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Why Does Literacy Research So Often Ignore What Really Matters?*

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Literacy education has once again risen near the top of the public agenda. As evidence, I would point to the recent Presidential initiative focused on developing independent readers by third grade and the several state-level initiatives in the same direction and the many other initiatives to raise the standards for expected levels of literacy proficiency at different points in the schooling process. I would also point to the contentious arguments about how best to teach reading that are now profligate in the mainstream media. There is much evidence of a public concern, reality based or not, about the sufficiency of current public education efforts in terms of literacy outcomes.

At the same time, a handful of formal critiques and policy analyses suggest public education policy and practice rarely seems informed by the research that is available (Teddlie & Stringfield, 1993). Educational research, generally, and literacy research, in particular, seem more often ignored than sensibly applied in developing educational policies or recommended practices. In fact, educational research continues to be held in low regard by both policy-makers and practitioners (Lagemann, 1996). It is in this context, then, that I want to discuss the efficacy of literacy research—the essence of the National Reading Conference—in enhancing public education, one of the potentially powerful uses of educational research.

ENGAGING A PUBLIC BEYOND OUR PEERS

> The issue at hand, then, is not to convince citizens that schooling is important, there is a deep faith that better education is linked to societal progress. The key problem is to devise plausible policies for improvement of schooling that can command the support of a worried public and the commitment of educators upon whom reform must rely. (Tyack & Cuban, 1995, p. 39)

Teachers, administrators, local and state board members, legislators, parents, and community leaders comprise the "worried public" that jockey for control of the public education environments we would alter, improve, modify, or eliminate. Public education involves a variety of actors, each with potentially differing agendas. For instance, the agendas of children, parents, taxpayers, employers, elected officials, teachers, and education researchers all might very well differ. But we seem to often overlook the agendas

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of the other actors and, in fact, may even belittle those agendas in our professional conversations. We seem more interested in them—all of them—listening to us than we are in listening to them. I will suggest that communication is always a two-way street but often we act as if we owned the one-way highway we travel along.

I think we need to reflect more often on just what our inquiry might have to offer the other actors in the public education enterprise interested in developing the "plausible policies for the improvement of schooling." I think we need to broaden our inquiry to regularly engage these other actors. For instance, there are literally hundreds of recent examples of public influence, or attempts to influence, the sort of literacy environments children experience. But I know of no recent effort to even describe and document such "lobbying" activity much less explain them (though, historically, one can point to Moffett's [1988] *Storm in the Mountains* as one of a few available examples of inquiry that counters this general trend). Similarly, there are too few analyses of legislative action affecting literacy education (though McGill-Franzen [1993] provides a key report here), parent/community advocacy for (or resistance to) the implementation of particular literacy environments, and of administrative rationales for current literacy program organizational plans. Public education involves a variety of actors but our research efforts explore the stances and roles of but few of the many involved.

Public opinion, I will suggest, is more often shaped by media reports than by literacy research. But the media tends to operate on a "crisis" mentality, focusing on worst case scenarios and simplistic cause/effect arguments (Kaplan, 1992). Still, according to the PDK/Gallup poll, most American parents with children in school report a certain confidence that the schools their children attend rate a grade of B or better (Elam, 1995). Parents work from both local knowledge and media information. Most of what parents know about their children's schools comes from what they hear and see in person from their children, teachers, and other parents. However, most of what parents know about other schools comes from media reports—reports like the broad television and newspaper coverage given this week to the teacher here in South Carolina who wrote, "Where are my glasses?" on the face of one of her kindergarten students before sending her home. The local Board voted to keep the teacher, the county Board and general public opinion voted to relieve her of her teaching duties. What did the local Board know? What did it believe about teaching and learning, about teachers and children? More importantly, perhaps, what did the public come to "know" about public schools? Public school teachers? Elected school boards?

I am not going to belabor this particular incident but I will argue that much of what any of us knows about schools derives from a combination of local knowledge and media knowledge. In fact, I will suggest that most of the research community derives much of its knowledge of public schools from the media (including our professional journals which seem more trend oriented than crisis oriented). Some members of the research community actually enter public school buildings and attempt to study teaching and learning while there. However, there seems to be only a few researchers who visit many schools and even fewer enter schools beyond their local communities. My reading of the research indicates that even those who enter schools typically spend only a few days a year in any given school or classroom. In addition, the descriptions of the research sites found most often in our journals seem to indicate that rarely are the

most impoverished, the most remote, or the most dysfunctional schools the ones that are entered. It seems that the schools most often studied are schools that are local, familiar, and schools our children attend.

Beyond the Local and the Narrow

I believe that because so few spend such limited amounts of time in such a restricted array of schools, much of our research reflects quite impoverished depictions of the curriculum, the instruction, and the organizational culture of public schools. In my view, literacy research generally offers local, narrow, and limited depictions of almost all aspects of the educational process except that aspect chosen to study. Some argue for an emphasis on "basic research" questions—questions that, unfortunately, often ignore virtually all aspects of the educational experiences of the subjects studied. In other words, literacy research that examines the strategies that children experiencing difficulty in school use and do not use, children's vocabulary growth or sight-word recognition, the acquisition of phonemic segmentation skills or composing competence, and so on. And then the studies often attempt to offer generalizable cause and effect statements. But the data reflect local conditions, at best, and even if local conditions are reliably represented in the sample, there is typically little convincing evidence that these local conditions approximate the conditions of education that might be found in another community, state, region, or nation. Without reasonably good information on the local conditions, it seems a little far-fetched to attempt to infer much about what led to the patterns observed or what might be considered "normal" development (Bray & Thomas, 1995).

For instance, unless it can be demonstrated that the classroom environments of the children in a study of vocabulary development reliably represent the variety of children and the variety of instructional settings found outside the local context of a particular study, it is difficult to develop inferences about vocabulary development generally. With little good information on the nature and range of local instructional environments, it becomes enormously more difficult to develop inferences about any patterns of development observed in the outcomes collected, much less inferences about how instruction can or should be altered to change the outcomes. But rarely do I see much evidence of caution observed in making such inferential leaps in the papers written and the talks given.

Even when considering field-based, classroom-based instructionally focused research, it is difficult to develop generalizable inferences because most researchers enter only a small number of the classrooms even in our local communities. Most spend rather little time in those classrooms. Most field-based studies have specific agendas, and often that agenda has rather less to do with solving particular, local problems—the reading difficulties of a particular child in a particular instructional setting, for instance—than with solving broader, general problems—how children in most, if not all, classrooms might come to participate in a book discussion or develop adequate phonemic segmentation abilities (or learn to spell the word *elephant*). But the knowledge acquired in the restricted range of classrooms studied seems largely unsuited for addressing broad, general issues or questions. Perhaps because of this the questions we ask are often kept adroitly narrow and specific. I think greater restraint is generally called for in the sorts of conclusions and recommendations developed "based on" literacy research of the usual sort.

The issue here is, of course, akin to what historically has been referred to as "generalizeability." But I am approaching the point in my career where I wonder whether anything is ever generalizable in literacy research, at least given the way research is normally conducted. Tuchman (1994) stated well my concerns when she wrote:

> But patterns are just that: *Merely patterns.* Attributing *meaning* to patterns is quite a different matter. (p. 311)

A large part of my wondering arises from the field-based work that I have been doing with my colleagues at Albany over the past decade or so (e.g., Allington, Guice, Michelson, Baker, & Li, 1996; Allington & McGill-Franzen, 1989, 1992; Johnston, Allington, & Afflerbach, 1985). Many, if not most, of our studies have been designed in attempt to try and capture the relational nature of the processes of teaching and learning as embedded in the larger organizational context of schools, districts, and states. We have dubbed our methodology a "multi-level design" because we have attempted to broaden the context of field-based inquiry conducted in schools and examine how and, perhaps, why the things we observe occur. Sometimes, however, it does not seem like my colleagues and I have actually captured what did occur, much less adequately captured the how and why of the observed events.

Nonetheless, I am convinced that most of the time literacy research is conducted in too small a box. For instance, in watching teachers in classrooms over time we notice that teachers seem changed by the cohort of students they teach—even when the students, year after year, remain at the same age levels (e.g., second graders) and the curricular goals remain relatively stable. That observation should not be surprising, but it seems to me rarely captured in the studies that appear in our journals. Teachers differ in how they teach different students and they differ as they teach different subjects to the same students. They differ across the school year and from year to year.

And what would happen to the teacher's and teaching we observe if we changed the local context substantially? For instance, if the teacher we study was assigned to a different school? To a different grade level in different school? To a different grade level in a different community, in a different school, with a different organizational culture and, perhaps, with different curriculum goals?

Usefulness of Research

Even the studies that Raphael and Brock (this volume) use as exemplars would seem constrained by local conditions—even though these studies represent our best attempts at making research convincing, contextual, and generalizable. In each case, though, important local conditions existed—as is the case in virtually all field-based research—that necessarily restrict generalizing the findings much beyond the schools and classrooms studied and the teachers and research team involved. Most such studies inform us about the relative capacity of the particular participants (e.g., teachers and researchers) to produce, jointly, some instructional change. But can we reasonably expect others to take the information from any of these studies and adequately and

successfully implement largely comparable instructional alterations in almost any other school? Is there anything about school districts, schools, teachers, and children who voluntarily participate in research projects that is generalizable? Do such studies (most studies?) offer anything truly useful for enhancing literacy education? For enhancing public education? Or is it enough to say that the research effort demonstrated that X can be done by this group of researchers (and/or teachers) and X produced these sorts of effects with these children in these classrooms in this community? Can we generalize beyond the classrooms we work in? Do we? If we do not, will not, and cannot generalize, is our work largely ego-centric curiosity seeking and self-promotion?

It seems to me, for instance, obvious that different communities create and sustain different kinds of school systems and different kinds of teachers. What little we know about such matters largely revolves around differences in organizational cultures (e.g., how educational decisions are made and by who—Board vs. Superintendent vs. teacher committees) and organizational expectations (e.g., Pygmalion effects, low expectations). But although we may capture some of these differences some of the time, I think there is a very limited understanding of how schools that exhibit different cultures came to be created and sustained or the impact of those cultural differences on how literacy is taught and acquired. We seem better at describing classroom practice than explaining it. A clearer understanding of these issues would be truly useful.

For yet another example, consider that although we do much research on a variety of topics, we have an incredibly limited stock of information on what would seem a critical issue for the profession—good teaching. There is a general policy logic in our profession that goes something like this: *Teachers who are more expert about language and literacy construct different sorts of classroom literacy experiences that produce improved literacy outcomes compared to teachers with little expertise in language and literacy.* This policy logic would seem to sit underneath advocacy of policies for improved teacher education in literacy instruction, for supporting teacher research, for developing school-university partnerships, teacher centers, teachers as readers groups, and so on. But I will suggest that it is exceedingly difficult to locate any research that actually supports the policy logic for enhancing teacher expertise on language and literacy development. Perhaps this is because there seems to be little consensus on what constitutes "improved literacy outcomes" and "good" teaching within our research community (see also Ruddell in this volume). But without convincing evidence supporting the policy logic, should we be surprised that policy-makers often develop educational policies that would seem egregious violations of the logical assumptions we hold?

My point is that we are often critical of the other actors—the media, school administrators, board members, parents, or legislators—for their simplistic cause-effect conclusions based on insufficient or unsatisfactory data. But do we not actually engage in the same sort of simplistic thinking? Have we considered what we really have to offer those concerned with the literacy outcomes of current public education programs? Our *Journal of Literacy Research* recently included a series of papers about what policymakers need to know, but have we much considered what it is that we really need to know? Not just about the realities of policy-making and school administrating but about literacy teaching and learning?

CONFRONTING BIGGER ISSUES

I think we often fail to confront the big issues—the important issues—facing public schooling and literacy education. We debate, argue, and even belittle ideas, especially those of the other actors, but I do think we often ignore the big issues that we need to examine, discuss, and, yes, argue about.

For instance, we seem forever fixated upon curriculum debates and debates about research methodology. But I am not sure that curriculum, in the sense we usually debate it, actually matters very much. And it is quite obvious that a wide variety of research methods offer useful insights on important educational issues. So it is a puzzle to me as to why debates about curriculum and method occupy so much time and emotion in our profession.

Curriculum

I do not mean to suggest that curriculum has no influence on teaching and learning but, rather, that we enormously overestimate the role of curriculum materials in this process. Look at the First Grade Studies of the 1960s (Bond & Dykstra, 1967) and you find curriculum did not really matter. Look at the current large-scale studies of effective schools and teachers (e.g., Knapp, 1995; Pressley, Rankin, & Yokoi, 1996; Teddlie & Stringfield, 1993) and you will find an incredibly diverse array of curriculum components and implementations that produce improved outcomes of various sorts. Yesterday, Gloria Ladson-Billings described two successful and effective teachers, and in her book, *The Dreamkeepers* (Ladson-Billings, 1994), describes a number of others. She commented on the traditional versus more progressive nature of the literacy lessons offered by the two successful teachers and her book describes an even wider array of lesson types and curriculum plans. In his Causey Award address (this volume), Robert Ruddell describes influential teachers, not influential curriculum. But our professional debates and those in the larger public media invariably focus on curriculum (e.g., the current phonics vs. whole language vs. balanced debates).

The potential irrelevancy of curriculum was driven home to me in an interview with an enormously successful second-grade teacher I interviewed 6 years ago in the first year of a longitudinal study. This teacher worked in a basal and books school but a school that gave teachers more authority to decide themselves what sorts of curriculum to create. I asked her about her almost exclusive use of trade books and a reading/writing workshop approach. I asked how she came to use this sort of plan and her views on the contribution of the plan to the success of her students. She then offered me a career history detailing the use of a variety of curriculum plans over a 20-year period—ranging from direct instruction to phonogram-based program readers to traditional basals to her current use of reading/writing workshop. But, she noted, she had always been successful in teaching children to read and offered her opinion that she could go back to any of the curriculum plans she had previously used without affecting her success.

So I asked her what it was that she attributed her success to if not the literacy lesson—if not the curriculum plans. She stared me in the eye and said, "I simply will them to read. It is the sheer power of making them understand that they will become readers. That they can trust me to help them and that they will become readers and writers in this classroom." She said using trade books "just felt right" at that point in time but she would not rule out a return to the basal or the invention of some other curriculum plan. I was more than a little taken aback. "Will power" was the solution?

I thought back to the "miracle worker" second-grade teacher one of my sons had—the teacher who made him a reader after his first-grade teacher had created some doubts about becoming a reader in his mind and in his mother's and father's minds. I had always attributed her success to the fact that all those little boys first fell in love with her and then learned to read in an attempt to woo her. It was not any sort of pure curriculum plan, that I knew. This teacher used self-selected trade books, basals, and core books in alternating 2-week segments across the year. Although she did collect running records daily on four or five students, the curriculum materials were eclectic and what children were reading depended on the week, the child, and the time of day. But she routinely and successfully created readers.

Many schools seem to have at least one of these miracle worker teachers and I will submit that if we knew more about these experts, we would find out that they would likely be successful at their mission if only provided chalk and chalkboard along with a room full of children. But we debate curriculum preferences.

Methodology

The irrelevancy of inquiry method, or perhaps the irrelevancy of *any particular* inquiry method, is driven home by the often complimentary findings of different studies conducted using a variety of methodologies. But we must look for commonalties across studies with differing methodologies in order to find the commonalities. Too often we read and talk only within those small, narrow methodological boxes that serve to constrain our research.

I do not think that anyone can deny the compellingness of studies as different as Heath's (1983) *Ways with Words;* Holland's (1975) *Five Readers Reading;* Barr & Dreeben's (1983) *How Schools Work;* Kozol's (1991) *Savage Inequalities;* Rutter, Maugham, Mortimore, and Ouston's (1979) *Fifteen Thousand Hours;* Hart and Risley's (1995) *Meaningful Differences;* Knapp's (1995) *Teaching for Meaning in High-Poverty Schools;* Snow, Barnes, Chandler, Goodman, and Hemphill's (1991) *Unfulfilled Expectations;* Mehan, Hartweck, and Meihls's (1986) *Handicapping the Handicapped;* and Purcell-Gates' (1995), *Other People's Words.* We can quibble about details of any of the variety of methodologies used but the findings of these studies are compelling and important regardless of the methodological stance displayed. But we debate methodological preferences rather than compellingness, consistency, and completeness of the inquiry effort.

But the best of the available studies, in my view, attempted to gather information broadly, deeply, and systematically in order to construct better understandings of some of the major issues confronting us. In that regard, such studies seem rather atypical of much of the research that is published. There seems to be a particular penchant in our journals for studies that address small, narrow questions. As a reviewer for a number of literacy research journals I see reviewers and editors that worry about manuscripts that are "too long" while simultaneously lamenting the "insufficient detail" provided concerning tasks, context, methods, and so on. It seems to me that the conventional wisdom about the general characteristics of "good" research papers literally works against publication of the sorts of studies that I think might prove useful in developing those needed for "plausible policies for the improvement of schooling."

So, where does this leave us as literacy researchers? I think that it is time to reexamine our inquiry. Time to question the questions we are posing. Time to step back and ask ourselves, "Where is all this research headed?" It is time to thoughtfully consider how our traditions might contribute to the current malaise.

Questioning Our Questions

We might ask ourselves some fundamental questions about the literacy research that is going on all around us. For instance, why is it we know so little about teachers who are enormously successful? Why do most teachers find it so difficult to create classrooms that foster the sorts of success these teachers have? Why do different communities create and support such different school organizational cultures? Why is public confidence in literacy research so low? Why are researchers constantly debating things that seem to hardly matter to the most expert teachers (and other actors as well)?

My hunch is that we would find commonalities in the literacy environments created by the most successful teachers but we will not find that commonality in the particular curriculum materials or even in particular literacy lesson plans, regardless of the methodology we use. I think we might find more of these teachers in certain communities and school organizations but I think we can find them almost anywhere right now. I think it is likely that certain communities, maybe even certain states, create, attract, or sustain larger numbers of such teachers but this seems to occur more by chance than policy or design.

Literacy research often seems to focus on topics and issues that I think play only peripheral roles in educational processes. To be more useful—more valuable, more influential—we will need to examine the nature of the research we do. We need to worry less, perhaps, about the particular methodological stance and more about the sorts of questions we pose and the scope of the information we gather. Perhaps it is time to attempt to foster more conversation about those things we can agree on—spend more time organizing the converging evidence, from methodologically and analytically different studies.

SOME THINGS THAT REALLY MATTER

I do think that there is a handful of things we know with some degree of surety about what really matters for literacy teaching and learning, and yet we often ignore these things that really matter in our debates and polemics.

We know that children need lots of opportunities to read and write. But many school organizational plans create such fragmented days and weeks that teachers find it difficult to find blocks of time for sustained reading or writing (Allington et al., 1996). We prepare the classroom teachers who routinely allocate more time to the "introducing of" and "following up on" stories and books than they allocate for reading those stories and books. Virtually all of us participate in the continuing perpetuation of special teachers and special programs (e.g., preparing reading teachers, special education teachers, speech therapists, art teachers, computer teachers, librarians, etc.) and yet these personnel and the programs they work in are often primary sources of the fragmentation of time and professional responsibilities and, thus, undermine the development of effective classroom literacy instruction (McGill-Franzen, 1994). But the pervasive influence of organizational schedules and professional specialization on the sorts of lessons that children experience is too often neglected in literacy research.

We know that children need easy access to an array of appropriate texts if we want them to actually engage in reading activity—books appropriate in complexity and interestingness (Adams, 1990; Clay, 1979). But one of the most depressing aspects of the recent work my colleagues and I have been doing is recognition of the substantial numbers of children who have literally no texts in their desks that they can actually read. Often these are children who participate in one of the special programs staffed by the special teachers we train. My best estimate is that 75% of the children who participate in remedial reading and learning disability programs rarely have access to books of an appropriate complexity. The situation is worse—a broader spectrum of children is included—if we add an "interestingness" criteria to the texts that are considered appropriate for these children and adolescents.

The notion of the importance of providing children with "appropriate" texts is hardly a new one, but the current evidence suggests we have largely failed in making access to appropriate texts an important universal in instructional design (Allington & McGill-Franzen, 1989). How is it that we can prepare classroom and special program teachers who so routinely ignore such a seemingly basic issue in effective instructional design? Why is it that it is so difficult to locate an individualized educational plan that provides useful and extensive information on the sorts of texts that would be appropriate for the child with a disability? Why do so few reading diagnoses ever contain such information? Why is it that classroom teachers typically have no access to the special program funds to purchase, for instance, the texts needed to enhance the instruction they are expected to offer the children in their classrooms who participate in these special programs? How is it that we can spend thousands of extra dollars per year on each child enrolled in special programs and still not, seemingly, be able to put books in their desks that they can read? How does our research address such issues?

We know that children need rich, coherent, and supportive strategy teaching—decoding strategies, organizational strategies, composing strategies, response strategies, and so on (Knapp, 1995). We know that different children need different kinds of useful strategy instruction at different times and in differing quantities and of differing intensity. Too often our research fails to recognize or acknowledge these complexities. Our research, instead, often seems to be searching for an instructional vaccine that will immunize all children against all sorts of instructional and environmental factors—a one-shot cure-all in which the same dose is given at the same time to all children. It is not just the media that is touting simplistic solutions that will once and for all solve the problems of literacy education.

We know that children need to develop a personal ownership of literacy—a recognition of the uses and power of literacy (Holland, 1975; Purcell-Gates, 1995). But this requires classrooms and lessons that foster ownership, investment, and engagement in literacy activity. Where is the research that might point to how such classrooms are created and sustained? The research that points to the sorts of state and district contexts that foster the development of such classrooms? Or points to strategies for gaining the support for these types of classrooms from the "worried public" that controls public education? Investigating such topics is neither easy nor currently much valued, but the importance to the improvement of public education cannot be denied.

We know that children need teachers who care about them deeply and believe in their educability (Ladson-Billings, 1994). But the fragmentation of professional responsibility through special programs and creation of deficiency labels for children who find learning to read difficult all work in not so subtle ways to convince teachers that some children cannot be expected to become readers and that it is someone else's job to foster their literacy development. Our teacher education programs often perpetuate both the fragmentation and reduced expectations. As researchers we can see these impacts, but only if we actually look at the broader picture of how schooling is organized—broadly enough to see our own complicity in the current organization.

School is life for children. But we seem much more concerned, generally, about children's achieving than their living. Rarely do we ever actually talk much to children or adolescents about their lives in school. In fact, we seem not to think much about how children view their lessons, their teachers, their literacy, or their lives in school. How do we research literacy acquisition without tapping into at least some of this? Or do we really believe that literacy can be reduced to a grade-equivalent score or percentile ranking on a standardized test? What should literacy lessons help children become? What should it help them understand?

We know that teachers need to be able to take risks, to think, and to talk with each other in order to develop their potential and sustain their professional growth (Duffy, 1997). But too little of our research has focused on these topics or incorporated such principles, and little of the professional preparation we offer establishes or fosters the professional support networks that might provide such opportunities for teachers.

There are a few things that really do matter in literacy teaching and learning in school settings. However, in my view, much of our professional conversation (including our argumentation) focuses on topics and issues that sit at the periphery. Unfortunately, much of our inquiry also sits at the periphery of literacy and public education—all the while many good educational efforts are maligned and misrepresented, often supported by the malevolent misinterpretation, misrepresentation, or misapplication of someone's "research."

SUMMARY

Literacy research is generally too narrowly conceived, too local, and too parochial. The questions, topics, and issues that must be addressed in literacy research are difficult. If we continue to focus our research primarily on the small, the manageable, the

local, and the familiar questions, it is unlikely that our work will ever foster the development of the "plausible policies" that Tyack and Cuban (1995) argued for, nor will our work likely ever engender the confidence of the "worried public"—the other actors in public education. We need to break out of the small, narrow box that constrains most of our research and much of our conversation.

We cannot simply bemoan the naiveté of the citizenry, media, legislators, boards members, or school administrators without asking hard questions about our own work and its relevance to the condition of education today. I am reasonably sure that we have few good answers to many of the problems of public education—that few of us, for instance, have anything truly useful to say to the New York City, Dallas, Miami, or Detroit Boards of Education about what needs to be done to enhance literacy development in these American urban educational systems. Few, if any, of us have "plausible policies" that might be implemented tomorrow to reliably enhance the quality of literacy lessons in, say, the 800 or so elementary schools in the Los Angeles Unified School District—given the real world constraints that other actors operate within.

I believe, however, that the crises in those school systems, and others around the country, are not fundamentally crises of curriculum, nor are they crises of research methodology per se. There is a host of other issues that need far more inquiry and reflection than what curriculum materials should be advocated, and there are useful roles for a variety of methods of inquiry in examining questions and issues. But we relentlessly debate curriculum and methods of inquiry.

We cannot continue to ignore the agendas (and constraints) of the other actors in public education. Unless we work harder to better understand these agendas, our work will have little impact on the course of educational change. Unless we improve upon the fleeting local knowledge that seems to dominate our inquiry and our professional conversations, I do not see how our work can be very useful to those in power who must develop the plausible policies that will gain the confidence of the public and the teachers.

If literacy research and literacy researchers are to become more often relevant outside this small box we call National Reading Conference, we will need to engage in some careful considerations of our work and a fair amount of rich, sustained, and civil conversations amongst ourselves and more sustained listening to others both inside and outside this box. Without such efforts, I fear a continued marginalization of our work to those who have the power to alter the conditions of education—locally and nationally.

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