



## Doubling Down on Serendipity

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### A Bit of History

I grew up in Toledo, Ohio, a blue-collar town close to the very polluted Lake Erie, in which I was never allowed to swim. Nowadays Toledo has lost much of its manufacturing base but in my youth it prospered, like its near neighbor and economic hub Detroit. I grew up in a house in “The Old West End,” a district of mostly single-family houses built in the late 1800s and so thoroughly passed over by any hint of economic renewal or expansion that it retains its status even today as an historic district on the National Register of Historic Places.

I attended private Catholic girls’ schools from kindergarten through high school. That choice was dictated not so much by my parents’ religiosity (my mother, offspring of Irish immigrants, was nominally and intermittently Catholic, but my father was anticlerical and very critical of the Catholic Church) as by their 1950s-style default racism; the local public school had an economically and racially mixed attendance zone. The Ursuline nuns who taught me at Saint Angela Hall were all pious and dedicated women, and occasionally quite competent. Catholic schools, for example, put a strong emphasis on penmanship and never succumbed to the temptations of whole language methods for teaching reading (not that it mattered to me, as my mother had taught me to read before I entered kindergarten). The school was small, and third and fourth grades were in a single class, which allowed me the advantage of doing both grades in one year (though it may be why I never really nailed long division). Sister Mary Aloysius gave me the regular task of helping a struggling reader get through our texts, an experience that opened up insights about patient scaffolding and the value of syllabic, morphological, and etymological analysis. I remember as early as second grade chafing greatly at the rote memorization of the daily catechism lessons, and a bit later being horrified at the simpering sycophantism the nuns showed to the decrepit priests who showed up for weekly ‘religion classes.’ I surmised only much later that teaching religion at a small girls’ school was a re-entry assignment for priests undergoing rehabilitation. I greatly appreciate that my early education provided so much material from which to critique organized religions of revelation, and that it

contributed so substantively to early inchoate feminist leanings.

I attended Oberlin College, an institution proud of its excellent conservatory (though I was, and remain, immune to the blandishments of music) and its status as the first institution of higher education in the US to admit black people and women. I became a psychology major, without any very strong reason for that choice, and was hired as a research assistant by Professor Celeste McCullough. In Professor McCullough's class on perception I had the privilege of being among the first to experience the contour-dependent color aftereffect she discovered, that is known to this day as the McCullough Effect and is now available to the world through the wonders of YouTube. Celeste had recently spent a sabbatical at McGill and suggested I should apply to their doctoral program. Being compliant, I did, and thus entered one of the last doctoral cohorts to experience the legendary Hebb Seminar, the only required course in the program (except for statistics).

The Psychology Program at McGill in the late sixties attracted many students (including many U.S. citizens hedging their bets on the draft) who would go on to significant accomplishments, thus perhaps confirming the value of a program that prioritized doing research over taking courses. As D.O. Hebb put it, explaining the brevity of his textbook of psychology, 'psychologists haven't discovered a goddamn thing worth teaching anybody.'

In order to stave off the boredom and anxieties engendered by our unscheduled time, the McGill psych students organized student-run seminars, in which more advanced students talked about their own research and the new ones volunteered to prepare presentations on topics of general interest. As it happened, I agreed (again for no very coherent reason) to prepare a presentation on language development, then a nascent research topic. I collected a variety of unpublished, mimeographed reports and just few

published books and articles devoted to the topic, including Chomsky's 1959 review of Skinner's "Verbal Behavior;" the 1964 paper by Roger Brown and Ursula Bellugi published in a special issue of the *Harvard Educational Review* devoted to language and literacy learning; papers by David McNeill and others from the Center for Cognitive Studies at Harvard collected in a conference proceedings that I can no longer locate. In the process of trying to understand this brand new field I was surprised by claims about the nature of what was then called 'primary linguistic data.' Though I had no personal knowledge about young children or their language environments, it struck me as unlikely that the language they heard was as garbled, ungrammatical, and impenetrable as presupposed by the linguists. Exploring (and undermining) that claim became the focus of my dissertation research. Publication of the findings (Snow, 1972) placed me in what was in the early 1970s the severely demodé environmentalist camp in the field of language acquisition. Nativist views had the intellectual upper hand and clear cachet among 'real linguists,' but fortunately, sociolinguists were somewhat interested in work like mine, from the perspective of *register* if not theories about language acquisition. Somehow or other, my thesis study subsequently caught the attention of Stanford linguist Charles Ferguson, a phonologist, Arabist, and sociolinguist who had studied (among many other things) the registers of baby talk and foreigner talk. Fergie secured funding from the Social Sciences Research Council for a conference devoted to these matters, and recruited me as co-organizer and subsequently as co-editor of the resultant volume (Snow & Ferguson, 1977). It was at that 1974 conference that I first met Roger Brown, Jean Berko Gleason, Elissa Newport, and Leila Gleitman, among other future colleagues.

In addition to being the site where I discovered child language as a field of research, McGill was where I met my future husband, Michael Baum, a fellow graduate

student in psychology. Michael was a year ahead of me in the program. He finished and received postdoctoral funding to go to The Netherlands while I was still writing my dissertation. I assumed (not entirely correctly) it would be easy to finish it in Europe, so I went along. For a brief period I was employed at the Medical Faculty of Rotterdam, doing behavioral testing to determine the impact of postnatal under-nutrition on learning capacity in rats, aiding biologists who were untrained in behavioral testing and learning paradigms (Slob, Snow & de Natrus-Mathot, 1973). Ultimately I reoriented my attention to the dissertation, got very helpful feedback from committee member John Macnamara, and graduated in 1971.

### Early Career

During my time at the Medical Faculty in Rotterdam I regularly offered unsolicited critiques of Dutch society, many of them focused on the endemic sexism of social roles in The Netherlands. This was no doubt somewhat annoying to the Dutch senior researchers (all male), and perhaps even to the assistants and support staff (almost all female). Ultimately, one of them gave me an ad for a position in the Department of General Linguistics at the University of Amsterdam, seeking a child linguist or psycholinguist who could teach statistics, with a promised concession on the claims of Dutch misogyny if I applied and were not invited for an interview. I lost the bet, but got my first academic job.

The department I joined was innovative in a number of ways. First, it had recently survived the widespread student protests of 1968, and responded with radical democratization. Second, it had two professors, a theoretical (Simon Dik) and an applied (Bernard Tervoort) linguist, who had chosen to team up to offer a broad and integrated curricular program. (Northern

European universities at the time featured a single professor/head of department who functioned in many cases as a local czar.) I thus ended up teaching statistics as well as child language and other such courses to students whose prior training was in Dutch Language and Literature studies. But I also had the chance to co-teach and collaborate with the legitimate linguists who were my colleagues, and from whom I learned a fair amount about phonology, grammar, lexical semantics, sociolinguistics, and creole studies. I remain grateful to Norval Smith, Henk van Riemsdijk, Pieter Muysken, and many students from that era for providing

me with some semblance of a formal training in at least one field. The value of a technical understanding of language for teachers, in particular bilingual/ESL teachers and teachers of reading, cannot be overestimated, though it is widely neglected in teacher preparation – a point Lily Wong Fillmore and I elaborated in a lengthy

chapter we sketched out in 2000 while stranded at a conference in Barcelona where all the talks except ours were being given in Catalan (Fillmore & Snow, 2002, in press).

One problem with working in The Netherlands was that it was rather difficult to study child language development before my own Dutch had become serviceable. In response, I launched a study designed to illuminate the claims about a biologically determined critical period for second language learning, recruiting as participants English speakers learning Dutch. I started the project with a rather vague notion that the nature of the input to the learners might explain why young children were so much faster and better at learning second languages than adolescents or adults. I was thus surprised to find that, in fact, on the wide array of measures my collaborator Marianne Hoefnagel-Höhle and I devised (there being no standard language assessments in Dutch at that time), the

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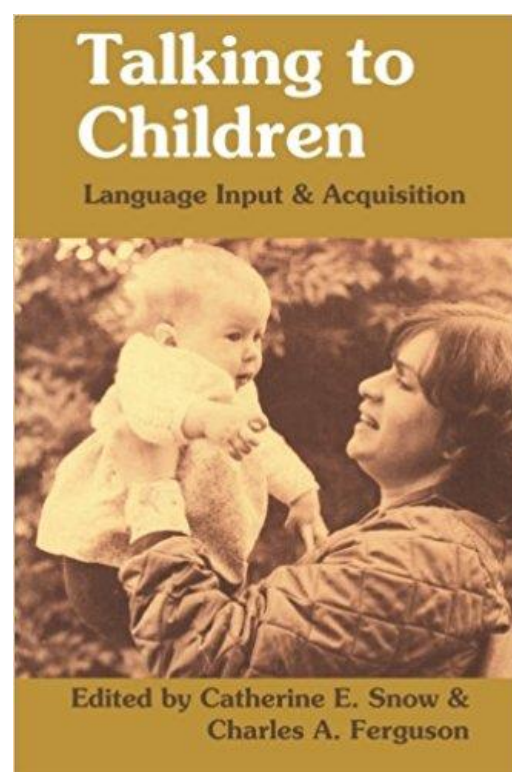
adolescents were by far the fastest and most efficient learners. Publications and presentations about this work (e.g., Snow & Hoefnagel-Höhle 1977, 1978) again put me squarely in the anti-nativist camp, and led to frank disbelief about the findings from many. Nonetheless, the fact that older learners are faster and more efficient at second language learning has now been replicated multiple times, though the implications for education policy and practice have never been fully acknowledged or processed.

### Working with Babies

In 1975, Michael and I had the opportunity to spend a year in Cambridge, England. Martin Richards, director of the somewhat misnamed Medical Psychology Unit (MPU; now the Centre for Family Research) at the University of Cambridge, displayed a remarkable lack of caution by agreeing to host me in his unit for a year, though we had never met. The MPU was the site of research on a diverse array of topics related to infant and child development, and it housed a lively group of doctoral students (Elena Lieven, Cathy Urwin, Jenny Corrigall, Ewan Klein, Joanna Hawthorne, Graham Richardson, among others) who were doing interesting research in between visits to smoky pubs and political meetings. Our lives in Cambridge were also enriched by the hospitality, friendship, and scientific interests of the group of scholars hosting Michael in the Department of Anatomy – Joe Herbert, Barry Keverne, Barry Everitt – who included us in innumerable dinner parties and pub lunches. I undertook during this time to videotape infants interacting with their mothers, in order to see the antecedents of the grammatical, redundant, and simplified input I had documented in my dissertation.

My time at the MPU was luxuriously unstructured, so when I came across a mimeographed preprint of a paper by Sacks, Schegloff, and Jefferson (1974) on conversational analysis, I took the time to read it. Though I was unfamiliar with either

the field or the authors, I was struck by the relevance of conversational exchange rules to the interactions I was observing between mothers and preverbal infants. I wrote an article applying the Sacks et al. 'simplest systematics' to mother-baby interactions (Snow, 1977), and started to recognize in a still somewhat underspecified way how social interaction created a context for learning language that went far beyond ensuring simplified linguistic input. Only later did I discover that the dog-eared mimeo I had read was a foundational work in the field of conversation analysis.



Back at the University of Amsterdam, I followed up on my interest in mother-infant interaction, launching a study together with students Akke de Blauw, Clara Dubber, and Ghislaine van Roosmalen focused on comparing Dutch and English patterns of play, talk, and child-rearing (De Blauw, Dubber, van Roosmalen & Snow, 1978; Snow, de Blauw & van Roosmalen, 1978). Unfortunately, the study was aborted because in 1978 Michael and I decided to move back to the US and the Dutch funding agency declined to support an international

collaboration – ironically, robust international partnerships of just that sort are highly valued nowadays by European universities.

### **From Linguistics to Education**

Upon returning to the states, I was fortunate to be welcomed to the Harvard Graduate School of Education by Human Development and Psychology department chair Bob LeVine and by Courtney Cazden (who had been the outside examiner on my Ph.D. thesis). Though I was sorry not to be able to work with Courtney that year, her departure to San Diego for sabbatical opened up a slot for me to teach her child language course. During that first year at HGSE I also got to know Jeanne Chall, whose fierceness about phonics and whole-hearted commitment to the reading wars led me to vow to stick with language and stay far away from reading research!

My early years at HGSE opened up to me a very different approach to thinking about research from that prevalent in the linguistics department in Amsterdam. Students and colleagues were focused on real-world problems – operating in what we have now come to call Pasteur's quadrant – rather than the theoretical issues favored by most linguists. They were also in general interested in older children than those focused on by child language researchers – and thus questions about literacy development and school success naturally started to interest me as well. A highlight of those years was the wonderful collaborations with HGSE students and recent graduates – Beverly Goldfield, Herlinda Cancino, Patricia Velasco, Matthijs Koopmans, Rosalind Davidson, Wendy Slattebo Barnes, Irene Goldman, Jean Chandler, Patton Tabors, David Dickinson, among many others.

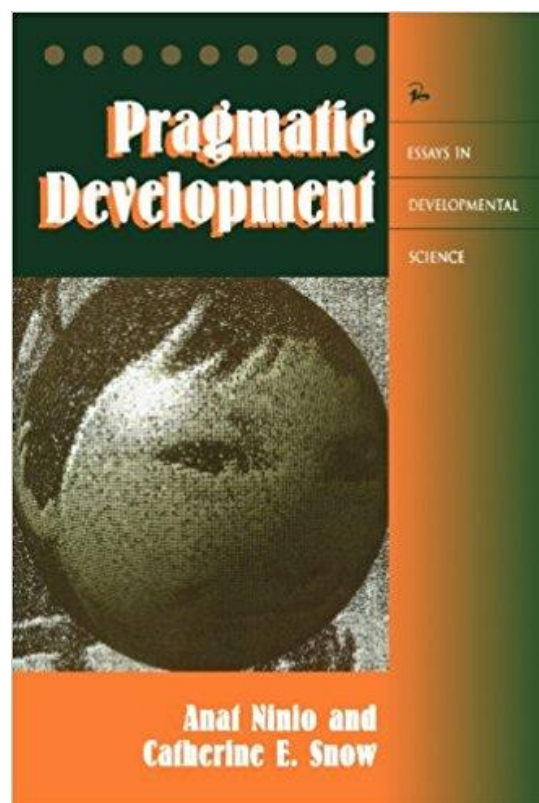
At the same time, though, my ties to traditional child language research had not been cut. For one thing, I now had a toddler at home, Nathaniel Baum-Snow, whose highly imitative and phonologically complex approach to language development was a

source of recurrent puzzlement (e.g., Snow & Goldfield, 1983). The child language focus was further extended by my membership in Jerome Kagan's 'node' of the newly established MacArthur Research Network on the Transition from Infancy to Childhood. Productive collaborations (and lots of child language data) were made possible by the node, and by the Network – notably through the establishment of the Child Language Data Exchange System (CHILDES), conceived in collaboration with Brian MacWhinney (MacWhinney & Snow, 1985, 1990) and in emulation of Roger Brown's generosity in producing and sharing mimeographed copies of transcripts from Adam, Eve, and Sarah during the first days of child language research.

Though MacWhinney was (and remains) the design genius behind CHILDES, I contributed modestly to the uptake of the tools by leading workshops to make its many features accessible to users during a period when computer analysis was unfamiliar and unintuitive, and by securing funding for a Program Project grant to demonstrate its utility in understanding trajectories of language development among children with various developmental histories (Snow & Pan, 1993). That work offered opportunities for collaboration with Heidi Feldman, Jean Berko Gleason, Pamela Rollins, Gina Conti-Ramsden, Jeff Sokolov, and many others (e.g., Rollins, Pan, Conti-Ramsden & Snow, 1994), but most notably with Barbara Pan, who co-taught many CHILDES workshops and coordinated the project on communicative development of normally developing children conducted at HGSE (Pan, Imbens-Bailey, Winner & Snow, 1996; Snow, Pan, Imbens-Bailey & Herman, 1996). Barbara subsequently went on lead one of the Early Head Start Evaluation projects, in the context of which we analyzed contexts for language acquisition among children in families living in poverty (e.g., Pan, Rowe, Singer & Snow, 2005). She also taught greatly appreciated courses at Harvard before her premature death in 2011.

Between 1985 and 1995 I was involved in several large-scale projects, all of which generated so much data that reams remain unanalyzed even today. An early project was facilitated by Jeanne Chall, who took me along on a visit to the United Nations International School (UNIS). It was an inspiring school environment, in which children from many different language backgrounds did high-level work in English, while also studying French. The work going on inside classrooms at UNIS contrasted sharply with level of instruction in many public school bilingual programs serving children of immigrant families, thus giving me a personal context for recognizing the sociolinguistic dimensions of second language acquisition, which is too often viewed as a purely psycholinguistic challenge. Collaborating with Kenji Hakuta, who was then at Yale, had opened up research opportunities for exploring predictors of success in English for immigrant Spanish speakers (Rodino & Snow, 1997), which Herlinda Cancino and I then replicated and expanded with the UNIS students. One striking finding from that work that has re-emerged in a much more sophisticated form in the current work on Academic Language (e.g., Uccelli, Barr, Dobbs, Galloway, Meneses, & Sanchez, 2014) was the power of children's definitions to index their analytic, literacy-relevant language skills (Davidson, Kline & Snow, 1986; Snow, Cancino, Gonzalez & Shriberg, 1989). That relationship held for the Spanish-English bilinguals at a school in New Haven and French-English bilinguals attending an elite bilingual school in Massachusetts, as well as for the UNIS students, and was replicated again with monolingual English speakers from low-income households in a later study (Kurland & Snow, 1997). Definitions require both conceptual analysis and precise vocabulary, and thus represent a quick view into a complex domain.

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Another lesson from the UNIS study was the importance of task in evaluating language proficiency. Students at UNIS scored higher in French, their much less proficient language overall, than in English on an interview task (Schley & Snow, 1972). Their French instruction had provided them with many routines for initiating conversation and asking follow-up questions, and for some reason they did not transfer those skills to their stronger language, English. Their narratives and picture descriptions, on the other hand, were much more sophisticated in English than in French, again throwing the notion of 'general language proficiency' into some doubt (DeTemple, Wu & Snow, 1991).

Work with elementary-aged bilinguals had convinced me that traditional approaches to tracking language development – measuring vocabulary and MLU – were inadequate, that experiences of language use in different settings predicted setting-specific skills. This was, in effect, the practice-oriented analogue of the theoretical

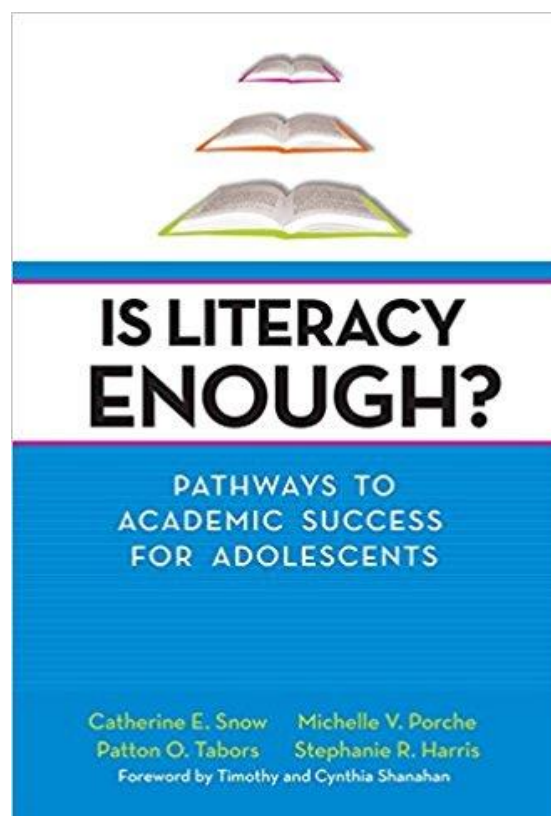
commitments developed in collaboration with Anat Ninio in the book *Pragmatic Development* (1996), in which we argued that young children's precocious social-pragmatic/communicative skills were the foundation for conventional language development – an argument later developed much more fully and with lots more data by Michael Tomasello and his many collaborators in Leipzig (much of it summarized in Tomasello, 2008).

Interest in informing practice also led to questions about how children acquire more sophisticated language skills, such as those tapped by the definitions task, that relate to performance in academic tasks like reading and writing. What early experiences promoted such skills? To sort that out, Patton Tabors and I conceptualized a longitudinal study of children from low-income households, with the plan to observe the children interacting with their mothers in a variety of settings (bookreading, playing with toys, mealtimes) in order to extract predictors of literacy outcomes in the early school years. David Dickinson convinced us that we needed to add observations of the children in their Head Start or other daycare settings. Thus we started the Home-School Study of Language and Literacy Development (HSSLD), an undertaking that, like most longitudinal studies, resisted closure. After investing four years in collecting data on children aged 3-7, as originally planned (e.g., Snow, Tabors, Nicholson & Kurland, 1995), we decided we had to find the resources to continue to follow them into the primary years, and then ultimately through high school (Snow, Porche, Tabors & Harris, 2007)! If there is a lesson to be learned from this experience, it is beware what you are getting yourself into when you start a longitudinal study!

### Into Reading, Seriously

I had by about 1995 conducted a few studies and written several dozen papers and a book about the predictors of good literacy outcomes, in particular for students at risk. The theme of much of that work was that

language skills – vocabulary in particular – explained a lot of the variance in reading outcomes, especially after third grade when the challenges of decoding accurately and fluently have been successfully met by most students. I had never forgotten, though, my alarm at the level of acrimony that seemed to surround “real” reading researchers, those who wrote and talked about reading instruction. The rancor that divided Jeanne Chall and Ken Goodman, for example, appalled me, given that they were both devoted to ensuring children learn to read, and they might have been good friends in another sociological universe. Though focusing considerable energy on understanding what experiences contribute to children's reading success, I had stayed far away from questions about instruction.



As a result, I had not publicly lined up on either side of the Reading Wars. Thus when the National Academy of Sciences (NAS, now National Academies of Science, Engineering, and Medicine) decided that it was time to launch a consensus report about reading, they approached me to be the untainted chair of the Committee, which

ultimately produced the report *Preventing Reading Difficulties in Young Children* (PRD; Snow, Burns & Griffin, 1998). Other committee members were pretty evenly divided, about half clearly lined up on either side of the issue. Nonetheless, the NAS decided that getting to consensus would be possible. And indeed it was, ultimately, by virtue of the hard work of Susan Burns and Peg Griffin, who staffed the committee, and the generously collegial attitudes of a few of the committee members on each side. Of course, it turned out that there were many points on which everyone could agree – the public squabbles in the field of reading is a classic case of the narcissism of small differences. There were many rewards associated with chairing PRD – among them the chance to learn a lot about reading instruction and to get to know some of the leaders in the field of reading. The report emerged to considerable acclaim, but unfortunately got caught up in the maws of the efforts to promote No Child Left Behind and Reading First; PRD was seen simultaneously as an obstacle by supporters of Reading First (“not sufficiently focused on phonics”) and as single-mindedly supportive of the Reading First phonics focus by opponents, including some who misrepresented the report as sponsored by Reid Lyon (Goodman, 2008). In fact, Reid was working furtively but proactively to undermine its influence by promoting as an alternative the *National Reading Panel Report*, which offered a compendium of “Scientifically Based Reading Research” via meta-analyses of lines of work that were completely unintegrated with one another or with the realities of classroom instruction. Lesson learned? Never assume that good research by itself will win the day.

### Coming to Grips with the Realities of Educational Research

The PRD committee’s work was overseen at the National Research Council by Alexandra Wigdor, who was also managing a project championed and

sponsored by Bruce Alberts, then president of the National Academies. Bruce, himself a biochemist, was puzzled and worried that educational research had not revolutionized the field of education in ways that medical or agricultural research had changed practices in those fields. He funded a series of reports to explore ways to replicate within education the close mutual connections between research and practice that exist, for example, at the National Cancer Hospitals or Agricultural Research Stations. In 2000 I was invited to join the culminating committee in that effort, and thus was inducted into a series of conversations and undertakings that have dramatically transformed my thinking about education and educational research.

The Strategic Education Research Partnership report (SERP; Donovan, Wigdor & Snow, 2003) proposed an idealistic plan, predicated on large amounts of funding which have, alas, never become available, to transform the relationship between research and practice. We realized that traditional models, focused on “translation” of research into practice, “applying” research to practice, or making research “usable” for practice, prioritized the researcher’s perspective. We proposed a new model in which researchers would focus on questions and issues nominated by practitioners. This would require establishing robust partnerships with practitioners, so that participants on both sides could learn about the other group’s concerns, constraints, and ways of working.

I could immediately see that the SERP approach resolved many problems. I had had the experience of devising educational interventions that proved successful, only to see them disappear after the study was completed – because the teachers had been implementing them at our request, rather than because these programs met their perceived needs (Carlo et al., 2004). I had frequently acceded to requests to provide professional development to schools or districts only to realize how

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difficult it was to make my recent research findings relevant to the teachers' concerns. I had also been involved in administrative

leadership at the Harvard Graduate School of Education, and so had confronted recurrent questions about how schools of education justify their existence if their research paradigms are just like those in faculties of arts and sciences, except perhaps with an additional dash of critical theory. What distinguishes education research, and how does it justify itself if not by improving education?

The Strategic Education Research Partnership (SERP) was ultimately launched, with some start-up funds from various foundations and a strong partnership with the Boston Public Schools, then led by Superintendent Tom Payzant. The BPS-SERP partnership was a fertile context for the development of tools, curricular units, assessments, and ways of working that have since proven their worth in hundreds of schools and dozens of districts (see [www.serp.institute.org](http://www.serp.institute.org)). The SERP work in Boston launched Word Generation ([www.wordgen.serpmedia.org](http://www.wordgen.serpmedia.org)), originally designed as a 'drop-in' academic vocabulary program to respond to BPS teacher requests. In observing the program being implemented, we realized that the debates we had included in order to create a context for students to use their newly learned words were themselves the active ingredient in the program. Students were deeply engaged, and teachers reported observing that their students could think more deeply and more powerfully than they had previously realized. In order to keep the BPS partnership work going during the early days, when funding was limited, I started teaching a new kind of course—a Research Practicum designed as a kind of 'lab' embedded in the ongoing research. With the assistance of Claire White and postdoc Joshua Lawrence, we got lots of work done showing the effectiveness of Word

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Generation (e.g., Lawrence, Capotosto, Branum-Martin, White & Snow, 2012; Snow, Lawrence & White, 2009) while simultaneously training several cohorts of students in

methods for doing research that are responsive to practitioners' urgent needs. Learning how to work with schools rather than in schools changes the questions one asks and greatly increases the likelihood that someone will notice the answers.

SERP emerged at just the right time to formulate a set of principles that appealed widely to education researchers frustrated by the inadequacies of translational models. Design-based implementation research (DBIR) and research-practice partnerships (RPPs) are now standard approaches (see, for example, Fishman, Penueel, Allen, Cheng & Sabelli, 2013), with their own champions, funding streams, and rationales; SERP did not itself ever grow to be the well-funded model of highly networked partnerships envisioned in 2003, but it did create a template and set the standards for much of the work that has followed.

The success of Word Generation and the work in BPS that surrounded it put us in a position to compete successfully for one of the large Reading for Understanding grants from the Institute for Education Sciences in 2010. Our group, called Catalyzing Comprehension through Discussion and Debate (CCDD), undertook the work of expanding and improving Word Generation, evaluating its effectiveness, and testing the underlying theory that post-primary grade students' reading difficulties are associated with failures to understand the academic language of texts, to follow arguments, and to recognize linguistic signals of alternative perspectives. The CCDD work demonstrated the viability of that claim, and the efficacy of Word Generation curricular materials to help students grow in all those areas as well as reading comprehension itself, by introducing and supporting good classroom discussion. Furthermore, the work allowed

for robust collaborations with my HGSE colleagues Robert Selman, Paola Uccelli, and James Kim, as well with Lowry Hemphill of Wheelock College on related lines of work. Meanwhile Suzanne Donovan was leading SERP into ever new and exciting ways of linking research to practice.

### Comings and Goings

Among the great pleasures of academic life is the opportunity to spend time in different places. The academic joys of spending 1975-76 in Cambridge, England, were enhanced by the two successive warm, dry summers the British Isles enjoyed during that time. A semester spent in New York City while working on the UNIS study enabled me to participate in recurrent semiformal discussions with Jerry Bruner, Carol Feldman, Dan Stern, Katherine Nelson, and others. I spent eight months at the Institute of Advanced Study in Jerusalem in 1983, establishing the basis for later collaborations with Anat Ninio and Shoshana Blum-Kulka, learning from Institute fellows Itzhak Schlesinger, Robert Cooper, Yonata Levy, Michael Maratsos, and Ruth Berman, among others, and coming to understand a bit of the complexity of Palestinian-Israeli relations in the years before that complexity was exacerbated by the rapid growth of settlements. I spent five months in Madrid in 1995, mostly focused on learning Spanish, a task made easier by the unsurpassed hospitality of Jose Luis Linaza and his many academic colleagues, the first cohort of Spanish psychologists trained during the Franco era. Since then I have benefited from regular trips to Oslo, to fulfill the responsibilities of a visiting professorship in the Faculty of Education and to pursue collaborations and conversations with Vibeke Grøver, Bente Hagtvet, Ivar Bråten, Veslemoy Rydland, and their many colleagues and students. I have also had the pleasure of working with Elizabeth Henning, Sarah Gravett, and their colleagues in education at the University of Johannesburg, where I am a distinguished visiting professor.

### Lessons Learned

The lessons I have learned over the last many years seem always to come in pairs – a lesson about the findings that brings with it a lesson about life as a researcher. I offer five such pairs here.

**Lesson 1.** Even as a doctoral student, I believed that the sorts of social interactions young children had with adults supported language acquisition. In 1971, when I completed my dissertation, that was a minority view, and one ridiculed by many. I was, unfortunately, deflected from a full-on commitment to research on the relationship between social environment and language development for many years by the general atmosphere of disdain for such claims. In the intervening years, of course, evidence to support the claim has accumulated, and now it is generally acknowledged that a large part of the variance among children in language skills can be explained by their language environments. This consensus might have been achieved earlier had I and others been braver about pursuing it.

**Lesson 2.** The issue of how language environments relate to language outcomes is confounded and complexified by social class. It is undeniable that some of the variance in quality of children's language environments is associated with social class, for reasons that have to do with parental education, parental knowledge about child rearing, parental fiscal and personal resources, and parental beliefs. This fact means that efforts to promote a particular model of language support can be interpreted as suppressing culturally appropriate practices or demeaning the most vulnerable groups in society. Thus acknowledging the robust findings about language environments can harm precisely the families one most hopes to help. The lesson here is to anticipate how others will interpret one's findings, and to seek common ground. Not all families may embrace the notion that getting young children to talk a lot is a good idea, but they do mostly want their children to do well in

school. Children need certain language skills to thrive in the modern, western models of schooling almost all will encounter, wherever they grow up. Finding our common ground with families – almost all of which want school success for their children – can help circumvent conflicts about particular child-rearing strategies.

**Lesson 3.** Language is the foundation for literacy, and advanced ‘academic’ language skills are critical to success in later grades literacy. This is not a controversial statement, but it is one that is often heard as licensing a laissez-faire attitude to teaching children how to map sounds onto letters. Well-designed and properly sequenced instruction in the alphabetic principle is crucial for some children, helpful for many children, and harmful to none. Code-focused instruction can be fitted into a limited portion of a literacy block during which lots of language and opportunities for content learning are also offered. These facts are clear, but messages about them are often distorted into either/or rather than both/and. Learning how to anticipate the way in which one’s claims will be mis- or over-interpreted is as important as learning what claims to make.

**Lesson 4.** Focusing on the urgent problems of practice that face educators could be thought of as in conflict with doing good research or generating novel insights. In fact, I (and increasing numbers of scholars) have found just the opposite – that the issues raised by teachers and curriculum developers are more generative and more exciting to work on than the questions that emerge from reviews of the literature or the history of research in a particular field. In the process of trying to improve educational practice we in fact improve our understanding of language and literacy development.

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**Lesson 5.** Finally, while I often counsel doctoral students to think about what they want to be doing in 5 and 10 years, and to sequence their doctoral research so that the early studies solve problems that might arise in the later ones, I never operate this way myself. I have benefited enormously in my career from serendipity – from having picked up random papers to read, having gone to conference sessions just because the room was convenient, having fallen into conversation with relative strangers. Serendipity gave me the opportunity to learn linguistics and study bilingualism at first hand as well as to learn about literacy and the kinds of instructional activities that ensure its development. Serendipity and a certain willingness to go with the flow have afforded me opportunities to collaborate with scholars from England, Spain, Norway, The Netherlands, and South Africa, as well as to

learn about those countries and their educational systems. Planfulness can be a good plan, but I personally have benefited more from taking chances and following detours.

My colleague and current collaborator Robert Selman would note, if he were reading this, that serendipity favors the prepared mind. This is just another way of saying, I suppose, that it is a better strategy for some people than for others, and better for some domains of life than others. I am often struck, though, by my (and others’) willingness to invest a great deal of energy and strategic analysis in some decisions – what brand of dishwasher to buy, or what hotel to choose for a one-week vacation – while operating on instinct in other domains, such as where to go to graduate school, who to marry, where to live, or what job to take. If serendipity favors the prepared mind, it also favors those with good instincts and good luck.

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## **About Acquired Wisdom**

This collection began with an invitation to one of the editors, Sigmund Tobias, from Norman Shapiro a former colleague at the City College of New York (CCNY). Shapiro invited retired CCNY faculty members to prepare manuscripts describing what they learned during their College careers that could be of value to new appointees and former colleagues. It seemed to us that a project describing the experiences of internationally known and distinguished researchers in Educational Psychology and Educational Research would be of benefit to many colleagues, especially younger ones entering those disciplines. We decided to include senior scholars in the fields of adult learning and training because, although often neglected by educational researchers, their work is quite relevant to our fields and graduate students could find productive and gainful positions in that area.

Junior faculty and grad students in Educational Psychology, Educational Research, and related disciplines, could learn much from the experiences of senior researchers. Doctoral students are exposed to courses or seminars about history of the discipline as well as the field's overarching purposes and its important contributors. .

A second audience for this project include the practitioners and researchers in disciplines represented by the chapter authors. This audience could learn from the experiences of eminent researchers—how their experiences shaped their work, and what they see as their major contributions—and readers might relate their own work to that of the scholars. Authors were advised that they were free to organize their chapters as they saw fit, provided that their manuscripts contained these elements: 1) their perceived major contributions to the discipline, 2) major lessons learned during their careers, 3) their opinions about the personal and 4) situational factors (institutions and other affiliations, colleagues, advisors, and advisees) that stimulated their significant work.

We hope that the contributions of distinguished researchers receive the wide readership they deserve and serves as a resource to the future practitioners and researchers in these fields.



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