AVANT, Vol. IV, No. 1/2013
ISSN: 2082-6710 avant.edu.pl
DOI: 10.12849/40102013.0106.0021

From information processing to the whole person

Wolff-Michael Roth
Lansdowne Professor of Applied Cognitive Science
University of Victoria
mroth[]uvic.ca

Received 14 June 2013; accepted 18 June 2013; published 30 June 2013.

What has changed in Wolff-Michael Roth's approach to research on knowing, learning and teaching since "Enhancing student achievement through computer-generated homework"? What is the Author's perspective on interdisciplinarity, multidisciplinary and transdisciplinarity? Is there any importance of cooperation between cognitive science and social studies of science? What will be the future of mathematics?

A lifelong pursuit of sense

What has changed in Wolff-Michael Roth's approach to research on knowing, learning and teaching since "Enhancing student achievement through computer-generated homework" (Milkent and Roth 1989)

When I look back today at my scholarly career—the research methods I used, the theoretical frameworks I adopted and abandoned when the phenomena I was interested in exhibited their shortcomings, the topics of inquiry—I can imagine that others feel that I have covered almost the entire expanse of the scholarly field (see Figure 1). I have published in the natural sciences, sociology, psychology, anthropology, linguistics, and, above all, in different educational subfields. I have used high-powered statistical tools, neural network modeling, voice analysis, phenomenological reduction, discourse analysis, conversation analysis, and hermeneutic interpretive methods. My interests, theories, and methods crisscrossed each other to such an extent that it is impossible to summarize my scholarly career in any linear narrative. In fact, in my most recent collaboration with colleagues from an Australian university, we do expert/expert studies among pilots not unlike the expert/expert studies

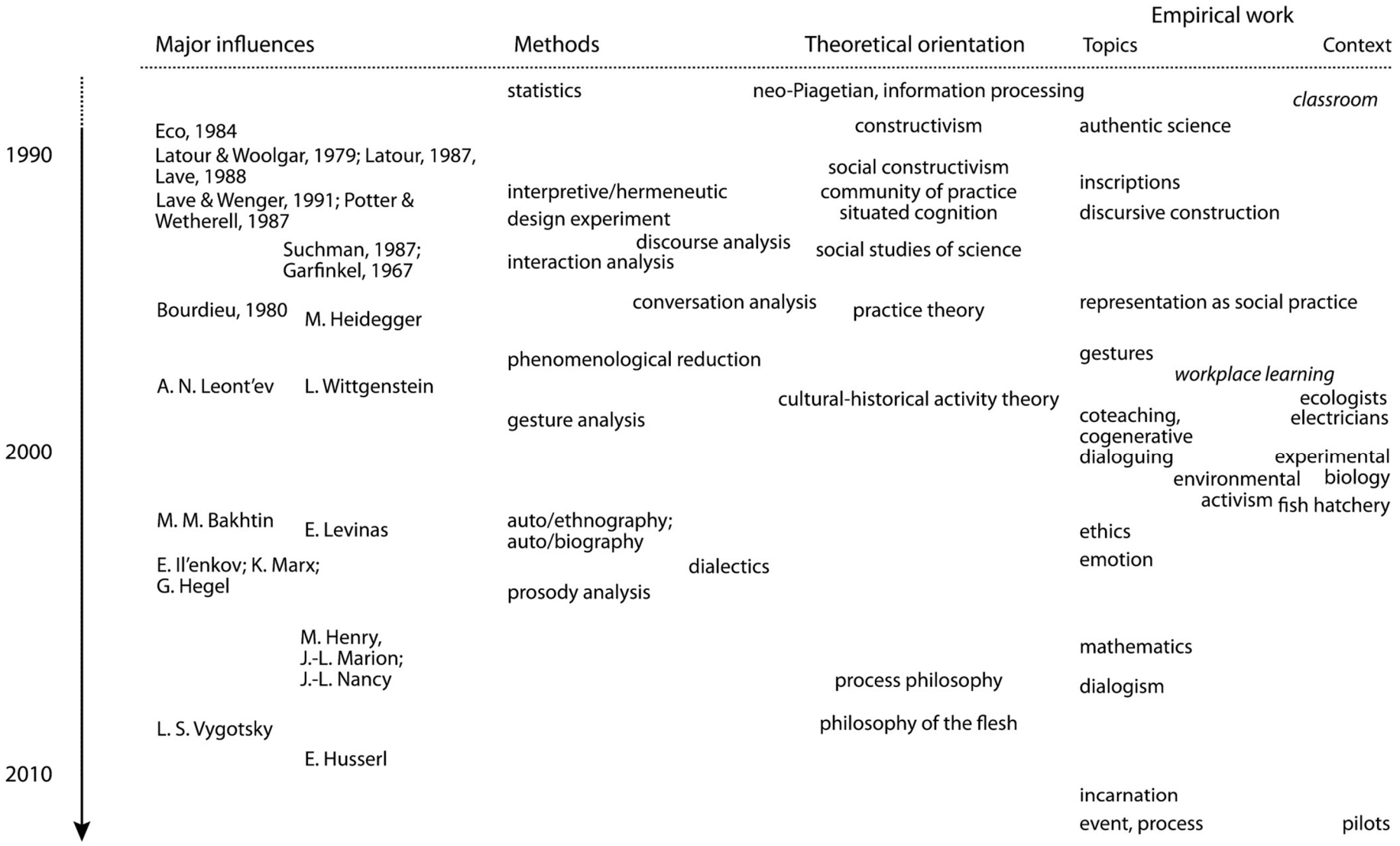
that I had done among scientists about 10 years ago. Overall, there has been a trend from considering manifestations of being (Heidegger's *Seiendes* [beings]) to Being (*Sein*), the whole person.

The historical background

The paper referred to in the question was written while I did my doctoral studies at the University of Southern Mississippi (Hattiesburg, MS). It is the result of work with my doctoral supervisor, who really had abandoned doing any research, but who, because of my insistence, agreed to collaborate on an empirical piece of work. Our study arose from a theoretical and empirical context of the time, itself part of a longer history of interests and inclinations, without which the work I conducted since then cannot be understood. In Figure 1, I map some of the major authors (books) I read, research methods I learned and methodological tools I acquired, the theoretical contexts of my research, and the topics of research studies and empirical settings. In anticipation of the text that follows, even the quickest of glances will lead a reader to conclude that I am "all over the ballpark," as we would say in vernacular English, with respect to the dimensions articulated in Figure 1. This perhaps dizzying array of methods, theories, philosophical underpinnings, and empirical settings is a reflection of a search for coherence, my lifelong pursuit of trying to make sense of human knowledge across and above boundaries created and maintained by academic disciplines and paradigms.

An important theme that was latent in my earlier life became increasingly central and salient in my work as a researcher: the separation of body and mind in most theoretical approaches and the lack of attention to lived experience, that is, the person as a conscious being that actively orients in the (social, material) world it perceives. The earliest point in my life when I had dealt with this issue was in the uptake of meditation and a development of Zen. This orientation allowed me to overcome what I had come to experience as the prison of mind and the repeated depressions I had experienced during my late teens. But these experiences and my scholarly career initially were far apart.

Figure 1.



This map integrates references to major empirical and philosophical influences on my work, research methods used, theoretical frameworks, and topics and context of empirical work.

By the time I began my doctoral studies, I had completed an MSc in physics (Würzburg, Germany) and had been a teacher of science and mathematics for half a decade (Quebec and Newfoundland, Canada). As I was a natural scientist, structuralist approaches to learning and knowing made sense to me. Thus, I pursued neo-Piagetian approaches, which combined developmental psychology and information processing theories. I used a think-aloud protocol to study learning and development as adults did ratio and proportion tasks in the context of problems that increased in difficulty once they mastered a particular level. I used statistical analyses to correlate developmental rates and short-term memory measures. At the same time, I took courses to obtain a minor as a statistician and also started a second PhD in physical chemistry. In that first paper referred to above, we reported, using multivariate statistics, the results of a study that compared the achievement of students in two physical science courses. In one, students has, as their homework, word problems presented to them on a computer in as many sets of 10 as they wanted (e.g., until they achieved mastery or the number correct they were happy with), whereas in the other class students did one set of 10 paper-and-pencil homework problems. Following this study and my dissertation, I did a number of other investigations related either to statistical methods or to the effect of short-term memory on achievement.

A radical change in direction: theoretical and methodological

Two major shock experiences radically changed my research direction. The first was actually a double shock at the University of Indiana (1988–89), where I learned about (radical) constructivism and semiotics—I had dinner with Umberto Eco and two colleagues—and an institutional context where colleagues and superiors told me that I was inept to make it as an academic. I left the university to take up a position as a department head of science and physics teacher in a private school back in Canada (1989–92). I quickly realized—and this was the second major shock—that everything I had done and learned during my PhD appeared to be useless: As a teacher, (a) I really had no access to short-term memory and (b) to help students I needed to know what hindered their learning, their personal specifics and contextual contingencies, which are dimensions that I had treated as unexplained variance as a statistician. A major revelation after the first year back in high school changed my life.

During the summer of 1990, I was teaching physics to elementary teachers at the University of Victoria. At the bookstore, I bought some books that lastingly influenced what I was doing and how I was doing it. These included Bruno Latour and Steve Woolgar's *Laboratory Life*, Jean Lave's *Cognition in Practice*, and Latour's *Science in Action*. These three books—as well as others related to the areas of the study of science and ethnographic work related to practice

and the situated nature of cognition, including Lave and Étienne Wenger's *Situated Learning*—shaped much of what I was doing for the next decade. At the end of that summer I said to myself, "I can do this kind of work," referring to the study of learning in the contexts where it occurred. This literature also influenced me as a teacher, because I emphasized even more than I had done before what was then referred to as *authentic learning*. As a department head, I bought a video camera to be used for professional development, which I, as other teachers in my department, used to study investigate and learning as these really occurred in our classrooms.

In our science classes, students engaged in the design of research studies, which they then completed and reported back to the class. This led to the study of "authentic science," "inscriptions," and the "communities of practice" that emerged. But I was also interested in the ways in which teaching science in a radically different way influenced the ideas students articulated about science and learning. I recorded classroom discussions concerning epistemology, interviewed students, collected essays on a variety of topics on learning and the nature of science. Whereas my first analyses were based on *content analysis*, I had come to read several books on discourse analysis and, during 1992, on *discursive psychology*. As I had collected over 3,500 pages of typewritten transcriptions, I wrote several papers in the course of which I also learned the method. Although many scholars in science education felt at that time that it was difficult to publish qualitative studies in the pertinent journals, every study that I submitted was accepted. In the context of difficulties I experienced with my high school administration, I accepted in 1992 a position as a statistician in the Faculty of Education at Simon Fraser University (Burnaby, British Columbia).

With funding from the national granting councils, I continued the kind of work I was doing. Because the teachers I was working with felt insecure about using novel approaches, I was teaching curriculum units using artifact design as a major focus. Children from grades four to seven learned physics in the course of designing architectural structures and machines. In the context of this empirical work, I was continuing to use the *design experiment* as main approach to method, combining ethnographic work with extensive video recordings and formal experimental studies of salient phenomena. I had been to XEROX Palo Alto, where I met with Brigitte Jordan and participated in one of her *interaction analysis* sessions. It became a method of choice for years to come, which I honed in the analysis of children designers at work. Although I had started out identifying as a science educator, publishing in the journals of that field, my cognitive oriented studies, the extensive data bases I established, and the thorough attention to the analyses allowed me to publish in the journals of an emerging field: the *learning sciences*. I had made that move because around 1992, I had felt that I knew how to publish in science education and needed new challenges.

An orientation towards practices: social studies of science and pragmatics

In parallel, I was developing a line of work on Latour's notion of *inscriptions*. Already, the data I had collected with my fellow teacher Michael Bowen at the private high school had provided me with data sources for analyzing students' use of inscriptions. This work intensified at the university, when Michael came to do his PhD with me and Michelle McGinn both did an MA study on situated learning of mathematics and worked with me in the design classrooms. With Michael, we set up studies of ecologists at work and think aloud studies focusing on scientists' interpretations of graphs (e.g., Roth & Bowen 1999b). Subsequent studies among electricians, fish culturists, and experimental biologists became the context for studying scientific practice, the use of inscriptions, and mathematics at work. It was in the context of these investigations that my ideas about *graphing as social practice* emerged. Because I conducted joint analyses with my doctoral students, interaction analysis became our method of choice and, together with it, an orientation to use *conversation analysis*, an approach that I had seen in the work of Charles Goodwin, an applied linguist some of whose work is also studying science and scientific practices. As I had become a member of the *Society for Social Studies of Science* in 1992, this work led me to publish in the main journals of that field (*Social Studies of Science*, *Public Understanding of Science*, and *Science, Technology, & Human Values*). This work also oriented our curriculum design work—e.g., fostering learning environments that allowed students to participate in environmental activism as a way of being, and developing as, citizens (e.g., Roth & Désautels 2004).

Being scientists ourselves, Michael Bowen (MSc, biology) and I negotiated entry among scientists by offering our help in collecting data. The idea of *apprenticeship as scientific method* jibed for us with Lave's work and the then-current interest in apprenticeship as analytic framework. It became the context for my interest and writing in *auto/ethnography* (and *auto/biography*), but my approach was different from the literature in the field associated with these names. I felt that the authors categorizing their method as autoethnography were too concerned with Self (*auto-*) and too little concerned with the study of culture (*ethnography*). I had a sense that there was too much of self-indulgence and too little orientation to do scientific work that held muster in methodological debates.

As I was interested in scientific communication, I began to notice the importance of gestures. Although there was a flurry of papers on the role of gestures in science learning during the 1999–2004 period, my interest was actually awakened in the context of the design experiments on children designers. At the time, I was doing whole-class discussions with the children arguing about the design of simple machines. Students and I were drawing alternative de-

signs, and we argued pointing to and gesturing over inscriptions. This led to a Latour-inspired paper "Thinking with Hands, Eyes, and Signs," which, though much less cited than my other gesture work, opened up for me a line of work that I only pursued with great intensity much later. I saw that there were definite links between the gestures and the subsequent emergence of scientific language (e.g., Roth 2000). It is in the context of gesture studies that I expanded my work to the areas of linguistics and came to publish in journals concerned with semiotics (e.g., *Semiotica*), pragmatics (e.g., *Journal of Pragmatics*) and cognition (e.g., *Discourse Processes* and *Pragmatics & Cognition*). While pursuing a review of the literature on gesture studies, I realized that although there were few studies on gestures in education at the time, there was a humongous body of work in anthropology, on the one hand, and psycholinguistics, on the other hand.

Coteaching and cogenerative dialoguing: a turn to praxis

An important line of research developed out of the children designer studies. Because the resident teachers felt ill at ease to teach through open inquiry, we agreed to teach together. I would take major responsibilities for the science and science pedagogy side; and the resident teachers were responsible for more general pedagogical issues. But we agreed that we had to work together, and in fact, everyone was responsible for all aspects of the lessons. As part of this work, other teachers visited often; and I invited them to interact with (and teach) during the time they spent in our classroom. I noticed that in the course of teaching together, all participant teachers developed. That is, not only did the children benefit from having more than one teacher, with an expanded, collective level of expertise, but also the teachers themselves were provided with opportunities to learn. A new field of interest and research opened up, which intensified when I began working together with Ken Tobin at the University of Philadelphia, who also had begun to look at the changing nature of science classrooms when two teachers (one experienced and one new, or two new teachers) were working together. Our work on *coteaching* was born. In that context, we also realized that if we wanted classroom environments to change, the teachers and students themselves had to participate in making sense of events and in designing courses of actions that would change their working conditions (e.g., Roth, Tobin, Zimmermann, Bryant, & Davis 2002). We were about to develop *cogenerative dialoguing*, a name for the sessions we had that brought together everyone teaching and two or three student representatives. We developed the pair of coteaching | cogenerative dialoguing—which we thought about as a dialectically related pair forms of *praxis*—not only as a *praxis* for changing the conditions of schooling, but also as an approach to learning how to teach, teacher evaluation, teacher supervision, and so on. Although my theoretical approach was initially grounded in Pierre

Bourdieu's *Le sens pratique* [The practical sense][226], it was in the context of the work on coteaching and cogenerative dialoguing that I came to read Alexei N. Leont'ev's *Activity, Consciousness, Personality* and realized the importance of cultural-historical activity theory to understand the way in which *society*—rather than individual, group, or community—mediates what we do and how we participate in the various contexts of our lives (e.g., Roth 2004). *At the Elbow of Another* (Roth & Tobin 2002) and related articles that preceded the book reported on the work on coteaching and cogenerative dialoguing, while also becoming my entry point to cultural-historical activity theory.

In the context of that work, I also developed the competence to use voice analysis for the study of prosody, which, according to studies in psychology and sociology, was related to interactive behavior (e.g., power relations) and emotions. I began to propose changes to cultural-historical activity theory to include the study of emotion in a more objective manner, different from what individuals might say they feel, which I applied in the case of the fish culturists (e.g., Roth 2007).

Cultural-historical activity theory became a dominant strand in my work, so much so that I became, five years after beginning to intensively work with the approach, editor of *Mind, Culture, and Activity*, a major forum for scholars using this theory (2005–2010). In the course of attempting to understand the theory, my understanding of dialectics developed and, ultimately, my understanding of its process aspects that are unattended to in the current literature also did. What I had not really understood initially was the process aspect in Karl Marx's conception of commodity, and its relation to the category of *inner contradictions*. I (re-) read Marx and Georg Friedrich Hegel and, importantly, Evald Il'enkov's presentation of dialectics and his analysis of Marx's method. This, in turn, allowed me to understand the work of the Russian psychologist Lev S. Vygotsky, who, though I was familiar with the name, had not really influenced my thinking. I came to understand that it is not the citing of Marx in some of his works—which a number of scholars consider to be lip-service to the reigning powers—that made his work Marxist, but, rather, the fact that the method he used followed *Das Kapital* [Capital], a work that he recommended, in a text on the historical significance of the crisis of psychology, as the type of book to be written by/for psychologists.

[226] In English, the text was published under the title *The Logic of Practice*, which does not do justice to the *sens* [sense] in the original.

A phenomenological turn

Over the years, a strand of work emerged in the context of my readings of Martin Heidegger and, simultaneously, ethnomethodological studies of work (Harold Garfinkel and Lucy Suchman). Although I had bought my copy of Heidegger's *Sein und Zeit* [Being and Time] in 1977, it became important to my work only in the context of situated cognition and cognition at work. I was dissatisfied with the absence of the *person* in the (my) studies of cognition, especially of the role of awareness in what we do and how we do it. As a teacher, I felt that much of the work on teaching and teacher learning did not describe my experience as a teacher; and the research on learning did not describe or make sense in the context of my own learning. Yet what I was doing in the classroom was in response to that of which I was aware. My initial references to Heidegger and the use of tools, which may be ready-to-hand (*zuhanden*) or present at hand (*vorhanden*) were already grounded in my use of everyday tools around the home. I do not represent and interpret the hammer in the way that constructivist approach suggests; I am, as Heidegger described, concerned with hammering a nail into the wall. But this interest took on increasing importance in the context of my interests in trying to understand practices through the living body of the person.

A review of a book on mathematics education provided an early context for pursing learning through the eyes of the learner. While I was reading the book, a graduate student of mine handed me mathematics word problem that kept my attention. I kept the traces of my engagement with it in my research notebook; and these notes then became a data source for the book review through the lens of the person doing mathematics. Whereas the book upheld the post-modern diction that "there is nothing outside text"[227], I used the analysis of my experience to show that there are things that a singular attention to text simply misses. We later used this phenomenological approach to study other topics in our research laboratory, including the use of inscriptions (graphs) in biology lectures (e.g., Roth & Bowen 1999a). The work in this area intensified in the late 1990s, when, together with Domenico Masciotra, we looked at high-level performances and the road to get there. Domenico was a seventh-dan black-belt karateka. I had been a world-class rower and was an experienced teacher, researcher, cyclist, and gardener. Both of us engaged in meditation; in my case, it is associated with the practice of my interests in a Zen approach to life. That is, my interests and practices pursued since the early 1970s now (re-) surfaced as a major field in my research. This work became the starting point of a line of work that increasingly intensified over the last two or three years and currently concerns the *eventness of the event*, *incarnation*, and *radical passivity*.

[227] "Il n'y a pas d'hors-texte" (Derrida 1967: 227).

Over the years, I became dissatisfied with the scholarly emphasis on *agency* and the *subject*, which underestimates the everyday experience that we are not only *subjects of* activity, but also are *subject to* and *subjected to* events that are larger than the sum total of people and things. For example, we sit in university (funding agency) committee meetings, and although all of us may have come with some idea about the results, there often are unexpected turns of events and associated outcomes. Even though all members may have come to the meeting in favor of a particular decision, the committee may end up making a very different one. Social processes sui generis *cannot be reduced* to the actions and intentions of individuals. This is one aspect of radical passivity. The other one arises from phenomenological analyses of the most basic bodily actions and perceptions have active and passive dimensions simultaneously. Once I realized this in detailed analyses of videotaped lessons, I found these consistent with recent ideas in material phenology (Michel Henry), and then found that the same ideas had already been articulated not only by Edmund Husserl, but also by Pierre Maine de Biran (1766–1824) more than two centuries before our time. This backtracking has become one of the major themes in my work. I first read Jacques Derrida before getting deeper into Maurice Merleau-Ponty, Heidegger and Husserl; I first read Leont'ev and Il'enkov before tracking backwards to read Marx and Hegel. I first read recent phenomenological analyses concerned with the event before seeing its importance in Mikhail M. Bakhtin and then backtracking relevant ideas to Friedrich Nietzsche.

In this backtracking, I also came to understand linkages in the way scholars work. For example, many younger scholars seem to take Derrida as if he existed outside of a cultural context. It was while reading Heidegger's *Identität und Differenz* [Identity and difference] that the similarities in their styles of writing struck me; it was also evident in Heidegger's slow and meticulous analyses, for example, in *Unterwegs zur Sprache* [On the way to language], that one can see strong kinship with Derrida's slow and recursive reading of texts that he engages with and writes about.

An increasing focus on linguistic issues

In parallel to my work on cultural-historical activity theory, which in Germany developed into critical psychology and the science of the subject [*Subjektwissenschaft*] (e.g., Klaus Holzkamp), French philosophy, especially the exegetes of Husserl and Heidegger, began to dominate in my reading list. I extensively read Derrida, Emmanuel Levinas, Jean-Luc Marion, Paul Ricœur, and Jean-Luc Nancy. This multifaceted reading allowed me to make unusual connections. For example, it was in the course of reading in parallel Derrida, Ricœur, and Marx that I realized some fundamental structural parallels between the three. In one instance, I began to replace the word "commodity" in

Marx by the word "sign," and every example of a commodity by an example of a sign. I ended up with text fragments that were so similar to texts by Derrida and Ricœur that I ended up publishing a paper in *Semiotica* on this phenomenon (Roth 2006a). The parallels were interesting to me in their own right; but even more interesting was the question why and in what such a parallel might exist. After some study, I concluded that at the heart of both—commodity and sign—there lie *substitutability*, *exchangeability*, and *iterability*. I realized only last year that these also lie at the heart of the philosophy of language developed in the group around Bakhtin and Valentin N. Vološinov. In their work, there is the same double dehiscence surrounding the *statement*[228] that also appears in Marx's *commodity*: distributed over social agents and time. In the exchange as a whole, the commodity belongs to both, simultaneously being use-value and exchange-value—though it manifests itself differently for different participants—and the exchange is one whole *process* including a transformation of use-value into exchange-value. Similarly, in conversation, the statement belongs to the speaker and listener simultaneously—there is no sense of speaking of conversation if there is no listening—though speech intent (illocution) and speech effect (perlocution) may differ, and active perception and replying is *one* process spread out in time. Thus, even though there are scholars claiming Bakhtin's *dialogism* to be different from and even antithetical to dialectics, the very structures of the two approaches are common. This also allowed me to understand that Vygotsky and Bakhtin are more similar than they are distinct in method and theory, even though there are scholars who want to claim the opposite. Most apparently, both are interested in the flow of life and the irreducibility of real life processes (i.e., their interest in *unit analysis*).

Inner contradictions in the idea and pursuit of (inter-, multi-, trans-) disciplinarity

W-M Roth's perspective on interdisciplinarity, multidisciplinarity and transdisciplinarity

The preceding account of my work and the associated intellectual and empirical map (Figure 1) provide evidence of my life between (inter-), yoking (multi-), and across (trans-) disciplines. On any one day, I may work—alone or with colleagues—on multiple papers with different topics, theoretical frameworks, and drawing on different methods. Even in the same text, I might draw on multiple methods, methods and theories from multiple disciplines, or work between multiple languages, each with its own sonorities, ways of articulating the world, and background culture. I feel as if working both within particular

[228] Высказывание [vyskazyvanie] is often translated as "utterance," though for numerous reasons, I prefer "statement."

disciplines and outside of disciplines simultaneously. In any case, I seem to be working at the margins of many disciplines, simultaneously both inside and outside them. I did, in fact, work on several projects that brought together individuals from very different disciplines, not only within education, but also from the natural sciences, humanities, and social sciences. In those projects, I saw how difficult many colleagues found working with those of other disciplines. Collaboration across the disciplinary silos turned out to be difficult when social scientists accused natural scientists of objectivism and realism, and natural scientists accused social scientists (including educators) of "post-modern bullshit." Personally, I did not and do not have such problems and have, even as recently as 2008, participated in the publication of a paper in the natural sciences all the while writing a post-modern paper on heterogeneity and hybridity. What matters to me is the development of ideas, inherently a collective and hybrid enterprise, because nothing I do would make sense unless it were always and already intelligible to and directed towards the Other.

In one text on the topic of interdisciplinarity, I suggested that the term evokes the possible impossible. For as soon as we create, for example, a university department of interdisciplinary studies, we have created but another silo among disciplinary silos.

I tend to think about academic disciplines in terms of Ludwig Wittgenstein's language games that not only differ between themselves, but also within themselves. I have come to understand that there is no such thing as *a* language, self-identical with itself. One reason was already articulated in the works of Bakhtin, who insisted on language as a continuously changing phenomenon, so that it cannot ever be identical with itself because its very exercise is equivalent to change. Moreover, we know that language translates into itself, thereby constituting and linking non-equivalencies. For example, when we say something and our recipient says, "What do you mean?", we tend to say "the same thing," but in different words (i.e., differently). The recipient might then say, "Oh, I get it!" or "Oh, I understand!" We have an instance of two expressions not being different, but one (the first) is not understood, while the other (the second) is. Paraphrasing Derrida[229], I therefore hazard an impossibility: *1. We only ever have one (academic) discipline. 2. We never have only one (academic) discipline.* Whatever we do, whatever we think, whatever we talk about, is always already a hybrid of all the things we do as humans. There is a logic in what we do, which, as Husserl showed, fundamentally bottoms out in everyday and pre-noetic experience. This is so because whatever we do and say presupposes intelligibility that arises from living in society with others. We might then come to the conclusion about scientific culture that

[229] "*1. On ne parle jamais qu'une langue. 2. On ne parle jamais une seule langue.* [1. We only ever speak one language. 2. We never speak only one language.]" (Derrida 1996: 21).

Nancy arrived at in an analysis of culture in the face of the events around Sarajevo:

> Every culture is in itself "multicultural," not only because there is a prior acculturation or because there is no pure and simple provenance, but more importantly because the gesture of culture is itself a gesture of mêlée: of confronting, transforming, deviating, developing, recomposing, combining, cobbling together. (Nancy 1993: 13)

Science as culture and the culture of science are topics and fields of research. Read from such a perspective, further exegesis of the quotation would be working out the self-evident. More than in many other individuals I know, this inherent hybridity is observable in the languages I speak. Despite having spoken German for 25 years as my mother tongue, I am more fluent in English than I ever was in German. Now, having spoken French at home for nearly 3 decades, I am more at home in this language than in my mother's tongue. My accents are further testimony: speaking German with a North American accent, I speak French with a more Germanic accent, and in my English one can hear a European influence that is often difficult to locate and sometimes completely disappears.

Is there any importance of cooperation between cognitive science and social studies of science?

Cooperation is important and productive not only between cognitive science and social studies of science. I once read a paper on the origin of truly innovative theories. The paper provided evidence that these innovations arise when individuals or groups are competent in multiple areas of inquiry. This allows them to identify congruencies, incongruencies, and open areas covered by one, but not by the other field. I strongly believe in—and live out—an affirmative answer to this question. But rather than engaging in talk, I like to propose concrete studies that generate data that individuals and groups from multiple backgrounds attempt to make sense of. Thus, for example, I once conducted a study on learning physics with researchers who took a theoretical framework very different from my own (e.g., Duit, Roth, Komorek, & Wilbers 1998). My colleagues came to the study from a conceptual change perspective and cognitive approaches to learning by means of analogies whereas I was interested in the phenomenology of learning (through the eyes of the learner) and in discursive psychology. Even if our ways of approaching phenomena were incommensurable, I could always ask the question about the assumptions made in each approach and the conditions under which each theory/method works. For example, in conceptual change theory, one has to assume that language does not change (e.g., in the course of an interview) and that

there is in fact something like a mental structure that is the cause of what people say (e.g., Roth 2008).

With respect to phenomena that might be of interest to cognitive science and social studies of science, we may look at the different kinds of results that we get from those doing ethnographic studies of science (at work) and those studies done by Kevin Dunbar, who studies thinking, reasoning, and problem solving in complex domains such as science. The two agendas appear to me to go in different directions. But it would be interesting to see what Dunbar's traditional psychological approach would yield if we were to use it on data collected in the everyday world of science; and it would be interesting to see how social studies of science explains what happens when they study scientists or mathematicians working on predefined tasks in a psychological laboratory. This is precisely the kind of work that we have done, simultaneously studying scientists at work and inviting them to work on problems typically solved by undergraduate students in their own field (e.g., Roth,2003; Roth & Bowen, 2003).

A great interest of mine is how a variety of different sciences, including cognitive and neurocognitive sciences, might usefully collaborate with phenomenological philosophers. The work that Francisco Varela has done alone and in collaboration with Natalie Depraz stands out for me. He proposed what he called neurophenomenology, that is, the study phenomena that bring together neuroscientists and practitioners of phenomenological reduction. The former have their scientific tools, such as functional nuclear magnetic resonance imaging (fNMRI), capable of monitoring brain activity from the outside; the latter are capable providing descriptions of experiences and their phenomenalization. Varela's work has been an inspiration for my own use of first- and third-person methods in the study of learning (Roth 2012b). This led me to study certain phenomena, such as learning something unknown from the perspective of the learner and the learning paradox. Thus, for example, I can observe in a classroom video that students work for 10 lessons with something as simple as a little glow lamp and then, for the first time, see that there are two electrodes rather than a single wire (Roth 2006b). I was able to show not only that it is very common that we become aware of aspects of reality even after years not noticing them, but also that there are specific reasons that are apparent in phenomenological studies perception.

The future of mathematics lies in mathematical education

What will be the future of mathematics?

I am certainly not the right person to answer this question, as I am not a mathematician. I do have an undergraduate minor in applied mathematics and I trained as a statistician for the social sciences. However, it appears to me very clear that the future of mathematics will depend, in part, on mathematical education. At this point in time, many students are turned away from really doing mathematics—as a science, disciplined inquiry—and too much attention is given to setting hurdles (i.e., certain curriculum standards) and jumping them, (i.e., getting passing grades). A primacy is given to the comparison of where, on a linear scale the students of different countries end up in international comparisons—e.g., the *Programme for International Student Assessment* (PISA) or the *Trends in International Mathematics and Science Studies* (TIMSS). On top of the achievement rankings there tend to be countries where it is not innovation and creative thinking that are emphasized, but rote learning and submission to externally set standards. Thus, when we look at the recipients of the Fields Medal (often referred to as the "Nobel Prize of Mathematics"), the highest honor a mathematician can receive, we do not find Singapore or China (Taiwan), whereas these are the countries with the highest mean scores on the PISA 2009 mathematics subtest.[230] On the other hand, countries such as France, United States, and the Russian Federation, with a high number of Fields recipients, scored at or below the 2009 PISA means.

Another interesting fact derived from research is that there is almost no correlation between (a) the number of years students studied mathematics or how well they did in school mathematics and (b) the competence with which they use mathematics in their everyday pursuits. My suspicion is that mathematics, as any other school subject, serves to (re-) produce a hierarchical order of society allowing those students on top to access coveted spots in universities and those at the bottom have to take jobs at the assembly line or as menial laborers (e.g., Roth & McGinn, 1998). What matters in schools is not mathematical knowledge, but receiving grades to access the next stage in the career and life progress. I have often wondered whether educators could not take a different position: rather than trying to make it through this or that mathematical curriculum, which students will have forgotten after the next test, and definitely by the time they step outside school, do some really interesting inquiries irrespective of whether students get to some previously defined, standard answer. Just as chefs become better by cooking, golfers become better at their game by golfing, and just as teachers get better by teaching, (mathematical) innovators get better by innovating. If we want (some) students to become innovators in

[230] OECD breaks "Chinese" into separate jurisdictions: Shanghai-China, Hong Kong-China, Chinese Taipei, and Macao-China. On PISA 2009 mathematics, these rank 1st, 3rd, 5th, and 12th.

mathematics, we need to give them the contexts in which they can become *innovators* of mathematics rather than making everyone acquire "the basics" before doing the real thing. This was the idea underlying my call for students' participation in environmental activism as a context for learning science. Early in my work I had already realized that students will develop very different types and levels of competencies when they produce mathematical representations for the purpose of convincing others (Roth & Bowen 1994). Our eighth-grade students outperformed university graduates (BSc, MSc) on a task requiring the interpretation of data: the eighth graders used more and more complex mathematical representations than their older colleagues. Thus, why would we teach factoring polynomials, which is still part of the standard fare of school mathematics? In the world outside schools, who needs competence in factoring polynomials?

I also believe that we need new directions in mathematical education research. Although a lot of money is pumped into education research and development to improve in the international rankings, we see very little in terms of returns from the students. My hunch is that there are some fundamental problems with the constructivist epistemology that currently underlies how mathematics educators think about mathematical learning (Roth 2012a). This epistemology drives their decisions about the curriculum. To push research further, we need to look both from phenomenological and from cultural-historical activity theoretic perspectives. What does mathematics look like through the eyes of the learner? What do we need to do to promote and provoke mathematical learning given that the student, who does not yet know mathematics, cannot intentionally focus on learning it? What kind of social (curriculum, school) context do we need to set up so that students are not afraid to fail when they engage in inquiry that inherently, by the very nature of learning, into the unknown? This is why I recently proposed, based on phenomenological analyses, that we need to think about mathematical learning from the perspective of the unseen and, therefore, from the perspective of the unforeseen (Roth, 2012c).

References

Derrida, J. 1967. *De la grammatologie (Of grammatology)*. Paris, France: Les Éditions de Minuit.

Derrida, J. 1996. *Le monolinguisme de l'autre ou la prothèse de l'origine*. Paris, France: Galilée.

Duit, R., Roth, W.-M., Komorek, M., Wilbers, J. 1998. Conceptual change cum discourse analysis: Towards an integrative perspective on learning in science. *International Journal of Science Education*, 20: 1059-1073.

Milkent, M. M., Roth, W.-M. 1989. Enhancing student achievement through computer-generated homework. *Journal of Research in Science Teaching*. 26: 567-573.

Nancy, J.-L. 1993. Éloge de la mêlée (Eloge of the mêlée). *Transeuropéennes*, 1: 8-18.

Roth, W.-M. 2000. From gesture to scientific language. *Journal of Pragmatics*. 32: 1683-1714.

Roth, W.-M. 2003. *Toward an anthropology of graphing: Semiotic and activity-theoretic perspectives*. Dordrecht, The Netherlands: Kluwer Academic Publishers.

Roth, W.-M. 2004. Activity theory in education: An introduction. *Mind, Culture, and Activity*, 11: 1-8.

Roth, W.-M. 2006a. A dialectical materialist reading of the sign. *Semiotica*, 160: 141-171.

Roth, W.-M. 2006. *Learning science: A singular plural perspective*. Rotterdam, The Netherlands: Sense Publishers.

Roth, W.-M. 2007. Emotion at work: A contribution to third-generation cultural historical activity theory. *Mind, Culture and Activity*, 14: 40-63.

Roth, W.-M. 2008. The nature of scientific conceptions: A discursive psychological perspective. *Educational Research Review*, 3: 30-50.

Roth, W.-M. 2012a. Cultural-historical activity theory: Vygotsky's forgotten and suppressed legacy and its implication for mathematics education. *Mathematics Education Research Journal*, 24: 87-104.

Roth, W.-M. 2012b. *First person methods: Towards an empirical phenomenology of experience*. Rotterdam, The Netherlands: Sense Publishers.

Roth, W.-M. 2012c. Mathematical learning: the unseen and unforeseen. *For the Learning of Mathematics*, 32(3): 15-21.

Roth, W.-M., Bowen, G. M. 1994. Mathematization of experience in a grade 8 open-inquiry environment: An introduction to the representational practices of science. *Journal of Research in Science Teaching*, 31: 293-318.

Roth, W.-M., Bowen, G. M. 1999a. Complexities of graphical representations during lectures: A phenomenological approach. *Learning and Instruction*, 9: 235-255.

Roth, W.-M., Bowen, G. M. 1999b. Digitizing lizards or the topology of vision in ecological fieldwork. *Social Studies of Science*, 29: 719-764.

Roth, W.-M., Bowen, G. M. 2003. When are graphs ten thousand words worth? An expert/expert study. *Cognition and Instruction*, 21: 429-473.

Roth, W.-M., Désautels, J. 2004. Educating for citizenship: Reappraising the role of science education. *Canadian Journal for Science, Mathematics, and Technology Education*, 4: 149-168.

Roth, W.-M., McGinn, M. K. 1998. >unDELETE science education: /lives/work/voices. *Journal of Research in Science Teaching*, 35: 399-421.

Roth, W.-M., Tobin, K. 2002. *At the elbow of another: Learning to teach by coteaching*. New York: Peter Lang.

Roth, W.-M., Tobin, K., Zimmermann, A., Bryant, N., Davis, C. 2002. Lessons on/from the dihybrid cross: An activity theoretical study of learning in coteaching. *Journal of Research in Science Teaching*, 39: 253-282.