

# Expanding the field of science education: A conversation with Ken Tobin

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This paper focuses on a conversation with Dr. Kenneth Tobin, which took place in June 2009 at The Graduate Center of the City University of New York (CUNY), where he is Presidential Professor in the Urban Education Program. Our purpose was to focus on Ken's career in science education, and to discuss the past, present, and future of his research interests. During our conversation, we explored the various trajectories of his career, focusing on the ways in which his research has evolved through the years. Further, Ken shared his thoughts on the field of science education, and provided salient advice for early career scholars. This manuscript includes an introductory summary of Ken's career achievements to this point, a record of our conversation (the audio-recording can be downloaded at the journal's website) and a list of selected publications that highlight Ken's key works.

*Keywords:* Science Education, Teacher Education, Urban Education, Theoretical Frameworks, Research Methods

## INTRODUCTION

This paper focuses on a conversation with Dr. Kenneth Tobin (hereafter Ken), which took place in June 2009 at The Graduate Center of the City University of New York (CUNY), where he is Presidential Professor in the Urban Education Program. Our purpose was to focus on Ken's career in science education, and to discuss the past, present, and future of his research interests. During our conversation, we explored the various trajectories of his career, focusing on the ways in which his research has evolved through the years. Further, Ken shared his thoughts on the field of science education, and provided salient advice for early career scholars. This manuscript includes an introductory summary of Ken's career achievements to

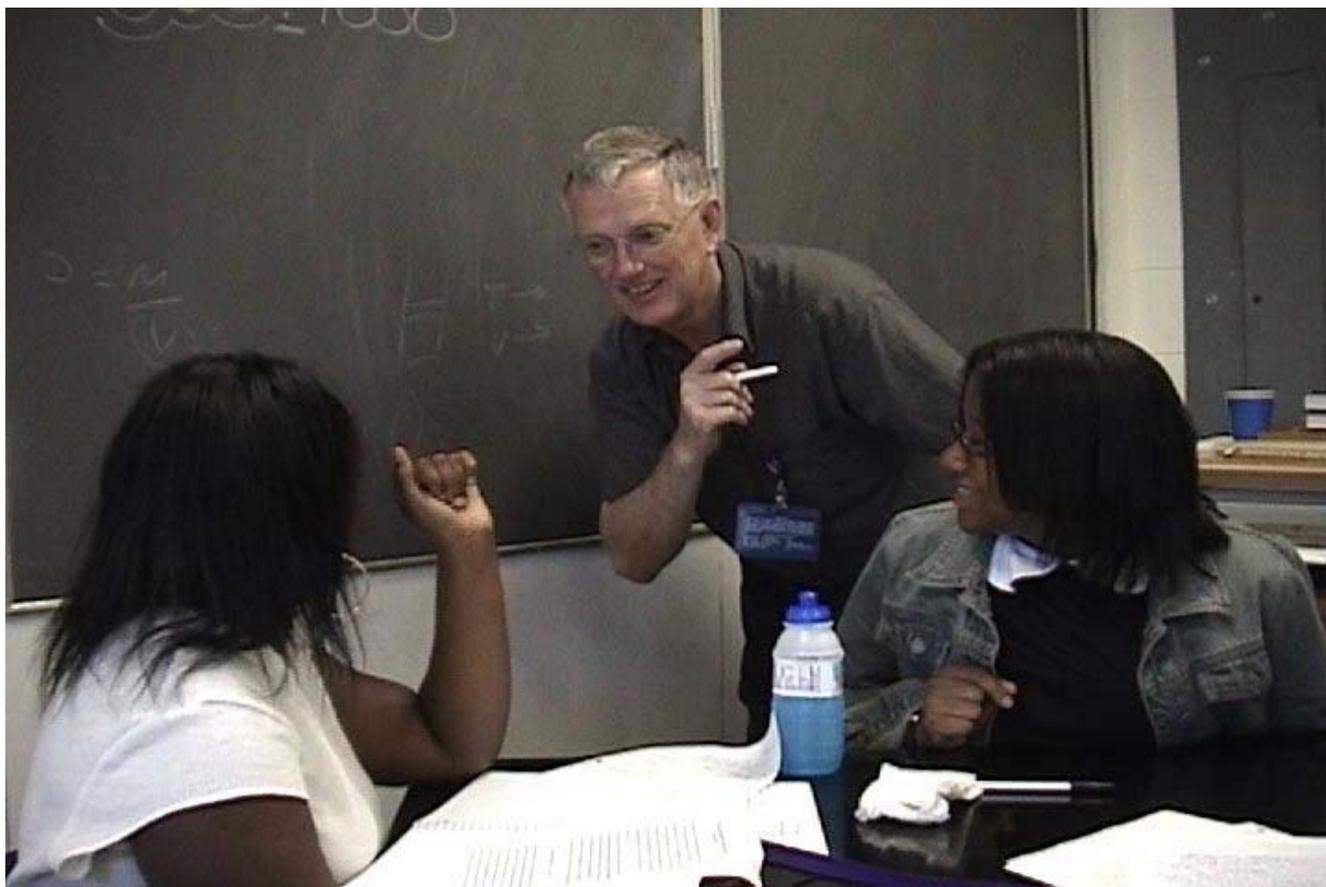
this point, a record of our conversation (the audio-recording can be downloaded at the journal's website) and a list of selected publications that highlight Ken's key works.

## FOREWORD – about Kenneth Tobin

Dr. Kenneth Tobin is a key figure in science education, and his research has had a significant impact on the field. In this section, I provide a brief biographical summary to highlight key accomplishments and to emphasize the multiplicity of his contributions. This is by no means an exhaustive summary of his career. Rather, I have chosen to highlight central components that provide evidence of the diversity of his endeavors, as well as the evolution of his research, teaching, and professional commitments.

Ken began his career as a science teacher in Australia, and earned his doctorate in Science Education at the University of Georgia in 1980. After receiving his doctorate, Ken returned to Western Australia, and was a science educator in two Australian universities (now known as Edith Cowan and Curtin) until he relocated with his family to Florida to a position as professor of science education at Florida State University. He held this position from 1989 until 1997, after which he

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**Figure 1. Ken Tobin teaching while researching in an urban high school**

transitioned to a position as professor at the University of Pennsylvania, from 1997 until 2003, where he served as Director of Teacher Education for the first three of these years. In fall 2003 Ken accepted a position as Presidential Professor in the Urban Education Program at The Graduate Center, City University of New York, a position he has now held for six years. He also has been an Honorary Adjunct Professor at Curtin University in Australia, and as such, he has supervised beginning researchers as they complete their doctoral theses.

Ken's publications number more than 700, and selected works are included in the appendix of this paper. He has, to date, 330 refereed publications, as well as 379 non-refereed publications. Among his books are two that have received the American Library Association (ALA) Choice Award for Outstanding Academic Titles. This prestigious award was granted in 2002 for the book *At the elbow of another: Learning to teach through coteaching*, which he coauthored with Wolff-Michael Roth, and in 2006, the book *Improving urban science education: New roles for teachers, students and researchers*, edited with Rowhea Elmesky and Gale Seiler received the Choice Award as well.

He is the founding editor of five book series, including the founding co-editor, with Joe Kincheloe, of *Bold Visions in Educational Research*, which is published in the Netherlands by Sense Publishers. At

present Ken edits two book series with Springer and two with Sense. Also, he is Editor-in-Chief of a journal he co-founded—*Cultural Studies of Science Education*.

Throughout his career, Ken has been actively involved in professional organizations, including the American Association for the Advancement of Science (AAAS), American Educational Research Association (AERA), National Association for Research in Science Teaching (NARST), National Science Teachers Association (NSTA), and the Association for Science Teacher Educators (ASTE). His service to these organizations includes being past President of NARST and having served, and continuing to serve, on various committees.

Ken has been awarded numerous awards to recognize his scholarship over the years, including multiple best paper awards from organizations including AERA and NARST. Of particular note is his 1987 article published with Jim Gallagher in the *Journal of Research in Science Teaching (JRST)*, *The role of target students in the science classroom* which was recently selected as one of the 13 most influential articles published in the *Journal of Research in Science Teaching*.

His professional contributions include having been appointed to eleven journal editorial boards, of which he now serves on five, including *International Journal of*

Science & Mathematics Education, Educational Researcher, Journal of Teacher Education, Research in Science Education, and Research in Science & Technological Education. Further professional distinctions and contributions include participating as a Fellow of American Association for the Advancement of Science (FAAAS) from 1999 until today, and multiple distinguished appointments internationally including most recently as visiting professor in the Departament de Didàctica de la Matemàtica i de les Ciències Experimentals, Universitat Autònoma de Barcelona, in Spain, where he collaborated with Mariona Espinet on sociocultural theory in science education and science teacher education. His substantial support of graduate students includes having successfully guided 42 students through their dissertations, myself included, and having many more students in the process of completion at The Graduate Center, CUNY, as well as Curtin University. His seemingly never-ending guidance to early-, as well as mid-, career scholars has earned him two Mentoring Awards, one in 2007 from ASTE, and one in 2008 from Division G: Social Contexts in Education Research, American Educational Research Association.

Ken has received numerous grants through the years focusing on improving teaching and learning, including his most recent NSF grant received while at The Graduate Center, titled “Use of Research to Improve the Quality of Science Education in Urban High Schools.” The purpose of this grant was to expand classroom research in science and mathematics classes in New York City, especially those in which teachers and urban youth undertook research on the teaching and learning that occurred in their own classrooms. Ken has several current research projects underway, which are in part the focus of the conversation that serves as the focal point of this paper, and is introduced in the next section.

## **INTRODUCTION – about the interview**

Kenneth Tobin and I both attended the 2009 National Association for Research in Science Teaching conference in Garden Grove, California. At this conference, Mehmet Fatih Tasar, the associate editor of the *Eurasia Journal of Mathematics, Science & Technology Education* asked Ken to consider being interviewed for the Conversation / Interview series of the journal. Shortly thereafter, Ken asked me to engage in this conversation with him, and I readily accepted. I found the invitation to interview him exciting, as this would provide a relevant way to engage readers, especially early-career researchers, into considering the variety of possibilities and directions that can emerge through a career that spans several decades. Further, the interview would provide me a chance to highlight one

of the things that has always struck me about him, his continually evolving theoretical and methodological perspectives. Ken facilitates several research squads at The Graduate Center, in which I have been fortunate to participate through the last several years. Through collaboration and a vertical alignment structure, he manages to create a forum for researchers to support, and learn from, each other. It is through these squads that I have learned to embrace the importance of seeking polysemic perspectives on science teaching and learning, and I expected that an opportunity to interview Ken would provide further examples of the development of this focus in his work with beginning researchers, and that this would in turn be extremely relevant for others in the field to consider in relation to their own work.

During our conversation, the focus for discussion included several strands of his trajectory as a researcher. The following points served to organize our conversation, and provide a biographical narrative of Ken’s career and accomplishments to today:

- His early career in Australia and the use of quantitative approaches in research,
- The beginnings of ethnography in his work in science education, as well as his focus on radical constructivism,
- The sociocultural turn in his research and focus on collaborative classroom research.

In considering the overarching theme of Ken’s contributions to science education, we discussed, among other things, the role of research squads and vertical alignment and the importance of polysemicity and polyvocality in teaching, researching, and writing. We spoke about how his perspectives have changed over time and with experience, as well as how those perspectives have changed the field of science education. Our conversation ended with his advice for early career scholars, science educators and students. The section that follows is a record of our conversation. We have chosen to retain the format of an interview, so that a reader can have a sense of the ways in which our conversation unfolded.

## **THE CONVERSATION**

### **Beginning a Career in Science Education**

Chris: You began your career in Australia in the late seventies, and your research perspectives have changed significantly since then. Can you speak a bit about how you began your career, and what was the field like?

Ken: At the time, I was particularly taken with the research that was going on in inquiry and problem solving, and we were coming through the era of big curriculum development in the United States.

Australians were coming to the US to do their Ph.D.s, and I heard about this research on wait time and I just thought it was fantastic. It was a series of studies that were looking at the way that classrooms change as a result of teachers pausing for longer periods of time and I figured that if that was the case, probably achievement was going up as well. So my initial studies looked at the relationship between teachers extending their wait time and students achieving at a higher level.

Chris: So when you think about that, and your move to the States then, one of the things that you have mentioned to me that I think is really interesting is how from there you then went into ethnographic work. Which, from my understanding of what was happening at that time, was new in educational research.

Ken: Right. I came to the states to do my doctoral degree around 1978 and came into a bastion of positivist thinking at the University of Georgia. They were neo-behaviorists, and I was more or less required to be that way. My interest has always been in teaching and learning and how to better teach science, so I was focused not just on learning, but also on teaching. In a way, that was a bit unusual in science education at the time. I think the focus had largely been on learning, and probably because of this emphasis on curriculum and "teacher-proofing" the curriculum, I think they were more inclined to have the teachers do scripts and then to produce learning through curriculum work. So the behaviorist way of thinking about it fueled into that a bit. I did some work on teacher assessment at that time which was another way to hold teachers accountable and control what teachers were doing.

The thing that was really of interest to me was how we can work with teacher models of sorts and have teachers teach more in an inquiry mode, and it really struck me that it was hard to get from the model to the teaching. We would use strategy analysis and video analysis and things like that, but there was always this gap between what was in the teacher's head and what the teacher would do. So that was issue number one. Issue number two was that in order to do the kind of research that we needed to do, which was quasi-experimental, you needed to control a lot of things. I spent a lot of time at the University of Georgia designing lessons plans; sequences of lessons that teachers would be able to use to produce process skill learning, concept learning, inquiry, things like that. At one point, I was giving a talk out at the University of Texas and there was this researcher called Walter Doyle in the audience, and he asked me a question that was like this: he said, "Ken, this is all well and good. I understand how wait time works. What I'm really interested in though is what the teachers do when they are not teaching your lessons." I thought that was a fairly interesting question as well, and that started the process.

On the trip back to Australia from Texas, I was sitting with a fellow called Rob Baker, an ethnographer that had worked with Harry Wolcott, and we argued a lot about what ethnography could look like. By the time we got back to Australia, I had decided that I would run an ethnography back in Australia on the teaching and learning of science that basically would be called "What's happening in High School Science?" So it was very similar to Doyle's query of me. Jim Gallagher came from Michigan State to work with me on this study in Perth, and we worked in an inner-city school (to the extent that they have those) in Perth. It was a school that was really starting to go "downhill" in the sense that the kids were from conditions of relative poverty and unsettled homes, and we started to get a glimpse of what we were going to see later in urban education. That was our first ethnography, and it was a transition from doing quasi-experimental designs to doing ethnography. We had way too many teachers involved and things like that, but it was intensive and we did learn a lot.

Chris: How do you think that was received initially in the field? That was very innovative, wasn't it, at the time?

Ken: There had already been ethnographies published in science education. As a matter of fact, the first ethnography that had been done was done by Barbara Spector, but that didn't show up in my literature reviews. Also, other people like Tom Russell from Canada were doing ethnography, but that didn't show up either. When Jim Gallagher and I submitted our work for publication, Russell Yeany was the editor of the *Journal of Research in Science Teaching*, fortunately I suppose, and he accepted the work, which ended up winning an award (it was our research on target students). So, oddly enough, it was well received in the literature, although at the time we didn't expect it would be. At NARST it was less well received; it tended to divide the community into qualitative and quantitative. Gallagher and I did some things at NARST to spread the growth of the community and I suppose that drew attention to what we were doing. This was also tied up with another move that I was associated with, and that was radical constructivism.

The move to ethnography was really grounded in dissatisfaction with answering important questions in science education and the inadequacy of behaviorism and positivism as frameworks for thinking of that. So, there were two "against the tide" trends that I got associated with, one being the move away from positivism toward constructivism, and the other was what people would describe as a move towards qualitative research. However, we saw it as interpretive and the award winning paper was both qualitative and quantitative, so I never saw myself as moving towards

qualitative as much as moving toward interpretive, which is a theoretical shift.

I think ethnography, when I initially came to it, was a way to look at big questions without this reductionism of variables and things like that. As we went along, issues of methodology (theory of method) became more important. So, we began to question whether the theory of our method was consistent with the theory being used to make sense of teaching and learning, for example, and usually the answer to that was no. So, that was one driver for change. Initially we started to use the model for ethnography that was informed by Guba and Lincoln, because they adopted a radical constructivist view of life. Ontologically, they were dealing with a situation where there was no reality. Virtually, that's what they were saying. So, from a methodological point of view, that had real implications for this idea of triangulation, for example; that you could get different data sources, and the reason to do that was to converge; to triangulate on "social truths" and then the contradictions were errors that had to be understood. With Guba and Lincoln, they had the idea that the contradictions had to be resolved through negotiation and consensus building, which is the old Piagetian model. Once we made that move toward using the authenticity criteria it becomes a slippery slope and we started to then really challenge this idea of, how do you deal with difference? Within my research group we started to look at retaining the differences rather than focusing on the convergences or the patterns of coherence. Then, people like William Sewell and his view of culture became a primary part of our methodology, where instead of looking for thick coherence and explaining away contradictions, we were looking for patterns that had thin coherence and always searching for contradictions. What that does is give you a different set of outcomes from research. We also realized that the outcomes from doing research couldn't just be theoretical. There were always two parts, one was the production of change and improvement, and the other was the production of theory.

### **Evolving theoretical perspectives**

Chris: So then from there, from radical constructivism and your emerging theories, how then did the turn towards sociocultural research in your work right now in urban classrooms come about? What are some ways that you shifted from radical constructivism in that direction?

Ken: I think that radical constructivism was associated very much with Ernst von Glasersfeld, who was an intellectual tower of thinking. He did wonderful theoretical work that was post-Piagetian. What was radical about radical constructivism was that the knowledge didn't exist anywhere outside of a cognizing

being, so it was embodied. Now this necessarily brings the focus onto the individual. So, very early on one of the criticisms of Ernst's work was that, just as it was with Piaget, the focus was on the individual learner and didn't say much about the social processes. I was close with Ernst and we would talk about this, and his response was always a bit glib. He would say, "it's constructed, the social is constructed, it's all the mind of the learner" which is solipsistic; it's all inside, nothing is outside. So there are various ways to get around that as an issue. Von Glasersfeld would always say that Piaget was never individualistic; he wrote a whole book on the social. Of course Piaget probably wrote 20 to 30 books, and one was on the social. But Ernst would point out, how many people wrote a book on the social aspects of learning? Piaget wrote one, so you can't say he ignored it. And yet, for me, classrooms are radically social. Just as we couldn't claim independence of learners in statistical research, we couldn't say one learner is independent of another because any teacher knows that's not the case. You couldn't do that either in ethnography because we were looking at the way that individuals work together to produce learning, and yet our theories of learning were not social.

So I started to look at social constructivism, and the work I did with Deborah Tippins was about social constructivism. We got very interested in metaphors and the ways they were used to organize knowledge within individuals, and the way they could be taught. We did several studies on the metaphors teachers used to construct teaching and the way you could change teaching by teaching them new metaphors, or they could change their teaching by constructing new metaphors. Power started to be an issue. It's always an issue, but some of the work I did with Sarah Ulerick and her struggles to get on top of middle school teaching down in Florida taught us that what we needed to do was to think a lot about power relationships and how they might be embodied or structured by metaphors. That led us to different researchers, and Jacques Desautels, a researcher in Quebec, reminded me of the work of Pierre Bourdieu. In particular, he reminded me of the work of Bourdieu and symbolic violence. We were thinking of symbolic violence in relation to why kids would become emotionally down in a classroom. We were starting to look at science as a form of symbolic violence because repeated failure, even though it was not intended that they would fail, students would receive this failure, and give meaning to the failure in terms of unintended violence. That really got me into the work of Bourdieu, and I became an eclectic social scientist of sorts. It really bothered me that I would find out what some people were doing, but I didn't know systematically about the field of sociology or cultural anthropology. At Florida I was sending my doctoral students to take courses in the sociology PhD program,

and I realized that the sociology of knowledge was an important place to be and that they were learning about people like Erving Goffman. So my doctoral students were starting to get a good background in sociology and in cultural anthropology, and at that stage I got to know a new researcher, Wolff-Michael Roth, who had just come into the field. He struck me as incredibly bright and well read in the realm of Piaget and within philosophy. So Michael's trajectory and my trajectory sort of came together, and in a way we worked together to move into sociocultural models.

Chris: So when you went from Florida State to the University of Pennsylvania, is that when your focus on urban teaching and learning really solidified when you were in Philadelphia?

Ken: Not really. It started a little bit before that because the state of Florida has Miami in it. It's a long way from Tallahassee where I was, but I got a big grant to work with teachers in Miami and it was part of a feeling that I needed to do more than just mainstream work that led me to do this. I was working with Alejandro Gallard who we had hired, one of Gallagher's students. As a Hispanic male, Gallard was constantly speaking to me about the necessity for people like me and Jay Lemke to do work with minorities. Otherwise, he was afraid, the minorities were not getting any attention. So it was kind of constant nagging from Alejandro that made this important in my mind. We had one of the biggest metropolitan areas in the United States in Miami. Certainly, with all of the immigration coming in, it was going to be a big issue. So we created a huge cohort of teacher researchers who were looking at what was going on in their elementary and middle school classes and they did a whole degree with us.

At the University of Pennsylvania there were two key thrusts in education—urban and international. I was interested in both and it took a year or so before I really focused on urban education in the US. Since about 1998 my key focus has been on urban schools, especially in Philadelphia and New York.

### Sustaining research squads

Chris: That's interesting. So, one of the things that you were just talking about was that when you were in Miami you worked with groups of teachers conducting action research in their own classrooms. Is that where you developed the focus, on maintaining research squads and having that be a part of the work that you do?

Ken: No, it actually began long before that. I think it really began through my work in physics. When I was studying in physics, the tradition is to have research squads, and I was becoming affiliated with a mass spectrometry group that was working on astrophysics. So I understood the idea that you did a certain amount

of course work toward a masters and then the coursework stopped and you attached yourself to a research team. You continued to get credit for courses, but what you did was to actually turn up for work, and do research that ultimately got submitted for publication. That was the way that graduate education was organized in Australia, where coursework was seen as rather a lesser kind of an entity and in the sciences (especially in physics). Groups, or squads, were pretty much the way things were organized.

When I did the ethnography with Gallagher, it was a group effort that wasn't only Gallagher and myself. Pam Garnett worked on that particular study, and before we finished that study, Barry Fraser and I organized a very large study that involved a group of 12 or more researchers. It was an ethnography that looked at exemplary teaching of mathematics and science. That was the first book I wrote too; a little homegrown book out of Curtin University that consisted of these ethnographic pieces, and it was the embryonic structure for squad work. We would meet regularly but not often, and talk about what we were doing, why we were doing it, what we were learning, and Gallagher and I met every day. Then, before I came to the University of Georgia on a Fulbright award, we started a study down in Perth that led to a book "Windows into Science Classrooms" that also had a research team. Jane Butler Kahle was involved in that, and Robin White, who now is a principal of a high school "down under." That was a big team study; I think there were seven researchers involved, and we studied two teachers and wrote the book *Windows into Science Classrooms*. At the same time that I went to Georgia in 1984, we set up a study with a squad that had all people that were not senior, like Hsiao-Lin Tuan and Chao-Ti Hsiung from Taiwan, Mariona Espinet from Spain, Antonio Bettencourt from Portugal, Elisabeth Swanson (Montana State University) and Linda Cronin Jones (University of Florida). So by the time I came to Florida State I had already done maybe three or four studies and we were starting to learn not to do research on teaching. What taught me that was *Windows into Science Classrooms*, where one of the teachers that we did research on - so many researchers, so few teachers - I think was harmed by the research. In a lot of ways I don't think that study was quite fair, even though we did the study as best we knew how to do it at the time.

Our research evolved on an ethical plane. I think we looked at power relationships, so that by the time we got to the Sarah Ulerick study, which was at Florida State University, it was a team approach again, and that study was very much framed by Sarah. "Why can't Sarah do what she wants to do" was the research focus, a very broad focus, and we met several times a week, if not every day. It was an emotional roller coaster because she was very upset at what she was having to go through.

That was when we were learning that good teaching didn't necessarily transfer in any ontological sense from one field to another; it needed to be reproduced every single time.

Chris: You know Ken, when I think about your leadership in forming and maintaining research squads, I think about how being able to participate in the NY squad has been so important for me and my own development as a researcher. You've talked already about some of the perspectives on ethical issues that have evolved through participating in squads, are there other ways that your involvement in these many squads through the years has shifted your trajectory, or has helped your growth as a researcher.

Ken: That's really a good question. The idea of being a mentor is something that basically I reject. It might sound a bit heretical, but I think that if you are alive; you are learning. It's an epistemological issue - what's going to count as learning? Every single activity we get involved in is an opportunity to learn, both agentially and passively, by being with the other. So, your good questions today are helping me to learn, even though I may not have come to this conversation as a learner necessarily, but I like to think that I come to every conversation as a learner.

I think I have learned an enormous amount in my squads even though I don't necessarily come with the intention of learning something, and I might not even enjoy the process of learning something new, because often times learning something new necessitates changes in direction when you were perfectly happy with the way you were going. A couple of quick examples; I think that I learned a whole lot about emotions in working with Sarah Ulerick. Sarah was so emotional, and she was such a creative person that you could see when she was thinking - she would turn her eyes up to the left and you could see she was actually taking time out to think. She taught me a lot about (a) emotion, and (b) using metaphors, and she was a great conceptualizer in lots of ways. I think working very closely with doctoral students, I'm thinking right now at the University of Pennsylvania, was fascinating. Gale Seiler was always taking the view of the underdog<sup>1</sup>, she always saw herself more closely attuned with the agendas of the urban youth than the agenda of me as the struggling teacher who was trying to make it work. I resented it at times, with her coteaching with me I never felt that she had my back<sup>2</sup>, and I felt continuously that she may have had the back of the students. Sometimes I felt, oh gosh, it's like a conspiracy in here. That was just the way I was framing it, and years later, I can look back

1 a euphemism for disadvantaged, usually oppressed by mainstream culture

2 an expression that means the person would support me if I encountered difficulties

and learn a lot from that experience. Looking at the tapes that Gale was responsible for producing, it is clear that she had really good insights.

Also, the Pennsylvania group, because they were taking coursework with some really powerful people, opened up my eyes to the whole realm of cultural sociology. I found my own way to Diana Crane at the University of Pennsylvania, and sent a lot of the graduate students to work with her, because I wanted to rigorously become a sociologist by working with her. She then started to send students to me because she knew I was at the Graduate School of Education doing research in schools, and so I got to work with some very good people like Regina Smardon, who was doing a PhD in sociology. Regina came to work on my research squad and she had been working with Randall Collins, and that enabled me to learn about Collins' work. I came across William Sewell's work through Aiden Downey who was a doctoral student of mine at the time, and he recommended that I should read William Sewell's 1992 paper on agency and structure. It was a paper that Diana had assigned and I had not yet read it--Aiden said "I think you should read this." It was impossible to read all of the assigned readings, and we saw things in that paper that Diana never saw and as you know, it became very important to our work in science education. I think Randall Collins' work opened up the whole area of the sociology of emotions, and having sociologists working with us like Stacey Olitsky, another person who was both a sociologist and an educator, provided incredibly important insights. When Rowhea Elmesky came to work with us, I saw a different side of being Muslim, and I saw the salience of being Muslim, and female, and young. So these things became important social categories. I don't think I would have learned so much if I hadn't been working with those people in squads.

### Incorporating multiple perspectives

Chris: So you made mention of coteaching with Gale as part of your research. I think that is something we haven't talked about yet, your teaching and research trajectory towards coteaching and cogenerative dialogue. Maybe you want to talk about that for a few minutes? I think is important to think of the polysemicity that we've been touching on.

Ken: I think doing research *on* became ethically problematic in the way that I talked about. So, what is the alternative? Doing research *with*. And so doing research with is one avenue, and auto-ethnography is the other. I became very convinced that doing research with was not going to be nearly as powerful as if I was also teaching, and so setting up my teaching in Philadelphia was an important thing to do. At exactly the same time that I was negotiating to teach a class at

City High, we were developing coteaching as a model for learning to teach in urban high schools. It was a survival model as much as anything because there was too much going on for one teacher to be successful. So we developed coteaching; Michael Roth was working with me because he was simultaneously developing coteaching in Canada and he and I were working it out theoretically and practically.

When I negotiated my entry into the school, I did so with the suggestion that we should coteach. Legally that was an important thing too, because it allowed me to teach without having Pennsylvania certification. And so, what I did when I went in was coteaching; the focus though was on my teaching and it wasn't until I became quite unsuccessful that it became clear that I was no different from anybody else. If I was going to survive I was going to need coteachers so that we could all learn how to do this together. So when Roth and I wrote our book, *Teaching at the Elbow of Another*, one of the chapters we had in there was coteaching as research methodology, and another was coteaching as an evaluation methodology. We wanted to apply coteaching to both these very important activities that happen in science education. That allowed me to focus more on my own teaching while having other teachers (a) to learn from and (b) to support me as we floundered around. Then it meant that when we sat down to talk about what we learned, it was not just my voice but there was also a coteacher's voice, there were student voices, there was Roth's voice, and these voices differed from one another, thereby requiring us to figure out, what the heck are we doing? Are we doing research that converges on a truth, or on a pattern of coherence like Geertz would have us think about it, that is a thick coherence, or are we doing something different? I think that is where Sewell's theoretical framework really was the answer to a question that we had already. I had been saying for a long time that what they called residual, or error, variance in statistical research was where we needed to be focusing and figuring out what's going on in there. And to us we just needed a term for it and a way of organizing it. That is, the contradictions. These contradictions are the different robust perspectives that later on we would come to think about in terms of polysemia, meaning the different perspectives that people had, not as sources of error, or that some are right and some are wrong. These are just different ways of expressing life through different theoretical frameworks often. We wanted to argue that these differences reflected distinctive life trajectories and placements in social space.

Chris: So that brings me around then to thinking about the journal that you are the editor of, *Cultural Studies of Science Education (CSSE)*, and how it is structured to provide a way to engage the authors, the reviewers, as well as the readers, in an open

conversation around what is being written, and also provides a forum for other scholars to respond to the work that you are publishing. I think of that and I wonder how an innovation in a journal like that has changed the conversation in the field, what sorts of feedback you've gotten from people, and also what brought you to that point.

Ken: I think an increasing frustration was with where the journals were going. There are roughly seven journals in science education, not counting CSSE, and pretty much all of them were doing the same thing, and most scholars in the field knew the ranking list, the pecking order. Although it changes from time to time, basically the pecking order was the *Journal of Research in Science Teaching (JRST)*, *Science Education*, the *International Journal of Science Education*, and *Research in Science Education*. They were the top four, and then there were others that some people knew about and others didn't. But the basic proposition was that you sent your paper to JRST and it got rejected. You took account of what the reviewers said, tickled up the paper and sent it either to JRST again, or you sent it to *Science Education*, where it got rejected again. Then, you worked it over a bit more until ultimately it got accepted somewhere. Some of our papers, because they were sociocultural, rather than slam-dunk positivism, were rejected numerous times. Some of the papers that got published in JRST went the whole cycle and ended up back at JRST and then got accepted. Now, this was very frustrating, and usually, it reflected differences in ontological and epistemological stance taking, difference in methodology, which was theoretically grounded of course, different in using sociocultural theory to conceptualize research on learning, for example, versus psychological reasoning. So, it was very safe for scholars like my good friend David Treagust to publish in any and all of the seven journals because basically he was willing to use mainstream methods and arguments and to work within educational psychology. For us (in the us I include me and my students and my colleagues like Wolff-Michael) it was sometimes unpleasant to get the reviewers' comments, because they didn't just say no, they were sometimes socially violent in what they wrote.

We were looking for a different way of treating colleagues. We felt that blind-review was a flaw; it was associated with positivism and the necessity to have some sort of ontological, authentic, one-truth world. There was one way of looking at the world and peer review was the way to maintain this way of thinking about knowledge. It was the old Newtonian way of thinking about the academy. We rejected that; we thought a more honest and ethically sound system was to have non-blind review, and have at least part of the peer review process public. We were struck by Bakhtin's ideas of dialogue and conversations. We decided we would have a peer review system that was more open,

and have a forum associated with papers that were published in the journal, and the forum would always be published in conjunction with the papers. Now, how has it worked? I think the jury is out. I would say that it has been a struggle. Initially it wasn't a struggle, but it has become more of a struggle because people learn how to appropriate opportunity for their own purposes. Sometimes these purposes are hegemonic. Dealing with hegemony of superstars has been an issue where some people feel a need to publish every thought they've ever had and get it out there and to reiterate that thought over and over. This has really forced some changes, especially in the Forum where I have found that rights of reply have often been abused. Instead of opening up the conversation, rights of reply have been used to reiterate a stance, to try to convince others of your point of view

Chris: Oh, by the original author?

Ken: By the original author. I think it has been an opportunity to indoctrinate people to a point of view. That has been something of concern to me. I've been concerned too in trying to create a quick turn-around review and that's hard to do. It's easy when just two people do the reviewing, but old habits, habitus, are hard to change. So a lot of the recent additions to our editorial board are well-schooled in the old way of thinking about it. Sitting on a manuscript for three months is no problem for some people, whereas we've been trying to have two days of turn around. We've wanted the turn around to be part of this dialogue, rather than review and evaluation. So, the decision for the editors or in this case, now, since I'm the sole editor, the editor, to make the accept / reject decision, has been slow. I want the reviewers to see themselves as more in a conversation and yet they still see themselves as more of a judge and jury kind of situation. I think the long-term health of the journal and the sociocultural movement is going to be seen in the extent to which it is successful in addressing some of the really big macro structures that I've been writing about just recently. I think it is going to be important in the next two to three years to see whether we can go from a community of about 200 to a community of about 1500, and probably that is going to be the critical mass that will decide whether we continue or don't. Largely, and I hate to put it this way, but the tussle between ed psych and sociocultural views of knowledge, is going to be very important, because it is associated with a reductionist view. Educational psychology doesn't have to be this way, but it tends to be quite reductionist and is getting smaller and smaller, right down to the neuronal level, and also in doing work in terms of variables and models, this is reductionist too. So there is a reductionist way of looking at learning within individuals, and a reductionist way of doing statistics, and I think both of those are counter to the kinds of trends that we are trying to

understand looking at social organizations, and looking at learning as cultural production. We'll see how it works; as always, those who want to be bricoleurs and have a bricolage are a little bit susceptible to the power of those that think there is only one way, and a right way, because you get marginalized, and sometimes we marginalize ourselves.

### **Moving forward**

Chris: I think the one thing we really haven't spoken about also, in terms of your trajectory, is your work in New York. What do you see as the next steps of your work now? What are you working on, or thinking about, right now?

Ken: In New York, the exciting thing that happened was to come to an urban education program with a whole lot of teachers. It's like passing on to heaven or something like that. So, our squads became large in number, and we had teacher researchers who have gone on to get jobs in New York, and hopefully they will have their own squads. That part hasn't worked as well as I had hoped, but it is starting to work now, where we have individuals throughout the university system in NY each with their own research squads. Probably it's worked best at NYU where Cath Milne has her own squad and is getting her own research grants. She's working from a sociocultural model that in some ways has appropriated tools from educational psychology and statistical analysis, and Sue Kirch has also gone there too, so we have those two individuals who have started up squads. Chris Emdin, up at Teacher's College, is continuing to do his thing focusing on hip-hop and sociocultural theory and I think there are others up at Teacher's College who are starting to produce squads as well.

In the CUNY campuses there is a large group of researchers now, and we have the potential to do that. For me, as just one of those satellites now in the larger scheme of things, what I would like to do is to take on a large-scale dissemination project working with the New York City Department of Education to try to look at the transformative potential of the work that we have done on cogenerative dialogue, paying attention to the things that we know about emotions and the production of identity that is science-related, but to cast aside the neoliberal agenda. So if I can take on the neoliberal agenda, and have a system that allows students to learn science that is not so much focused on producing new forms of workers, I would much rather see the science that is focused on sustainability issues, such as the sustainability of human life in a dignified form, the sustainability of the planet, and things like that. If we could have new forms of curriculum, new forms of engagement that focused on the collective rather than competition and the promotion of individuals, then it

would be a happy day for me in NYC. That is my next agenda; I have been reaching out to the Department of Education in the hope that we can get some grant money to do some of this, and I would hope that the new generation coming into the Graduate Center would be interested in participating in some of this.

Chris: Thinking about science education as a field, what do you think are the biggest challenges right now, and along those lines, what are your hopes for the future of science education?

Ken: I think the field has been characterized by this monosemic way of looking, that there is a right way to do things, and that those who don't do it right get othered and marginalized. What that gets translated to is that we end up with these little groups that tend not to communicate with one another and learn from one another. So we have this group right now that is, what I would say very much informed by Comtean positivism. A very neo-liberal type of group, and what they do essentially is fall into line and do whatever they think it is going to take to get the money to do the research. I think there is all of that, and by and large I don't respect that work much, and I have to kick myself to stay in tune with that. I think the conceptual change group has been much the same. They have dominated teaching and learning to such a degree that it is very difficult for others' views of what learning might be to get a toehold. So, I think just to cut to the chase, on the road ahead it is going to be very important that we learn to respect one another's difference and resolve to learn from differences rather than marginalizing those that are different. That would be the challenge. Will that happen, in the future, I'm not too sure. I think that by looking at the larger picture the sociocultural ways of making sense may become more prevalent in education. Within science education we have the issue of scientists and the powerful voice of science, and I think that is a challenge because educators are not respected by many scientists. I think this makes a big difference in the way that resources are allocated to do professional development and research. This complicates the road ahead, so it is going to be political Chris.

Chris: So then as a last point of question, you've talked a lot about how you have seen the field of science education changing, and your own trajectory, what would some advice be that you have for other people, for other science educators, early career scholars, students?

Ken: Not to waver. I think the key issue for people starting out is to realize that you are not starting out, you are already on a trajectory. As you create networks and listen to people, be aware of who you are listening to, and where they have been and what their trajectories are. I think a lot of the conventional wisdom is not well-based. In other words, to start out rather than to continue on; I think this is a bad way to think about

building a career. Advice along these lines may be well-intended, they have an eye on what is needed for promotion and tenure, and the old idea, the modernist idea, is to show separation from your mentors. I think this is wrong. I think that you have to establish your own identity as a scholar, but it is not done by starting afresh. So, in your case, I think it would not be a good move to sever your ties with me, for example. It is better to continue on and to use that network to help and to continue to build your career. If distance is necessary, then that'll be evident and we'll both realize that distance is necessary. This cutting of ties is an old-fashioned way, and if you look at the people that are giving this advice, they are generally people that never had good ties to begin with. My experience has been in the sciences where the way that this works is maintaining ties.

## CONCLUDING THOUGHTS

We would like to thank Fatih Tasar for the opportunity to publish this conversation. It is my hope that this conversation with Dr. Kenneth Tobin illuminates the various directions and trajectories that a research career can have over time, and that the points we have raised provide insights from which others can learn and gain inspiration.

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The audio recording of this conversation/interview is available from the journal web site.



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