate areas such as cultural studies and cultural sociology, leading me to Stuart Hall and William Sewell as theoreticians whose work was central to my projects.
Having identified the work of a scholar such as Randall Collins as having salience, my next move was to understand the scholars whose work underpins Collins’ theories. In this specific case I began to study other researchers who had done work on emotions, such as Jonathan Turner and Thomas Scheff. Over a period of years I then began to study Erving Goffman’s sociology, especially in relation to the interaction order and encounters. Goffman’s work on the sociology of interaction provides a basis for much of the research I undertake and a necessary next step was to move on to Harold Garfinkel’s ethnomethodology and the value in studying “what more?”

Making decisions about what is included in a bricolage is no slam-dunk. As is evident from the genealogy presented in the previous paragraph, starting with Collins necessitated the co-presence of many others on whom Collins and his peers had built their work. However, my project is mediated by more theory and empirical work than Collins and those involved in the sociology of emotions. For example, Stuart Hall and his work on diaspora and the creation of interstitial forms of culture is central and leads to important work by Homi Bhabha, Kwame Appiah, and others involved with creating solidarity around difference. From here the road leads to philosophers of difference, such as Paul Ricœur and Gilles Deleuze. A perspective we sought to adopt was to regard difference as capital; a basis for solidarity that takes advantage of differences within a collective without regarding them as deficits. For example, Jacques Derrida’s cities of refuge involve a form of cosmopolitanism built around the rights of refugees to live in a city and to benefit from hospitality (Derrida 2006). His proposal broke from a tradition of building solidarity around sameness to include the acceptance of moral stances that respect the rights of individuals to remain different, while preserving full rights of citizenry. Assimilation is not a requirement. Participants have the right to pursue their goals while contributing to the collective attaining its motives. Evidently, this value is the beginning of the thin coherence that kick starts the creation of solidarity within a field—each individual having the right to be different and the responsibility to contribute to the motives. These ideas are compelling when they are applied to the diverse students that turn up to science classes in New York City. Irrespective of their differences, from the teacher and one another, these students have a right, not just to be there, but also to reap the benefits of science education. It is regarded as a civic duty of all participants belonging to the cosmopolitan class to act in ways to promote the learning of others. Perspectives such as these have the potential to be used as referents for science teachers in diverse classes such as those found in the Bronx.

As is evident from the foregoing, there is a significant amount of theoretical flux in our research frameworks and theoretical shifts perturb research foci and methods continuously. For example, Bourdieu’s construct of field is salient to our work. We regard a field as a site for cultural production (i.e., reproduction | transformation) in which participants pursue motives in ways that are structured. Related to motives are activity (Roth and Lee 2007) and a plethora of dialectical relationships, such as agency | structure, self | other, and agency | passivity, that I use to make sense of social life. Initially we thought of the field more as a pasture...
with a porous border (Sewell 1999). The concept of weak boundary, as distinct from strong boundary, had implications for method as we began to look within fields for culture that did not belong there. For example, in schools we observed that street culture was enacted to the detriment of students seeking to become educated as a means for social advancement. Theorizing a border shone light on transitions and comparisons between those who were close to the border and those who were immersed “deep” in the field. Of course questions also arose about what constituted a boundary. We decided that boundaries were structures, in which case there was no need to regard boundaries as separate entities. This realization led us to see the parallels between social fields and those from physics, such as in electric, magnetic, and gravitational fields. The possibilities began to excite us as we were suddenly freed from a reluctant tendency to relate fields to particular times and spaces. Freed from this restraint we were able to allow fields to move freely with individuals through space and time. Hence, border crossing became a phenomenon that was no longer salient in our research since there were no borders. The situation of street culture entering the school then invited a different way of doing research on the issue and the possibility of solving such problems in very different ways, perhaps through the lenses of creating and legitimizing interstitial cultures. Also, the dialectical relationship that constitutes macro-, meso-, and micro-lifeworlds could be explored quite differently because it was now clear that at any time and in any space individuals could be involved in activities in multiple fields. As our theoretical perspectives evolved, so too did the research foci and methods used in our studies.

From what I have said it is easy to see that lags will occur and it might be inferred that I am arguing for temporal ordering whereby a change in focus causes a change in method. Such an inference is not implied and I regard it as a dangerous stance. Method and focus presuppose each other and there is no temporal imperative. In fact changes in method can elicit fresh theoretical perspectives and new research foci.

**Hegemony of Psychological Models**

For as long as I have been a science educator, psychological models have been the mainstay of theory supporting science education. During my days as a high school science teacher my theories mainly were tacit. I had a sense of students needing to engage and stay focused in order to learn. I also knew that through their efforts they could learn from me and from their peers. Later I learned about the psychology of Piaget, when I became a curriculum designer who wrote teachers’ guides and students’ workbooks. Having such an emphasis meant that the activities we recommended for teachers were intended to get students actively involved and to learn science in terms of what they knew already and were interested in learning. To a significant extent the approach emphasized the importance of individual students making sense of their experiences by resolving cognitive conflict.
When I went to the University of Georgia to do my doctorate I joined one of the most behaviorist science education groups in the USA. Although the faculty members were open to the use of other frameworks for making sense of teaching, learning and curriculum, most of them were committed to behaviorist ways of thinking about education. This was especially true in regards to methodology, since most of the faculty used methods built around positivism. In required courses I had to show an understanding of and value for Julie Vargus’s radical behaviorism and apply it in numerous contexts. However, when I returned to Australia and endeavored to apply radical behaviorism to my research, I had little success. To advance my research project I needed to move beyond methodological positivism and identify frameworks to illuminate teaching, learning, and learning to teach in a more productive light. I searched avidly for new ways to think about methods and especially to make sense of teaching, learning and learning to teach. I explored neo-Piagetian theories and then radical and social constructivism. Fortunately, Ernst von Glasersfeld’s work in language and mathematics education seemed highly applicable to my experiences and perceptions of the pressing problems in science education. Glasersfeld’s insights encouraged me to adopt radical constructivism and consider the potential applicability of other theories such as George Lakoff and Mark Johnson’s work on metaphors. My understanding of theory was changing in that I regarded theorizing as akin to shining lights on experience, providing fresh perspectives and catalyzing research projects that could evolve continuously based on the adaptation and adoption of chosen theories. Also, I was learning how to not regard discarded theories as wrong. It was apparent that the use of any set of theoretical lenses illuminated certain aspects of social life while failing to discern others.

“I don’t believe in that sort of research,” was the preamble to a question from a scholar at the back of the conference meeting room. “The audacity of the guy,” I thought as one of Australia’s most eminent scholars admonished a master’s degree student who had just given his first presentation at a national conference (Berry and Tobin 1984). Like so much of the work done in my research squad in the early 1980s, this study reflected a transition toward ethnography, from process product research that explored teacher and student variables in relation to one another, students’ perceptions of the learning environment, and science achievement. Though I did not realize it at the time, the first shot had been fired in an exchange that was to continue to the present day. Apart from the public comment being discouraging from the student’s point of view, this eminent scholar’s stance was imperialistic. From the east coast of Australia, he was signifying to those in attendance that our way of looking at science education was not up to par, and that such work in the future would receive public rebuke.

A year or 2 earlier the signs of this first public salvo were evident in a meeting of the Australian Science Education Association (ASERA) at Macquarie University in Sydney. I had just returned from the United States, where I had done my doctorate in science education. I was involved in my first meeting of ASERA and noticed the dominance of scholars from Monash University (in Eastern Australia). One of them, Peter Fensham, was the first professor of science education in
Australia and his presentation to the group involved an innovative way of examining student misconceptions. The topic was concept mapping, an approach that was new to me and did not align with my current orientation to doing research on teaching and learning science and learning to teach science. Although I was an enthusiastic participant in the exercises Fensham used to introduce participants to the theory and methods, I had little inclination to use concept mapping in my research or to embrace conceptual change theory. My stance was very much in a minority and I felt I was being shown the approved way to proceed with research in science education. There seemed to be an expectation that one “tried-and-tested” way of looking at science education was preferred and conceptual change theory was oriented in the right direction.

The growing oppression I experienced in Australia did not deter me from plotting my own course and before I left for the United States in 1987 I continued to develop my own frameworks, foci, and methods. My pathway toward theory-rich, qualitative studies of teaching, learning, and learning to teach science developed in my research squad on the west coast of Australia as the conceptual change approach swept from coast to coast, becoming the predominant theoretical voice in Australian science education.

What I experienced in the early 1980s in Australia was just the tip of an iceberg. Conceptual change theory was to sweep the world of science education and dominate the ways in which science educators did their work. The theory did not fit my way of looking at science teaching and teacher education and my search for appropriate theory took a path that increasingly diverged from the ones chosen by my colleagues in science education. Because my foci were different, the extent of the divergence increased with time and for the most part I only noticed it as an issue when peer review occurred. When peers from the conceptual change group reviewed my proposals to seek external funds for research, present papers at national meetings, and publish books and papers, I often experienced their ire at my failure to conform to their senses of what was of value, how to think about knowing and doing, and which methods to use. If my work was to be accepted in peer review, I felt pressure to conform. Alternatively I could accept a minority status whereby rejection would occur with a higher incidence because of the objective blades of peer review and the consequences of failing to conform in a monosemic field.

I regard my experience of peer review in the past 30 years as evidence that there is an effort at work in our community that intends to create a master narrative for science education. Powerful brokers in Australia and other parts of the world developed a theory focused on the concepts of science and for this reason there was appeal to scientists who could look at their work and make sense of what was being done. Failure of students to learn key concepts from the science canon clearly was an important problem to scientists and massive support was given to those who focused squarely on science subject matter, the misunderstandings of youth, and ways to extinguish wrong ideas and replace them with scientific truths. An irony in what was happening is that my standpoint, though different than the mainstream view, embraced polysemy. It was fine by me for good scholars to do
formal analysis through the theoretical lenses of conceptual change supported by methodological positivism. Yet, others’ adherence to monosemy led them to view my standpoint as incorrect, a misconception that needed correction! To be accepted in most peer review contexts I would have to change my ways of thinking and doing or pay the price for my difference.

The combined impacts of conceptual change and positivism on research in science education have been totalizing, saturating sensibilities to the extent that to raise alternatives has been difficult in terms of risking rejection and marginalization. Yet, even within the dominant paradigms there is now a realization of a need for change. David Treagust and Reinders Duit (2008), two of the leading proponents of conceptual change theory, noted that “certain limitations of the constructivist ideas of the 1980s and early 1990s led to their merger with social constructivist and social cultural orientations that more recently resulted in recommendations to employ multi-perspective epistemological frameworks in order to adequately address the complex process of learning” (p. 299). One of the early pioneers of conceptual change theory in science education was Peter Hewson, a nuclear physicist who turned to science education. Hewson noted:

My physics background gave me a view of the world that stressed the importance of unique, exact, confined, causal accounts of natural phenomena. Initially I did not see that this needed to be adjusted when I stopped focusing on atoms and nuclei, and turned my attention to a student learning physics. While I knew there was a context in which people learned, the notion of an isolated system was a conceptual device for effectively ignoring it. In time, however, the limitations of trying to identify unique causes became apparent, and I came to appreciate the complexity of factors that influence learning, and their interactional nature. While there clearly is an individual, cognitive character to our learning, we are also social, cultural, affective creatures who respond to those around us in a variety of ways that are strongly influential of the questions we ask, the opinions we espouse, and the understandings we create. (Hewson 2007, p. 131)

I maintain that a fertile approach to research in science education is grounded in the adoption of a fresh theoretical framework for learning and doing science. I use theory in the spirit of Karl Popper (1974) that “our theories are our inventions; but they may be ill-reasoned guesses, bold conjectures, hypotheses. Out of these we create a world: not the real world, but our own nets in which we try to catch the real world” (p. 46). I argue that tinkering with the dominant ideology does not offer the fertile insights needed in science education. In making a case for change I am not arguing for a new way to replace the old. My stance is in opposition to the idea that there is a correct way to see the world, to interpret the work of others, and to apply theory to science education. I am arguing for polysemy.

Accepting Difference

Adherence to monosemy, an ontological stance, may be at the root of a dire problem in science education. As I mentioned in the previous section, my polysemic stance supports a multilogical foundation for science education. In contrast,
monosemy can motivate moves toward assimilation; a one-size-fits-all approach to science education with winners and losers, oppressors and oppressed. This has been the status quo during my career as a science educator. Throughout the history of science education there have been paradigm wars, debates in the literature in which the expectation was that one way of seeing the world would prevail. In a sense the view of our field was shaped by Comtean positivism that there was a correct theory for learning and teaching science and the role of scholars was to find that theory through careful conceptual analysis supported by formal empirical analysis. Obviously polysemy is an alternative to this quest for the Holy Grail.

There are numerous stances that can be taken to others’ scholarship. Our first inclination was to ignore most conceptual change research. Even the best of the research had little relevance to our projects and to an increasing extent the foci and methods were such that there was little overlap with our chief concerns. Perhaps our approach might be thought of as telling the group to go play in some other sand patch. There was no room for conceptual change research or researchers in our projects. There were exceptions. For example, in a large collaborative study in Australia we intentionally included diverse perspectives in the design. Leading researchers in that study included David Treagust, Barry Fraser, and John Wallace. However, the book project we planned to disseminate what we learned from the study was never a priority and was not published. Instead, papers and chapters were eventually published separately. This example of involving scholars with diverse perspectives in collaborative research highlights the difficulties of navigating difference within the field of science education. There seems to be greater payoff for the hard work of being a scholar, to collaborate with those with whom our approach is commensurable, where the theories comprising our bricolage are complementary.

My theoretical journey from 1980 to the present day might be described in terms of six epochs, though with a finer grain size it seems as if each year (at least) might be listed as a significant point on a continuously evolving trajectory. As Thomas Kuhn pointed out in relation to scientific discoveries, such as the discovery of oxygen, it is not easy to pinpoint a date at which a particular way of thinking was predominant, even in autobiography. Given this caveat, the six points on my theory trajectory are: neo-Piagetian, radical constructivism, metaphors, social constructivism, theories of action, and cultural sociology. As is the case in any trajectory any one point can be examined with a finer grain size to reveal an extensive bricolage consisting of theories that are to some extent complementary. In an endeavor to show how this works, I take cultural sociology as a point of departure for the remainder of this chapter.

Social and Cultural Theory

The search for new theoretical spotlights is relentless. In 1997 I took a bold step in pronouncing that learning involved cultural production. The stance included