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Autobiography and the Paradox of Change: (Dis)Locating Ourselves in the Process

Abstract

As researchers, we often seek to change science education practices that have become outdated. Throughout such change processes we are faced with the dilemma of embodying relationships of power or discourse that we are trying to transform. A situated cognition framework would suggest that this conundrum is inevitable because a community of practitioners is fundamentally bound by the institution and its resources, concerns and ways of being. If we cannot step outside the institution we are trying to change how can we change something which defines who we, as agents of change, are? We reframe the issue as one of cultural production, where we locate ourselves within a cultural field, struggling to change it. This paper embodies some of the tensions involved in cultural re/production of science education. These tensions are played out both in our autobiographical accounts that are part of this article and in the relationship of the authors as graduate student and supervisor. Through a conversational hermeneutic analysis of the authors' autobiographical writings, we examine some of the salient features of bringing about change in science education.

“Science for all” with its activities of questioning, discovery and technical empowerment (Fourez, 1997) is an important part of a democratic society. Science, especially as enacted in the everyday world of a community, can be densely interconnected

with everyday experience, community history, values and economic pressures (Lee & Roth, 1999) rather than an isolated, authoritarian practice carried out by privileged experts. In this case, the doing of science is an extension of peoples' intentions and goals for a better community.

But in this article we focus on a different aspect of science. Science and science education (Lave, 1990) both have strong elements of silencing and control embedded within them. Many writers have discussed the violence, both symbolic and physical that are part of the present culture of science (Roth & McGinn, 1998). In particular, the use of language to create the decontextualized, objective voice is an important aspect of this dynamic. This censoring of language has led investigators to claim that science excludes or marginalizes women, people of working class and visible minority backgrounds (Eisenhart, 1998). Science education has a very authoritarian structure (Lave, 1990; Postman, 1992), which can be said to perform acts of symbolic violence on those who do not follow the canon (Roth & McGinn, 1998). Not only is the language and hierarchy structured to exclude these alternative voices, but if these voices are expressed, they often meet with fierce opposition.

There is a growing movement of postmodern science educators who are working to make science a more inclusive practice. Their writings contain quotes such as "we... feel compelled to contribute to a research agenda which promotes such things as inclusion and social justice" (Barton & Roth, 1999). They recognize that "canon cannot be removed from the ideologies and practices it legitimates as part of a broader process of social and moral regulation, it must be seen as an ideological and political attempt to organize and reconstruct history, teaching and learning in particular ways" (Giroux, 1994, p. 160-161). They engage in heroic struggles first to recognize and then to change practices. Angela Calabrese Barton participates in teaching science to students in a local homeless shelter, in part because "I wanted to offer... science... to a population I believe to be unfairly marginalized, misrepresented and downright demonized in schooling and society" and "I

feel a bit of an angry passion regarding the treatment of homeless families by our society” (Barton & Roth, 1999).

But she also recognizes “I have been positioned with agency from both the standpoint of the “oppressor” and the “oppressed” within a larger framework” (Barton & Roth, 1999). Indeed, Bourdieu positions intellectuals within the “dominated dominant” class (Bourdieu & Wacquant, 1992). This puts educators in a complex space within the field of those involved in cultural reproduction (alongside writers, artists, intellectuals and others). We have agency in the sphere of teaching a canon to others, can have a large influence on students’ future lives, and through our research, writing and self-promoting, we can also influence the means and ends of educational praxis. Yet we are dominated by political agendas as we are funded mainly by public institutions. We experience poverty during our lengthy training and need to frame our agency in ways acceptable to a community of peers whose views we may not share. Our experience is that of balancing between the needs of the master and our own agency.

In this article, we are concerned with the struggle of cultural re/production on two levels. First, as part of becoming research scientists, we learned to re/produce science as canonical and authentic practice. We both escaped the domination of our “primary” discourses and became more critical of science. However, we now find ourselves in a new discipline, science education, characterized by similar re/production processes. We use autobiography to account for both of these struggles making this article a reflexive account doubly historicized by our own lived experiences. Through this discussion issues of location, symbolic violence, and voice are brought out, demonstrating that autobiography is an excellent way to examine central issues in science education.

The Paradox of Change—Really?

Many science educators work toward change. Some want science education to be more “authentic” (Brown, Collins, & Duguid, 1989), some want students to perform better on international tests (NRC, 1996), some work towards a “science for all” and seek to critique

the role that science and technology play in our culture's narratives of power, domination, and exclusion (Barton & Roth, 1999; Fourez, 1997). The desire for and the enacting of change is fraught with difficulty, however. Not only are the present institutions, networks and actors strongly stabilized and supported by our society's structures, but by consciously initiating change, we are confronted with our own implicit participation in the very systems and habits that we work so hard to redefine. At first, this point seems like a conundrum because effective change could never come from within a system. But when considered from the view point of situated cognition, this conundrum unravels.

Situated cognition is a way of looking at learning and acting in the world that considers knowledge to be distributed across the landscape. This landscape includes not only the material artifacts and tools available but also the social landscape in which the agent finds themselves. This includes hierarchies of power and influence, conventional discourses, language common to the community, tools, and the discourses surrounding their use (Lave & Wenger, 1991). When we look at change within an institution from this perspective, we realize that change must be generated as a product of the people, discourses and practices of the institution. If this is so, then it is expected that it reflect both the status quo and the tensions for change within the organization. And if this is the case, then we may lay much of our anxiety about the perfect or ideal change to rest, as, like "pure science," it cannot exist outside practices of human agents (Bourdieu, 1982).

Autobiography and Understanding—Some Comments on Method

The prejudices of the individual, far more than his judgments, constitute the historical reality of his being. (Gadamer, 1975, p. 245)

Gadamer's quote does not mean that we give up the reflexive task of searching self analysis, however. Reflection on practice is an absolutely necessary part of the change process, because as much as we like to think of ourselves as "white knights," we must, by dint of being a member of a community within an institution, embody those aspects of it we

wish to challenge. Autobiography, because it is a narrative which re/presents the Self, is an effective tool in this instance. It is a tool to achieve “a fundamental understanding of one’s positioned experiences, and the significance of those experiences” (Barton & Roth, 1999) which are “critical to enacting a science for all with students, teachers or researchers” (Barton & Roth, 1999). These authors paraphrase Paulo Freire as saying that “enacted agency is coextensive with learning to perceive social political and economic contradictions in one’s experiences and in the world” (Barton & Roth, 1999). This perception, they contend, is necessary to develop the agency to effect change.

However, autobiography can turn into a form of self-congratulatory ideology, a closed horizon of representations, ideas and conceptions. To avoid being trapped in delusion, we enact a critical hermeneutic that is paired and acts in concert with phenomenology (Ricoeur, 1991). A hermeneutic phenomenology seeks to recovery of meaning by developing understanding through a critical, explanation-seeking analysis. Understanding and explanation thereby stand in a dialectical relationship. Understanding which arises from lived experience is the nonmethodical moment of the analysis. The methodical moment consists in explanation, which arises from enacting critical hermeneutics. Explanation develops understanding analytically. In this, explanation is preceded, accompanied, and concluded (thus enveloped) by understanding. In the text, understanding and lived experience are re/presented in our autobiographies and metalogues; the explanation-seeking hermeneutic moment is re/presented in the analysis of understanding and lived experience through our historically-grounded critical analysis of the understanding-seeking moment.

Our hermeneutic framework draws heavily on the reflexive sociology of Pierre Bourdieu who insists that to have a truly reflexive account:

We need to thoroughly sociologize the phenomenological analysis of doxa as an uncontested acceptance of the daily lifeworld, not simply to establish that it is not universally valid for all perceiving and acting subjects, but also to discover that, when it realizes itself in certain social positions, among the dominated in particular, it

represents the most radical form of acceptance of the world, the most absolute form of conservatism. (Bourdieu & Wacquant, 1992, p.73-74).

An analysis of the social conditions in which one finds oneself is vital. We therefore need to “discover things that are *generic*, things that are shared, banal, commonplace” (p.72). What is it about one’s job, one’s position in an institution, the purpose of that institution, the way power is represented that contribute to our experience? Through this analysis we locate ourselves within relations of power and expectation formed by the social worlds in which we find ourselves. This type of analysis can lead to a radical clarification of our motivations and actions, sometimes serving to dislocate ourselves from our taken-for-granted places.

Our article is *reflexive* (Ashmore, 1989) on several accounts. First, our autobiographies provide images of the struggles associated with becoming-in-a-science. Second, in the construction of this very article we struggled with experienced relations of power that were played out in our scientific autobiographies. Third, our approach enacts a hermeneutic phenomenology with its dialectic of understanding and critical analysis which is mirrored in our writing that uses our own voices (autobiography, metalogues) and detached theoretical analysis. Fourth, we employ metalogues—conversations in which content (semantics) is reflected in form (syntax)—which take temporally-prior achievements as their new topics.

Stuart: Chasing Science

Autobiography

I.

Some of my earliest memories are of doing naturalistic inquiry. When I was three, my father would take me down to the creek and point out tracks in the mud, different plants and flowers and we would name the wildlife we could see. What world was this that was full of things unseen, yet living their lives and leaving their footprints in the mud? Suddenly, the

forest I could see became something else—trees and bushes somehow full of invisible life, harboring rich mysterious silence, from which innumerable, unknowable sprang.

My brother and I would play completely unsupervised for hours among the trees and swamp. I loved the sensual experience—the cool softness of rotting wood as I rolled a log over, looking for salamanders, the smell of the forest air, the sensation of water and muck, the keenness of senses required to see and hear correctly. I loved catching animals—bugs, fish, frogs, snakes, squirrels, and watching them up close. I never meant to harm them although looking back I am sure I traumatized them, catching and watching was my way of getting to know them. I was fascinated by the endless possibility in a fallen log—nature swarmed, you could never exhaust her, things were always changing, there was always something going on.

At age 7, when people asked me what I wanted to be when I grew up, I would say “a herpetologist.” Then I would have to explain to them what that meant.

II.

As a seventeen and eighteen year old, the wonder of genetics, which determines the fate of all life and nearly life too, captured my imagination. I enrolled in a degree program tailored to biotechnology, applied genetics.

I thrived in the “difficult” classes where we focussed our attention on the problem solving (story telling?) aspect of molecular genetics. Experiments became word games—what was the correct procedure to use to figure out what problem? What did so and so do to show such and such?

It was a heady time, the dawn of developmental genetics. Single monstrous genes were being discovered that had the ability to determine the creation of entire sections of an animal. Genes that conferred immortality onto cells grown in test tubes were being identified. Whole sets of genes that allowed bacteria to switch from harmless intestinal hitchhikers into deadly pathogens were being uncovered by ingenious experiments. The

tracks in the mud were getting clearer and deeper. Intoxicated with this sexy world, I jumped into grad school, carrying my federal research scholarship with me.

III.

The science I met in graduate school was very different from the science that I had learned as an undergraduate student.

I was bored by the eternally slow progress of ideas at the bench. Instead of reading about the latest cool experiments and experiencing that rush of understanding, I was sitting through lab meetings and learning stuff in which I was not very interested. “It’s good for the lab,” I was told.

I learned that science is not about quietly reading a bunch of stories of elegant experiments in a library. Science is found in air tight labs where people (like me) used phenol, chloroform and formaldehyde at the bench top because it is more convenient than working in a fume hood—creating a toxic aromatic ambience. Science was about that special queasy feeling you get in your stomach when you work with radioactivity. It is not a question of *whether* or not you have contaminated yourself with it, but rather *to what extent* have you contaminated yourself with it. It is about a mindset that develops among those who make their living in a world where your tools promise to burn you, crush you, scald you, cut you, electrocute, radiate, poison or mutate you if you are not giving them your full attention all the time. I could never reconcile myself with the tremendous amount of attention required to do something like properly wash a glass plate prior to DNA sequencing, and the mundanity of the task. I was spending far too much energy on worrying about whether or not I had touched a table top with the pair of gloves that I was wearing and not nearly enough about the state of the conceptualization of the system I was working with.

I was tightly monitored by a supervisor who motivated me by challenging my ideas. I was determined to please (beat) her by proving her wrong and teaching her how things were. My soccer games got cancelled. So did my rowing excursions. When you are

running experiments, it is very difficult to plan and carry out anything else. I felt tremendous pressure not only from her, but from what I said I would do and by when.

Time became a very important factor in my life—within 14 months, two close friends and roommates died, interspersed with a number of other friends and acquaintances. In rationalizing these deaths, I became very aware that we are here for a finite amount of time, and that it ought to be cherished. Spending my life frustrated and in a noisy small smelly cage was not cherishing life. I was beginning to want to work in areas that had more direct impact on social issues. I wanted a sense of participation and human involvement in my life.

IV.

I left the science world and began volunteering as a “compost doctor” with a local activist group. I read stories of compost campaigns, of the economic and social benefits of using less resources. I learned the stories that said without inventing any new technology, we can reduce waste, reduce consumption and reduce pollution.

But I also heard stories of resistant senior municipal bureaucrats (“dinosaurs”), engineers who explicitly designed wasteful systems because the systems employed engineers (“nozzleheads”), and strong business lobbies aligned against “green” innovation. It became clear to me within a month of working on a compost education campaign that the battle we were fighting was not one about best practices, but one about best stories, and the unspoken assumptions within a culture. Our way of life was like the tracks in the mud of an unseen, unspoken web.

As I canvassed door-to-door for the Western Canada Wilderness Committee, I was shocked by the amount of active apathy that people had toward the consequences of their actions. Not only did they not know, they did not want to know. I felt as if I had been slapped every time I encountered this type of word game. I was enraged but also felt a deep sadness.

I was convinced that the problem was one of awareness—if people only knew the scientific information about their world, then they would be compelled to make the right

choices. The morality of compassion (or at least “enlightened self interest”) is obvious once one understands what is actually going on not only in our environment, but also within ourselves.

I worked hard to get to the scientific bottom of many of the forestry controversies I was involved in. I read, in the government review of a huge pulp mill proposal, how the forestry company used sophisticated modeling programs to predict that the mill’s effluent would have minimal impact on the relatively small northern river they were discharging into. The parameters of the model, however, concluded that there would be significant anaerobic degradation of many of the most toxic compounds—and this had never been observed in the real world. I read about toxicity testing on fish, where tests lasted a day or a week, and results were measured solely in terms of mortality. If half the fish did not die, within twenty-four hours, then the effluent was considered non-toxic. Is this a valid measure of toxicity? This the kind of research we want to be basing vast decisions on? What kind of knowledge are we using to make very serious commitments and plans?

I realized of course, that political pressure to “develop” the forests of northern Alberta was strong. I realized that we had an environment minister who gave “the finger” to environmentalists, and I realized that government funding for environmental science was constricted. With all these circumstances bearing, how could anyone do good science?

As I participated longer in the controversies, my sureness in science as liberator began to unravel—what was better, to use a whole lot of energy washing diapers or to compost disposable ones? Were biodegradable plastic bags really biodegradable? Things began to become very complicated—often the “good” science was just as tenuous as the “bad” science, claims became dependent on multiple factors: Who did the research?, Why?, and What was the time-line? As a result of the research the choices to make become based on values, not “objectivity.”

For example, as a forestry activist, suggesting changes to forestry practices, the huge gaps of knowledge about forests was the one factor in the debate that stood out most

strongly. I used to be very angry and indignant about the political resistance to filling in those gaps until one day, my attention was brought to the forestry act, the document laying down the guidelines for the relationship of our society with our forests. It explicitly states that the people of British Columbia regard forests as economic entities, there to provide jobs and resources to communities. Of course we would not be very interested in a forest's biodiversity if all we want to do is manage it for economic gain. Our hands were tied. The science that we do is limited by the community's perception of the forest. I could no longer think of science as our saving grace. Change must come from somewhere else.

Metologue

[Stuart:] The most important thing for me to address is how in the writing of this article we have re/enacted the power relations that characterized my experience in science. I have left science, but now I find myself caught again, struggling for the legitimacy of my ideas and my expressive means.

Take a look at your memo to the first draft of my autobiographical account (Figure 1). These are strong comments and a suggestion for action which I think has rich implications. I had already written and edited the piece to exclude what you had referred to as “flowery language,” paring the story down to what could be told of my experience without misrepresenting myself. This comment opens up a number of issues of power. The term “flowery” with its pejorative and dismissive connotations could have been substituted with “emotive” or “descriptive.” I see the adjective you chose as an expression of domination. It is also a very macho put-down—calling another man's work feminine and superfluous (Spender, 1980). It reflects the masculine, competitive nature of science (Connell, 1993), which is something we work against in creating an inclusive “science for all”(Roth & McGinn, 1998).

Date: Sat, 17 Apr 1999 00:10:16 -0700
To: Stuart Lee <shlee@uvic.ca>
From: mroth@uvic.ca (Wolff-Michael Roth)
Subject: Re: autobiog

I find that your language is a bit too flowery. For example, take a look at the following paragraph:

“It was a heady time, the dawn of developmental genetics. Single monstrous genes were being discovered that had the ability to determine the creation of entire sections of an animal. Genes that conferred immortality onto cells grown in test tubes were being identified. Whole sets of genes that allowed bacteria to switch from harmless intestinal hitchhikers into deadly pathogens were being uncovered by ingenious experiments. The tracks in the mud were getting clearer and deeper. Intoxicated with this sexy world, I jumped into grad school, carrying my federal research scholarship with me.”

Headly time, dawn of developmental genetics . . . I won't be able to sell this. Would you mind going back through your part and write it in such a way that it is passionate, yet reads a bit more like the other parts, a bit on the hermeneutic side. It is not even clear whether you experienced it as this, or whether this is your posthoc objectification.

Figure 1. Beginning of a memo written by Michael in response to Stuart's first draft of his autobiographical account.

[Michael:] While acknowledging that all readings are partial, positioned, and political, your reaction strategically places you into the position of someone who has been hurt. Latour might argue that yours is just another power move to get your autobiographical piece written in the way you wanted it. And in so doing, you leveled my own actions intended to facilitate your process of becoming a scholar.

[Stuart:] Actually, I was trying to illustrate a point I thought was emblematic of the conflict so eloquently addressed in other publications of yours (Barton & Roth, 1999; Roth & McGinn, 1998), that of a hegemonic science canon effectively silencing students' undisciplined yet valuable lifeworld experiences, and the consequences of this both in terms of people turning away from science, and the violence suffered by those who continue on in it. I thought it was a rather fine-grained, discriminating analysis. I was hoping to sound right rather than hurt!

But back to power issues. I think that your comment is a perfect example of Star's assertion that “stabilized networks seem to insist on annihilating our personal experience” (Star, 1991, p. 48). Your position as the gatekeeper to publication (which is the primary

form of symbolic capital that academics accrue) is a position of power within a stabilized network. This position gives you the authority to judge and label my writing, (itself a form of symbolic violence) and that judgement carries great effect; my voice, as a junior contributor and your student is completely dependent on your approval.

The uniqueness of my experience, the crazed overzealous ambition described in the quote you objected to was (and is) a primary level of my experience of the world. I could not honestly write that “I was interested in genetics, and having recently gained a scholarship, decided to enroll in the local graduate school.” It is not me. Thus my experience of being a first author on an autobiographical paper where I am told my means of self-expression is inappropriate (“make it read like the other parts” which were all yours) is that of confusion and being told how to write my own story, which seems silencing. I am caught between the trappings of freedom and cooperation and the experience of dependence and necessary submission that our power relationship delineates.

[Michael:] Given that autobiography is a novel and potentially contentious form of expression in science education, my position as senior scholar, co-author, and editor, puts me at the intersection of multiple responsibilities and constraints. I am doubly accountable to the editor of RISE (Cam McRobbie) for producing an issue and to the community for which RISE is the official organ (ASERA). Other responsibilities are those of a graduate student supervisor who intends to be active in scaffolding his students’ access to the community, and to becoming successful members themselves. Finally, I am responsible to make sure that projects to which I contribute are congruent with what I deem acceptable writing and scholarship.

If the community rejects and marginalizes the practices and products of an individual, her actions have no effect and even worse, may not even exist in the perceptual field of a community. I need to remain aware that autobiography is not yet part of the Kuhnian paradigm (“normal science”) in science education, and do my best to ensure that this issue does get published.

[Stuart:] Your description of your position makes me think of the plight of the dominated dominant - the class of people who are, I paraphrase Bourdieu and Wacquant (1992), dominant as possessors of the power and privileges conferred upon possession of cultural capital, in this case academic credentials, but are dominated in relation to the holders of political and economic power. As you mention above, your position as a member in the community is dependent upon that community's acceptance of your work. When you exclaim in your e mail to me that "I won't be able to sell this," it highlights the relationships of power in which you are entangled - you need to please a higher authority, a gatekeeper on whom you are dependent. Here we see the paradox and difficulty of bringing about change—even with your symbolic capital and educational training, you still need the approval of the community which you are seeking to change.

[Michael:] Of course, because belonging to a community implies subscribing to its ideals which, in the academic community means, to accept its standards of interacting, publishing, and regenerating itself. But let me move our conversation to another issue. I recently suggested that no research article can ascertain that what is described actually has occurred. There are community norms of accountability, but as a community we seldom check whether researchers actually have audit trails. Therefore, as members we review and read research articles with an implicit trust that the text is "trustworthy." At the same time, writers subject themselves to a self discipline of producing accounts that can easily be audited. This self discipline differs across disciplines. The difference in acceptance rates between the natural and social sciences is attributable to the amount of "self discipline" developed by members in producing writing that is acceptable by others. In this context, my comments were not designed to obliterate your voice, but to scaffold your development. I want you to be able to produce research articles which are acceptable for publication. My comment "flowery language" reflects my self discipline produced by a disposition which you were enabled to observe, in praxis. This, as you know, is the only place where this disposition to self-discipline reveals itself.

Our collective work of writing together plays out interesting tensions. As advisor, I want to facilitate my graduate students' entry to the field. But in so doing, they may find themselves caught: in order to be successful, their opportunities for innovating are constrained because of community norms that determine the standards of research. Thus, any innovation has to be played out in this tension between cultural re/production. Total innovation, a complete abandonment of the history of one's field, a Kuhnian revolution and paradigm shift is likely to be accepted only if the objects of study seem to require them rather than when it appears to come from the fancy of an "not-yet disciplined," that is, an "un/disciplined" graduate student.

[Stuart:] I notice your word play around "discipline." The intermingling of punitive, limiting practices within the production of a community of practice is a fascinating dynamic. Where does freedom end? Where does it begin? When is it appropriate to guide the student, even if it means they suffer or are silenced? I think these are issues all educators encounter. This conundrum becomes even more intriguing in a situation such as this when our rhetoric is ostensibly around changing science so that it is more inclusive. The sociological point of view is particularly helpful in clarifying the real conflicts surrounding our self-proclaimed emancipatory stances. Unlike the cowboy riding into town to save the day, unfettered by relationships to anyone in the town, we are empowered and constrained by our positions within the field of cultural production, and necessarily must use its understood norms of power relationship. I wonder how this tool of understanding could be of use to other science educators planning change within their classrooms?

Michael: Becoming a Research Physicist

Autobiography

Coming from considerable poverty, I was not destined to go to university, let alone get a masters degree and become a research physicist. I had repeated grade 5 because I could not calculate, among others, the amount of wall paper it would take for a room given its

dimension and those of windows and doors. Later, I continued to receive my lowest grades (C+) in mathematics, even at the college level. When I wanted to enter a teacher education program to become a physics teacher, the advisor told me that, given my mathematics background and course selections (geography minor), I would be dropping out of the program very quickly. In spite, I changed my program to that of a research physicist, but included applied mathematics as my minor.

Four years later, having passed the two bi-annual comprehensive examinations, I found myself choosing a research topic (question) from the lists provided by the five physics departments. I had made it through the physics program and now was about to start a 12 to 15 month research apprenticeship ending with a thesis in experimental physics. Because all the topics offered in the different departments related to cutting edge research, my course could not have prepared me to make an informed judgment as to the nature of any of the projects. I decided to go with atomic physics and here with research on the stopping cross section of beryllium for interactions with protons.

When I arrived in the laboratory, everything was new. I hardly could understand what the professor, lecturers, postdocs, and doctoral and masters students were saying. The only fix points seem to be a large laboratory-filling accelerator for protons and helium up to an energy of 120 kilo electron volt, a teletype minicomputer, the rotation and diffusion pumps that produced the vacuum in the apparatus.

The professor and postdoc expected from me the design of an apparatus to produce thin beryllium films, 10 to 200 atomic layers, including the device to measure their thickness. Furthermore, I was to produce computer models for simulating the passage of protons through beryllium. In the end, I was to conduct acceleration experiments on my own to measure the energy loss of protons in beryllium at various energies (speed) and varying thickness of the targets.

Initially, I was so overwhelmed that I did not even know where to start. I could not really understand what the other members of the laboratory were telling me, the devices were

unfamiliar, the referents of the various acronyms unknown, I did not know BASIC (the programming language), and had no idea about the complex mathematics involved in conducting the modeling. But, I decided to persist and began to read the theses and articles produced by the members of the laboratory, and followed up some of the references that I found in the articles I read.

Over the next few months, I began by designing the apparatus for producing the films and for determining their weights. To control for the temperature, everything had to be mounted onto a 1-cm copper plate that was cooled by copper pipes filled with running water that had to be soldered to the plate. Because my work was to become equipment for the entire research group, I had expected more help or confirmation that I actually did. I wanted to be confirmed to have done the right thing, but never received it. After I had completed my plans, I submitted them to the shop where the supervisor indicated to me that it would take at least two months until they could complete the device.

In the meantime, I did some of my computer (Monte Carlo) modeling, but soon seem to run out of activities and went away for a couple of weeks. At the end of the two months, checking up on my apparatus, trouble. It was more difficult and would take another month. This month was extended by another month during which I could not do any work on the apparatus. I went away or helped others in their thesis work.

When I finally had my thin films, I found out that the accelerator was pretty finicky. Sometimes, after 6 hours of start up work (heating the pumps, generating the vacuum at 10^{-9} of normal pressure), something did not function and we had to find the error. It might led me to shut the entire accelerator down without having done any measurement. I went on for weeks without collecting any suitable data. I began to stay long hours in the evening. Eventually I decided that once I had the apparatus running, I would stay until I had completed several measuring cycles often working 36 hours without rest. One night I was so tired that I touched part of the high voltage generator and got zapped by an 80,000 Volt burning my skin on the side of my body. Another time, I accidentally touched two open

conductors of a 220 Volt line burning holes into the back of my hand. But every time I had completed one of these cycles, I was exhilarated, having more data; at the same time, I was sad because I knew it might take me a week to get the accelerator to work again. I finally decided that I had enough data and stopped measuring.

[INSERT FIGURE 2 ABOUT HERE]

I plotted the energy loss that I calculated and found that they did not neatly fall onto a line curve. The data did not show just a small scatter (as Figure 2), but there appeared to be a large scatter even around the same energy reaching up to 15% of the mean value. I did not know what to do, and played around with the idea of plotting not just data points, but distinguishing them by the thickness of the beryllium film. After I had done this, I began to notice that the large deviations all came from the measurements with the thinnest films. This led me to plot another graph (Figure 3) which appeared to show a trend: Higher energy loss per unit thickness in thin films than in thicker films. It did not make sense to me, and I tried to wiggle my way out of the dilemma by arguing that these are measurement error and that the error bars confirmed this. When I dropped all the thin films from the analysis, a curve such as Figure 3 resulted. I presented my results to the postdoc and professor without the small films suggesting that there are problems. But the this undeniable and unexplained trend kept on bugging me. For weeks, I returned and checked all my measurements, the counter results, my calculations. But no change.

[INSERT FIGURE 3 ABOUT HERE]

One day, I had a break. I remember sitting in my office, staring across the interior yard enclosed by the laboratories. I had heard a presentation about the research on the surface of different metals. The presentation pointed out that after heating off all foreign materials from the surface, it would soon be recovered with various gases. A vague image came to my mind of aluminum that was not only covered but actually formed a chemical reaction on the

surface forming aluminum oxide. What if beryllium did the same? It was close enough to aluminum, and also pretty reactive. When I investigated, I found out that beryllium in fact did not exhibit its pure surface, but always oxidizes. Making an assumption of 2 or three layers of oxide, I then adjusted the energy losses and lo and behold, the new values fell within the range of those extracted from the thick films. I was elated. I had cracked it.

In the end, I felt good about what I had done. Despite all odds and predictions, I had completed my work including a lot of mathematical modeling. On top, my work was one of only three in 25 that ended up being published (Bruner, Hink, & Roth, 1980).

Metologue

[Stuart:] With our previous conversation in mind, I see some interesting themes emerging from your work. First and foremost in my mind is that this story is a heroic narrative. Constantly, obstacles were placed in your way, which you, through persistence, overcame. “Despite all the odds and predictions. . .” you succeeded in not only completing the work, but in getting it published, which put you above the 87th percentile of master’s students. The story is a story about successfully meeting the challenges put to you by your institution, and coming out on top. Like Sean in your recent publication, you represent yourself as “as (an) autonomous being, responsible for its own destiny... make(ing) it in the face of adversity” (Roth & McGinn, 1998, p. 409). Yet in that article, you deconstruct such heroic narratives, and call for a valuation of “contingent achievements, interactions, and worlds of people and things that have been excluded from traditional accounts in science education” (Roth & McGinn, 1998, p. 415). These marginal voices are silent in your story. There are no contingent achievements—you succeeded 100%, there are no accounts of interactions with anyone, nor is there any mention of a world outside the lab, where you spent a good deal of time “away.” I am not saying this as a criticism of your writing, but rather to remark how your story, written for the community norms, supports the very tendencies against which you write so eloquently. Again, I refer to Bourdieu, who might

suggest that in your role as a guest editor, your relation to the position of power, obliged you to construct the traditional heroic individual narrative (Bourdieu & Wacquant, 1992).

[Michael:] Your reading is particularly negative and out of context. I find it interesting that you focus on the outcome rather than the struggles associated with learning. From a first-time through perspective, learning for me is always a struggle, always a journey into the unknown. I am never sure where it will end, and yet, in a singular move, my experience becomes reduced to success.

Your comment is also interesting because it brings out one important aspect of the relationship between writing and reading in that the meaning of a text does not reside in or behind the text, but emerges for each reader as a function of her current horizon. Because you read my autobiography before I had been able to interpret it, you read it as a heroic story without knowing my underlying intent, which was to argue that *this science is not what we ought to use as a referent for science education that aspires to be "for all."* Thus, if my autobiographical text was standing by itself, I agree with you that it falls among the heroic stories, particularly given that I came out of extreme poverty, that I was on government support since my high school years, that I succeeded despite failing to understand. It could then be used to argue that everyone can make it, that even students in West Philadelphia can break out of the cycles of cultural re/production of resistance, poverty, and inequalities in which they always and already find themselves from the beginning (e.g., Tobin, Seiler, & Walls, 1999).

[Stuart:] Isn't it interesting that this paper is indeed a product of its process. You urged me to write my autobiography with a definite end in mind, as you had done. Then we passed these texts to each other without revealing our veiled points. Our impressions of each others' writing certainly showed that the points were well constructed, but the arguments in which they were embedded were completely missed. I find this an illuminating exercise on writing and interpretation.

[Michael:] You raise an important point which has also been made by Leigh Star (e.g., 1991b) or researchers who employ actor network perspectives (e.g., Redfield, 1996). But as I point out in *On Agency, In Science Education* (Barton & Roth, 1999), all texts are inherently incomplete, always leaving out another story to be told. My autobiographical text was written for a particular intent. It is therefore constructed such as to allow a particular argument to be constructed, and to have its place in this argument.

Metalogues: Around Autobiography

Metalogues are conversations that exemplify their topic both semantically and syntactically (Bateson, 1972). Furthermore, metalogues are emergent (second-order cybernetic) phenomena in the sense that they take their own previous results as new topics. The following metalogues are therefore not end points but simply a new stage in the conversation to be taken further by the readers. Here, we broach two important issues that were raised by the previous conversations: autobiography as a method of questioning trajectories of learning (enculturation) into science and science education.

On Apprenticeship and Power Relations

[Stuart:] In many of your papers, you argue against the silencing of voices that occurs as a result of the operations of the scientific educational institution (Barton & Roth, 1999; Roth & Lee, 1999; Roth & McGinn, 1998). Yet as part of the process of writing this article, I was silenced. How do you account for this contradiction?

[Michael:] It is in the nature of the beast that, as agents, we cannot both engage in doing[something] and in doing[accounting[something]]. To prevent the implied infinite recursion, we usually settle into a mode of “getting things done.” When and how this happens depends on the individuals in a relationship. I thought that we had settled on this and made my comments in the spirit of helping you to get your part done within a reasonable time frame.

[Stuart:] I think that “when and how this happens. . .” depends just as much on “the objective structure of the relations between the positions occupied by the agents who compete for the legitimate form of specific authority” (Bourdieu & Wacquant, 1992, p.105) as the individuals involved. Our dialogue could not have happened without the structuring effect of the graduate student to supervisor relationship.

This conversation has helped me to locate myself within the science hierarchy and explains much of the uneasiness I feel within the community. I notice that my autobiography is full of emotional expression and my stories are almost all relational—I situate most stories in a relationship, and often include detailed physical descriptions of my experiences. Many of my activities were done in groups or situated within a group setting. As I read of the exclusion that the blue collar class, many women and visible minorities face when they encounter the scientific culture (Eckert, 1990), I can relate my own experience to theirs. I have realized through this exchange that my roots as a poor person have indeed shaped my experiences and values. This location helps me clarify and objectify your suggestion for me to write my autobiography so that it reads like yours. I now view this suggestion as a the response of an agent in a dominated dominant position within the field of cultural production to that of a dominated, marginalized voice (Bourdieu & Wacquant, 1992). It is as if we are “particles” (Bourdieu & Wacquant, 1992) in a field of power.

[Michael:] At the same time, one needs to deal with the dangers that arise from constructing one’s autobiography in terms of a marginalized position and then taking this position as an avenue to the moral high grounds. In my own situation, talking about living repeatedly in (non-chosen) poverty is immediately heard against the background of my current life as a successful scholar. Whereas I attempt to tell an autobiography, you deliberately hear it as a heroic “otobiography” (Derrida, 1985); this conversion, your “monlingualism” (Derrida, 1998), *I* experience as symbolic violence. What we can learn, then, from our double-layered autobiographical narratives (becoming a scientist, becoming a science educator) is that there are always tensions in the cultural re/production of any field.

As science educators we are involved in the co/production of these tensions by contributing to the tensions experienced by students in science classrooms and by those who are aspiring members to the field of science education.

[Stuart:] For me the usefulness of this writing has been that it will forever affect my rhetoric around change. By analyzing your e mail, which in my experience is typical of the scientific culture, I have been able to explode my own myth about the nature of change—namely how one individual can criticize a whole system of practices and then change them with no reference to those practices. It makes me less concerned about being perfect and more concerned with becoming involved. It also brings home the point that our actions are substantially influenced by the relationships and societal structures we find ourselves embedded in.

Curricular Choices

[STAGE SETTING:] The going referent for science at school is scientists' [laboratory] science. However, over forty years of curriculum development and associated research on learning (e.g., Brown, 1994) have not led to an improvement in "scientific literacy" of people (Hazen & Trefil, 1991). Some recent work in science education seems to suggest that one of the most important questions has yet to be asked: Is the referent for the *science* to be taught appropriate for engaging *all* as the title of the AAAS (1989) report *Science for all Americans: Project 2061* appears to achieve? Thus, it has been suggested that "science for all" has to be reframed (at least in some context) so that it is appropriate for students in poverty and destitute housing projects (Barton, 1988; Tobin, Seiler, & Smith, 1999). Another suggestion is to *deinstitutionalize* science education and to make it part of the local community where students contribute their part to the overall environmental activities (Roth & McGinn, 1997).

[Michael:] Our autobiographical accounts of becoming scientists and the current malaise in science education raises the question: What is the form of science that we want as referent(s) for teaching science in schools. Few science educators ever asked whether what we offer as “correct,” “canonical,” and “authentic” science of scientists is that which is appropriate for *all*. Why should the activities environmental or AIDS activists engage in not be an appropriate referent for designing science curriculum? and What are some of the constraints using laboratory science as a referent to construct *science for all*?

[Stuart:] I think we should deepen our analysis and ask how will we discipline the children in this *science for all*? As I think about teaching in terms of cultural reproduction, and reflect on your discipline comments, I now consider an important part of the curriculum its “repressive” aspect. We need to be cautious in our rhetoric, not to promise a utopian vision that does not include the means of social reproduction which have been discussed in this article. If we cannot escape them within the constraints of an article, how do we expect to escape them when society confronts a radically different notion of science?

[Michael:] I think that we have to re/define what science in schools might be, and suggest that science education might well have as its goal to allow students from early on to participate in sociopolitical action and to learn as central part of this participation (Roth & Désautels, in preparation). In contrast to my earlier views (e.g., Roth, 1995) I do no longer think that my own experiences as a graduate student or as a research physicist are appropriate images that should guide (planned and enacted) curriculum. An exclusive curricular focus on scientists’ science constitutes a homo-hegemonic approach and may constitute indoctrination rather than education for democratic citizenship.

[Stuart:] If, as agents in the field of cultural production, we seek to change the very definition of science in schools, I think we must take into account our position in the field of power and the constraints which this will put on us. By this I mean the relationship schools have to universities, industry and governments who all have investments in a certain way of “producing” employees and students. If we propose to do away with the hegemonic ideal

of scientists' science, where and how do we apply disciplinary boundaries? How do we determine what the boundaries are? What is acceptable and what is not in terms of the compromises that our generative dispositions will inevitably place on our practice?

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Figure 2. Stopping power of beryllium for energies from 20 to 120 kilo electron volts with corrected values for the oxygen contamination on the thin films.

Figure 3. Graphed stopping power of beryllium for varying film thickness at a proton energy of 100 kilo electron volts. Open and closed circles signify self-supporting and backed films.