Integrating Educational Research and Practice:

Reconceptualizing Goals and Policies: “How to make what works, work for us?”

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Introduction

This article proposes reconceptualizing most current education research programs to more effectively promote the integrated co-development of scholarship, practice, and policy. In our analysis, we explicate the strategies underlying exemplary funding programs such as the National Science Foundation's (NSF) "Research on Learning and Education" (ROLE)[2], and the federal Interagency Educational Research Initiative (IERI)[3], co-sponsored by NSF, the U.S. Department of Education (DoED), and the National Institutes of Health (NIH). From this analysis, we draw implications for the further evolution of funding strategies that emphasize a scholarship *of* practice rather than scholarship *on* practice.

This perspective on research funding and policy is based in part on our experiences while Senior Program Officers in the Directorate for Education and Human Resources of the National Science Foundation, where we helped conceptualize and direct a number of peer-reviewed funding programs on education research in science, mathematics, and technology. Our collective experience includes work on instruction and policy, learning and intelligent systems, applications of advanced technologies, networking infrastructures in education, and more recently what can be termed "the sciences of learning."

There is general agreement that effective practice is based on the reflective application and adaptation of research. In turn, practice that includes principled experimentation can raise critical questions to be answered by research. This feedback loop can create a sustainable strategy for innovation in the nation's education enterprise. However, a recent NRC report[4] on education research observed that transfer of scholarship into practice is "a last frontier," despite the fact that the application of research is paramount for moving education reform from transitory fads to proven strategies. Similar concerns were voiced earlier in the 1997 PCAST[5] report on the use of research in learning technologies to strengthen U.S. education. Both these reports cite an urgent need for researchers to foster a transformative school culture centered on sustainable, scaleable, high quality educational practices of value to all students

This article is not a synthesis of the extensive prior literature on the need to link scholarship to practice as a means of reducing "the awful reputation of education research."[6] We presuppose such a perspective as a foundation for our thinking.[7,8] To empower such a shift in the nature of scholarship, we argue for a reprioritization of research policy and funding initiatives, highlighting crucial types of education studies currently undersupported. Studies that emphasize integrated co-development by researchers and practitioners have special requirements for extraordinary size, length, and appropriate capacity in the field to conduct them. Unless mechanisms for providing targeted resources are specifically incorporated into the redesign of current programs and policies, the stringent conditions for success in these types of projects make sustained support at necessary levels unlikely.

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[2] Role: http://www.nsf.gov/pubs/2000/nsf0017/nsf0017.pdf

[3] IERI; http://www.nsf.gov/pubs/2001/nsf0192/nsf0192.pdf

[4] *How People Learn: Brain, Mind, Experience, and School,* Bransford, Brown and Cocking, editors. National Academy press, Washington, DC 1999

[5] President's Committee of Advisors in Science and Technology, Report to the President in the use of Technology to Strengthen k-12 Education in the United States, OSTP, March 1997

[6] Kaestle, C. (1993). The awful reputation of educational research, *Educational Researcher*, 22 (1), 23-31. Kaestle, C. (1997). Improving the awful reputation of educational research, *Educational Researcher,* 26(7), 26-28.

[7] See, for example, the literature listed in the companion volume to How People Learn: *Bridging Research and Practice,* Donovan, Bransford and Pellegrino, Editors. National Academy Press, Washington, DC 1999

[8] School Reform and Research in Educational Psychology. Ronald W. Marx, Guest Editor; Special Issue of *Educational Psychologist*; Vol. 35, 2000.

The Importance of Moving to a Scholarship *of* Practice

The strategy we advocate for increasing the impact of research on education practice goes beyond "transfer" and "action research" towards reconceptualizing the relationship between scholarship *and* practice as instead a scholarship *of* practice. An analogy for understanding a "scholarship of practice" is to consider the levels of experimentation and applied research that take place *after* scientific research in the physical sciences is conducted and published, and *before* the results of this research are applied large-scale in society. The field of education does not provide roles akin to engineering for developing research prototypes into robust practices and products. As discussed later, the outcomes of research include people, not just knowledge, and transfer between research and practice is implemented through both scholarly products and human capacity-building.

Stokes' recent book[9] and related policy papers[10] reinforce this perspective by presenting models for relating fundamental and targeted research, strategies largely absent in educational scholarship. These include the *use-driven* research model successfully applied by NIH to simultaneously:

- provide resources for fundamental biological research and its medical public health applications,
- resolve persistent problems in medical practice through the adaptation of research findings, and
- develop public and political support for allocating resources to both basic and applied research.

Stokes argues convincingly for the Pasteur (or NIH) model, in which research on use-driven, applied questions demands and fosters studies of fundamental scientific problems associated with that practice. In fact, "use-inspired" basic research has the same quest for fundamental understanding that is present in "pure" basic research, associated in Stokes's analysis with Bohr's work as the defining paradigm. In contrast, in Stokes's analysis Edison's work is shown as driven solely by considerations of use, without a concomitant quest for fundamental understanding.

Providing a complementary perspective, Branscomb[11] and others[12] have reconsidered the reasons for public support of research, given the transformations that have taken place in the production and consumption of knowledge outside traditional venues such as universities, think tanks, and industry. This line of argument leads to a definition of modes of research that distinguishes between "Newtonian" or pure basic science, "Baconian" or pure applied science, and "Jeffersonian" science where considerations of use drive the quest for fundamental understanding. Both Pasteur's approach and Jeffersonian science ideas differ from limiting consideration of research to basic vs. applied, and converge instead on a thrid way of using application-driven issues to organize and prioritize the questions behind researchers' quest for fundamental understanding.[13]

These two formulations make clear the importance of new types of mechanisms for tying fundamental research to practice in the field of education. For example, the NIH strategy of incorporating use-driven research perspectives into the definition of clinical research and research trials has much to offer our distributed and fragmented education system, including potentially

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9 *Pasteur's Quadran*t, Donald E. Stokes, Brookings Institution Press, Washington DC, 1997

10 *Rethinking What Research Government Should Fund: A Vision of Jeffersonian Science*; Gearld Holton and Gerhard Sonnert; http://www.nap.edu/issues/16.1/holton.htm.

11 *Science for Society: Cutting-Edge Basic Research in the Service of Public Objectives: A Blueprint for an Intellectually Bold and Socially Beneficial Science Policy* , Lewis Branscomb, Gerald Holton, Gerhard Sonnert; May 2001. Report on the November 2000 Conference on Basic Research in the Service of Public Objectives

12 *Higher Education Relevance in the 21st century;* Michael Gibbons, http://www.worldbank.org/html/extdr/educ/edu-pb/giboeng3.pdf

13 *The False Dichotomy: Scientific Creativity and Utility.* Lewis Branscomb: http://www.nap.edu/issues/16.1/branscomb.htm

shortening the timeframe in which education research can have real and sustained impact in schools. NIH has recently revised its policies for funding medical and public health research on the basis of its past successes in medical research, coupled with failures in using similar strategies in advancing the public health aspects of medicine.[14] These changes highlight the parallels between education and the public health aspects of medical research, rather than medical research per se. In developing better methods of applying research in schooling, education scholars should consider how and why existing models for implementing research in practice outside of education succeed (or fail).

Beyond reformulating models of research application, the underfunding of educational research compared to other fields' investments in scholarship results in a dearth of research-minded practitioners who join the instructional workforce in its many guises. This in turn undercuts a generative, field-driven demand for quality research that speaks to practice. Allocating a greater proportion of educational expenditures to research, as well as increasing the research preparation of practitioners, would contribute to solving many problems of capacity in education that have been discussed lately, including the need for a higher quality instructional workforce and for instructional leadership in the school's administrative hierarchy.

What is required from research-minded practitioners is not "action research" along the lines of academic research carried out in classrooms. Rather, it is the more profound experimental ethos of (and support for) data-driven iterative assessment and revision of classroom practice by practitioners with the collaboration of researchers. Practice-minded researchers need concomitant recognition of the new ways in which the expertise of researchers and practitioners interact. The practice of education is always localized to the interactions of a particular student or group of students, with a particular instructor, and within a particular environmental context. Any general conclusions and recommendations of research need to be adapted to specific local conditions and capacity before they can be expected to show results.

Principled localization of general research knowledge requires more than manuals, textbooks, and a vision of what "good practice" ought to be. Localization requires the ongoing hard work of thinking through options and evaluating them, based on very specific sets of conditions and resources, to arrive at a *path* for action. Excellence is not achieved in one jump; reflective modification of this path is the "experimentation" that leads to optimizing gains. Partnerships such as those advocated by the LETUS group[15] and by Confrey et al.[16] are centered on the content and pedagogy of specific instructional activities and have stringent requirements for the preparation of researchers and practitioners. Graduates of these learning partnerships are able to experiment on demand within specific classroom situations, simultaneously gaining understanding from such experiments and revising them to obtain increased student learning.

Education Studies as a Network of Research Problems and Methodologies.

Education research problems and methodologies span many disciplines, whose goals and methods need co-adaptation to effectively address the complex components of educational practice. This adaptation calls for the nurturing of communities of researchers that share language, methodologies, and goals across disciplinary boundaries—the only way to avoid "a bridge too far."[17] Two examples will suffice to highlight the possible gains to practice of research that purposely integrates this breadth of topics. One is the work of Huttenlocher, Gentner, Newcombe

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[14] The Office of Behavioral and Social Sciences Research was created in the Office of the Director, NIH, in 1995. see http://obssr.od.nih.gov/about.html. See also *Bridging Science and Service, A Report by the National Advisory Mental Health Council's Clinical Treatment and Services Research Workgroup,* NIMH, http://www.nimh.nih.gov/research/bridge.htm.

[15] http://www.letus.org/aboutus.htm.

[16] http://www.edb.utexas.edu/syrce

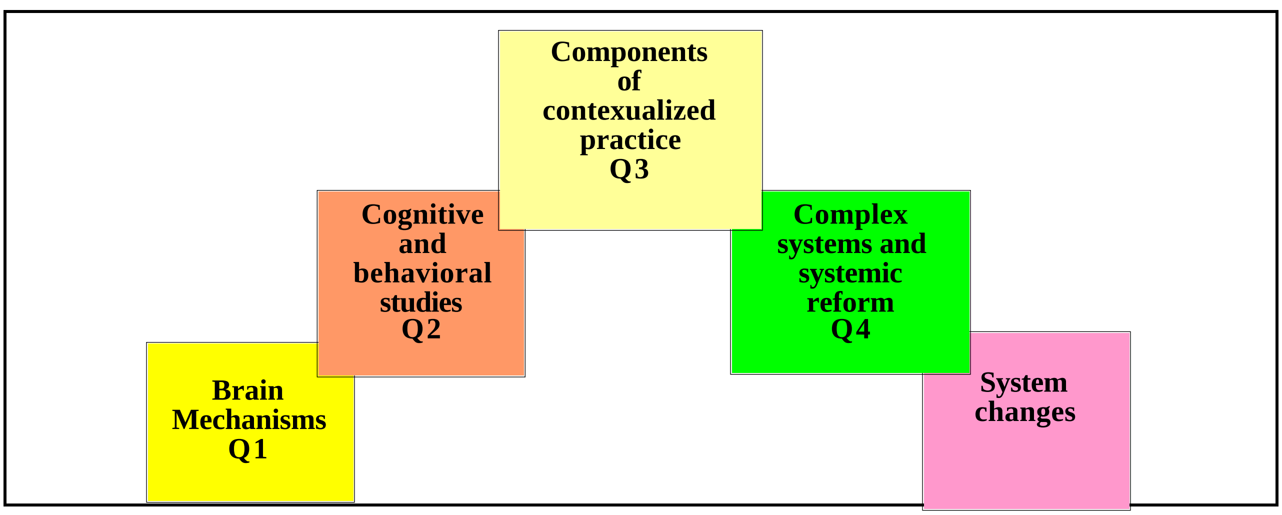
[17] *Education and the brain: A bridge too far.* Bruer, J.T. (1997, November). Educational Researcher, 4—16.

et al. on understanding and fostering spatial competence.[18] Another is implicit in the work of Dehaene[19] on the neural mechanisms that underlie counting and estimating, and the related work of Gelman and collaborators[20] on the interaction between the structure of the brain's learning mechanisms and the structure of the data that support learning.

Figure 1 is one view of the different aspects (quadrants) in NSF's formulation of education research and includes a placeholder for the "system changes" research to be discussed later. The use of the word "network" in this context deserves an explanation. We want to avoid any implication of a linear continuum between research and practice, or of a continuum of granularity levels from individual to social. Every quadrant has it own set of granularities, from small to large, and every quadrant may include aspects of practice. There is a qualitative change of focus in bridging quadrants. This change redefines the conceptual scales, and it is this redefinition that encourages posing questions from different perspectives. Redefined questions demand a rethinking of the methodologies used to answer them, and offer the possibility of new insights. Thus, the *network of quadrants* should reflect the limits of a reductionist approach to studying the complex nature of education.

The quadrant labeled "Components of contextualized practice" provides the *use-driven* problems for research to consider, stemming from issues in curriculum, pedagogy, assessment, professional development, etc. Other quadrants represent various types of research that contribute in different ways to understanding and improving practice. These include studies of individual learning at biological scales, of social learning by individuals and groups, of the organization of the educational system, and of policy and economic issues. These areas of research run the gamut from immediate usefulness in practice to setting the stage for advances a decade hence, and from individual phenomena to organizational dynamics—i.e. along axes of both time and aggregation.

Each quadrant receives outside inputs (methodological, problematic) from less *education-problem driven* aspects of contributing disciplines. Besides framing disciplinary aspects of research in terms of their contributions to practice, such a conceptual framework highlights the importance of the *interfaces between quadrants* as areas where research is needed and where the nurturing of cross-disciplinary communities of scholarship is of paramount importance.



[18] *Understanding and Teaching Spatial Competence*. J. Huttenlocher , D. Gentner, N. Newcombe https://www.fastlane.nsf.gov/servlet/showaward?award=0087516 (and prior awards)

[19] *The Number Sense: How the Mind Creates Mathematics.* Stanislas Dehaene; Oxford Univ Press 1997.

[20] *Learning in Complex Environments by Natural and Artificial Systems*. R. Gelman, C. Taylor, E. Stabler, O. Chapman, C. Gallistel. https://www.fastlane.nsf.gov/servlet/showaward?award=9720410 . *The Children Understanding of Number*. R. Gelman and C. Gallistel, Harvard University Press, 1978.

Figure 1. *Reification of the concept of a continuum of use-driven education research.*

Quadrant 4 in Fig. 1 is the home within NSF's portfolio for studies that explore educational systemic issues. The relationship between this quadrant, the IERI program, and the types of research we advocate for is shown in Fig. 3 where the placeholder for "system changes" shown in Fig. 2 is analyzed. The IERI program represents an extension of implementation research into the conditions for scalability and sustainability. "Sustainable localized system changes" represents the type of scholarship of practice that we advocate.

In our strategy (see Fig. 2), we differentiate research on separate aspects of education practice (e.g., the cognition of teaching and learning), from studies of these components embedded into a sustainable implementation in real contexts (policy and practice research on education innovation). The middle box in Fig. 2 is crucial, because this type of research bridges and connects advances in isolated portions of educational practice with an understanding of the policy and systemic actions that promote or hinder their classroom applications in a scalable and sustainable way[21,22] Illustrative examples of research that falls into this middle box include:

- creating strategies for the sustainable, large-scale adoption of experimental learning technology prototypes into educational practice[23]
- using statistical data for assessing student progress to provide professional development on content-specific pedagogy and teacher proficiency [24]
- understanding how to foster the conditions for productive participation of novice learners in scientific research.[25]

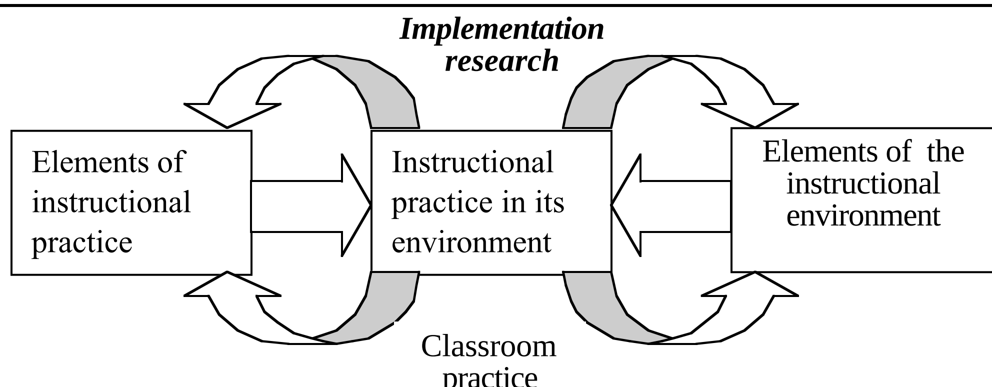


Figure 2. *Type of Research Studi* *n the Practice of Education*

The three bullets illustrate non-exclusive aspects of how the term "implementation

[21] *Systemic Crossfire: What Implementation Research Reveals about Urban Reform in Mathematics*. J. Confrey, K. Bell, D. Carrejo, unpublished (AERA 2000 presentation).

[22] *Systemic Educational Change: Towards A Complex Systems Perspective.* J. Lemke et al. Complexity and K-16 Education: Working Group 3 Draft Report, http://www.necsi.org/events/cxedk16/edreform.html

[23] *The Living Curriculum Project.* L. Gomez et al. https://www.fastlane.nsf.gov/servlet/showaward?award=9720423

[24] Work with Fathom and Student TAAS Data. *Proposal for a Systemic Research and Design Center in Mathematics, Science, and Engineering Education*; Confrey et al. https://www.fastlane.nsf.gov/servlet/showaward?award=9816023

[25] *Center for Highly Interactive Computing in Education*. http://kidsideas.org/investigationstation

research" has been used by the groups referenced. The parallel interests of implementation research are (a) to contribute to sustainable and optimal improvements in practice *in a given environment*, *responsive to that environment*, and (b) to treat the process as a local experiment where researchers and practitioners can support and reflect on the development *of the site* as a "learning organization." By "sustainable and optimal" we mean research that succeeds in raising the expectations and practices of the implementation site to match the maximum gains that could be achieved under given conditions. To achieve sustainability, research must help raise the local capacity for negotiating with policy makers the timeframes for achieving expectations, and then engage in continuing to raise these same expectations.

Such work must take as its bases a consideration of local conditions *at that implementation site*, the development of human and infrastructural capacity for principled improvement *at that implementation site*, and the uncovering of a path for continued organizational learning *at that implementation site*. Often, at that site this research strategy requires a combination of design experiments, the creation of ongoing support relationships among researchers and educators, and the need for ongoing research on locally posed questions. This integrated type of system-embedded research is what we call implementation research. It requires collaborative teams of researchers and practitioners involved in co-design and co-analysis, as well as the use of experts outside of education, such as social psychologists, complex systems theorists, organizational researchers, policy analysts, and economists. In these research partnerships, providing support mechanisms for participating practitioners to learn the language, methods, and culture of research is essential.

In this situation, the implementation site experiences individual and organizational learning from its own attempts—via an ethos of experimentation. The operational term for describing the relation between research and practice is not for us "transfer" or even "integration"; rather, it is "integrated co-development." The next section discusses systemic research funding of such large-scale intervention experiments.

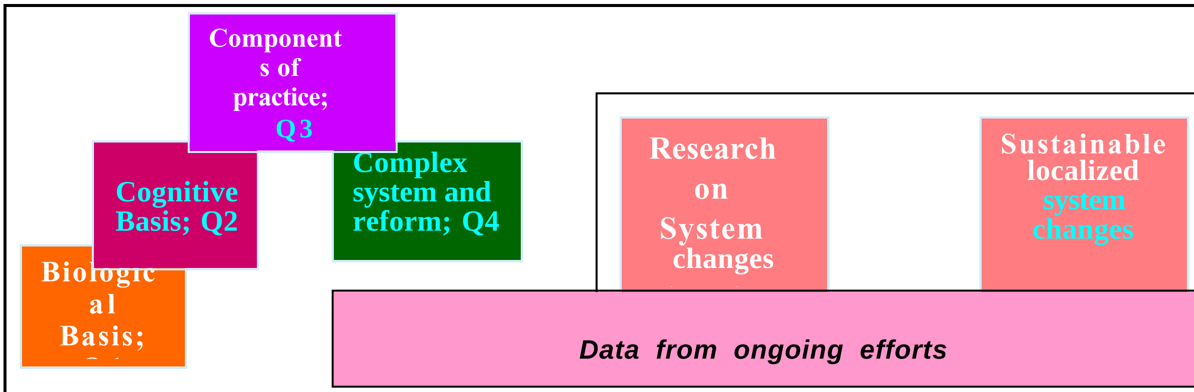


Figure 3.- Extension of the continuum research model to integrate localization research.

Implementation Studies That Extend Current Research Methodologies[26]

Reconceptualizing research priorities and processes to focus more on implementation studies mutually developed by scholars, practitioners, and policy makers is a promising strategy to develop sustainable impacts on practice. This agenda can be seen as driven by a proactive view of the role of the school system (which is the target institution) and is reflected in the NSF-managed Interagency Education Research Initiative, a collaboration of NSF, DoED Office of Educational Research and Improvement (OERI) and National Institute of Child Health and Human Development (NICHD).

IERI is based on the belief that too little emphasis has been placed on funding for *in-situ* adapting, analyzing, and scaling-up interventions and policies that, as isolated islands of innovation, have been successful in some educational contexts. We believe that even less priority has been given to modeling and generalizing the coherent processes that led these innovations to succeed in design experiment settings and other implementation venues. To achieve sustainable and optimal change, we posit that understanding the process of innovation by teachers and administrators (i.e., how they alter their standard practices) is as important as studying the outcomes and the instructional practices that we wish others to replicate. Such knowledge can not be obtained using a dissemination strategy. Dissemination implies that the changes that take place in new sites will be minor or non-existant. We know that that is seldom the case, and that critical aspects of innovations are most often co-opted when performed in isolation from their bases on research. In essence, we are saying that innovations studied by researchers succeed because of the special flexibility provided by the presence of researchers. We need to develop strategies for reproducing not only the innovation itself, but also the environment that led to its success.

Table I differentiates between types of classroom studies and the research questions that we believe should drive them, and is an attempt to place the implementation research in an ecology of studies that bridge traditional research and practice,

Certainly, this type of research on the sustainability and scaling-up of reforms is expensive, people-intensive, and time consuming; but these are not the only reasons why such studies are seldom done[27]. Implementation (systemic, applied) research does not fit well within conventional scholarly academic career paths. These studies demand a multiplicity of expertise and of theoretical and methodological perspectives; this type of scholarship also requires researchers to share control of the investigative process with practitioners and policy makers. When such close partnerships are in place, researchers must not only relinquish sole power over the analytic process, but also act as brokers to guide and mediate the reflective interactions of other stakeholders. This is a skill for which most investigators are not prepared—in part because conceptual frameworks for this type of research are not well developed, in part because this capability is best fostered through supporting changes in practice within integrated implementation and research “testbeds” (an uncommon experience for scholars), and in part because such goals and flexibility are seldom allowed by funding agencies.

As has been suggested by the President’s Committee of Advisors in Science and Technology (see ref. 5), if our ultimate goal is long-term, pervasive, quality educational improvement, we must find ways to invest a “critical mass” of funds and human resources in *clinical-type research*. This article argues for a definition of such research as reflective interplay between basic research and practice, a process that is bi-directional and helps both sides evolve towards increasingly sophisticated objectives along the lines of recent redefinitions of the compact

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[26] We foresee that systems and complexity theory frameworks will provide important quadrant-specific insights and questions across the continuum (see ref. 22). For such theoretical insights to be validated into theories of action, the recent availability of data from existing interventions, though obtained under varying conditions and not aggregated, offers information of potential value if funding were provided for this type of analysis.

[27] See footnotes 21-25.

between research and the public. [3,4]

Table 1.- Different Styles of Classroom Research on Innovation and Practice.			
Term	Definition	Characteristics	Question
Innovation	A new curriculum, technology, material, etc. and associated pedagogy	May include isolated classroom studies	Is this an useful pedagogy?
Intervention	The use of that innovation in one or more regular classrooms	My include outside evaluation	Is this pedagogy up to being disseminated?
Intervention Study	Interventions are always experiments, but not always treated as such	Includes extensive evaluations	Does the pedagogy work in new environments?
Implementation Research	The study of mutual impacts of the innovation and the intervention	Ongoing work by the site and by the research team	How can the site learn about the nature of pedagogy from the study?
Field tests (called clinical studies in medicine and public health)	The aggregation of outcomes from multiple intervention research studies	Requires a common framework	What is the range of applicability of the pedagogy?

In such a relationship, innovation is not followed by dissemination: the expected acceptance of recipes and materials for innovation developed by others. Innovation is instead the reflective adaptation or recreation of a process that enabled a similar group to succeed in another educational setting. Scholarship needs to provide validated examples of processes carried out under similar circumstances. Local innovation will then take these processes and chart a path from their own current capacity to a similar but evolving goal. Focusing on the process as well as on outcomes enables practitioners to start with objectives consistent with their own current problems and worldview, then evolve towards increasingly more powerful goals as they reflectively adapt innovations and engage with scholars in answering research questions needed for principled adaptation.

In our view of research, the conventional intent of large-scale research endeavors to achieve an expected outcome instead shifts to sustained planning for continual, reflective evolution. In our view, even the best of predetermined goals too often leads to implementations that call for either locally unattainable or unimportant outcomes, or worse, to the co-opting of the original goals to make them locally viable. Evolving objectives, on the other hand, require developing a shared, long-term vision of what excellence in education means.

For example, one of the authors is a co-investigator in a five year IERI project studying how the systematic usage of scientific models in the high school curriculum shapes students' cumulative motivation and understanding in science, as well as learners' generic understanding of models. The co-design and co-analysis by participating practitioners of our educational interventions is essential for enabling the large-scale integration of alternative curriculum. This involvement can lead to a sustained process of innovation in science education that persists after our specific curricular studies are finished.

Some central questions in a process of mutually conducted implementation research are: What are the critical insights needed *here and now* to plan for and achieve an educational system's long-term goals? What is the existing knowledge base in a particular situation, and where are the pressure points in that context for augmenting that knowledge? What types of intermediaries can aid or subvert the institutionalization of an ongoing relationship between practice, policy making, and basic, applied, and systemic research? Current research funding programs seldom take these kinds of questions into account in the criteria they use for peer-reviewed allocation of resources.

Limitations on Research Imposed by the Current Programmatic Context

Barriers to this formulation of research exist not only in the scholarly community, but also among policy makers and practitioners. Traditionally, the primary goal of the latter groups has been to efficiently follow standard operating procedures, modifying these only when current learning outcomes fall dramatically short of society's needs and expectations. In the event that innovations are necessary to remediate shortfalls, the interventions attempted are seldom based on prior knowledge, on validated experience, or on research evidence; but instead often emerge from political expediency or currently popular strategies for change—silver bullets.

Intervention projects seldom monitor relevant research and generally limit their goals to "fine-tuning" current approaches rather than searching for more ambitious visions. Too often, practitioners and policy makers reject as "impractical" a reflective, iterative approach to implementation, thereby undercutting the leverage research might provide in developing strategic objectives and effective evolutionary processes for improvement. One should remember, though, that what is impractical is a function of resources of time, funds, and knowledge available, not of an inherent lack of appropriateness.

As discussed earlier, funding implementation research is one means to introduce alternative, reflective innovation strategies into a large complex context. However, in order to succeed, integrated co-development research must be based on insights from design experiments and must focus on reflective adaptation to given contexts. Unfortunately, in practice this is not often the case; the balance between "general," "generalizable," and "localized" knowledge acquired from educational interventions is usually not made clear in reports on innovation effectiveness because it is not clear in the negotiations between research and intervention goals. In part, this is because the demand of practitioners and policy makers for immediate results washes out the development of general ideas, and forces researchers to generalize ideas too early in the process of implementation and it thus often limits them to rediscover what is already known in a different context—what should instead be only a required to calibrate outcomes with prior work and with the literature. . The studies we need should document the complete long-term process, including "failures" that are in fact only explorations critical to the process of optimizing an adaptation.

Interventions mandated by local boards, politicians, or the public are typically not viewed as *applied* experiments that require iterative refinements to match a particular situation. Rather, they are seen as "silver bullets" that will magically solve a pressing problem. The question too frequently asked (*what works*) is inferior to the perspective *how can we make what works, work best for us*?

A variety of problems plague attempts by educators to initiate research-based integrated co-development studies. Performers and audiences are often different for laboratory research and implementation studies; in the absence of common participants, building connections among these types of experiments is difficult. Organizations that contribute to educational inertia, such as standardized testing corporations and textbook companies, are seldom included in our models of change; yet involving these types of stakeholders in systemic innovation efforts is often vital to success. These and other disconnects among basic research, implementation studies, and policy-making hamper aggregating the outcomes of experiments towards larger goals of systemic reform and undercut the ownership of change by those who must implement innovations.

Implications for the Nature of the Demands on Research Made by Practice.

In the previous sections we presented a perspective on the special demands made on scholarship by research on sustainable change in practice. We turn our attention now to the implication of these demands for capacity in both research and practice. We argue that researcher and practitioner communities need to be prepared for a deeper understanding of their own roles in experimentation and experimental research.

Since a compelling need exists for more clinical and implementation research, the educational community must undertake the task of capacity building for a new generation of applied researchers and research-informed practitioners. At present, most scholars know better how to perform laboratory analyses than how to conduct large-scale implementation studies as theory-building research (rather than evaluation). Most investigators do not have good mental and methodological models of intervention and systemic research, which are similar to applied scholarly activities in medicine and engineering where the relation between general knowledge and local and individual interventions is crucial. In the absence of sophisticated implementation strategies, practitioners and policy makers are skeptical of what they see as "ivory tower" remedies and often inadvertently eschew or co-opt the core of innovations in the process of adapting them. Funding agencies must provide greatly increased support for the evolution of innovative, mutually evolved methodologies for this type of research.

Another barrier to integrated co-development research is that teachers are too often viewed as tools to implement improvements in students' learning designed by others. Such views are often a realistic appraisal of the current situation, but the field should have in mind—and work towards—the development of greater partnership roles for teachers as co-experts, particularly in areas where teacher knowledge of content and pedagogy are crucial determinants of student success. Terms such as "teacher enhancement" illustrate a perspective that views knowledge about educational practice as a one-way flow from experts and researchers to teachers—a linear perspective on practitioners increasingly called into question in all areas of research, not only education. Absent a supportive relationship in which the educational research community acknowledges and helps build the capacity of teachers and administrators as professionals with insights to offer, and educators' institutions as learning organizations that shape the evolution of practice, those in the field have few inducements to incorporate and sustain research-based improvements that reshape standard operating procedures.

Lagemann[28] analyzed the history of educational research from the very useful perspective of how the professionalization of this field has influenced linking knowledge and action in education. As a complementary perspective, limits on the professionalization of practitioners in U.S. education (i.e., not supporting the development of their capacity for independent judgment and knowledge of their subject matter and its pedagogy, not providing time for their reflection and sharing ideas with colleagues) hamper linking knowledge and action. Comparisons of the role of teachers in the U.S. and other countries—or of the role of teachers relative to practitioners in other fields such as medicine and engineering—illuminate how underestimating the professionalism of U.S. educational practitioners in turn undercuts the sustainability and quality of systemic educational reform, and of the education system itself.

Beyond these problems of disconnects among basic research, applied research, and policy—as well as between researchers and practitioners—the value of innovations is too often reduced to and measured only by immediate improvements on standardized tests of student performance. This focus, though useful within its limits, provides too narrow a framework for evaluating the gains, successes and shortfalls of educational innovation's process and outcomes. When taken as the *only* measure of worth—and this is how most discussions frame this issue—evaluation based on these limited measures does not provide the public and policy makers with

[28] Lagemann, E.C. (1997). Contested terrain: A history of education and the United States, 1890-1990. *Educational Researcher*, 26(9), 5-17.

enough knowledge for a continued progression of thoughtful innovations. Sustaining a coherent strategy for *higher-level* student learning to meet the long-term needs of society then becomes very difficult. For preparing students to compete in a knowledge-based global marketplace, an all-or-nothing improvement strategy that is both focused on immediate results and designed for purposes of ranking rather than formatively enhancing learning is self-defeating.

Change is a process, not an outcome; a "one shot" improvement implies that the environment that nurtures the current failure is left unchanged. Funding initiatives should explore ways in which measures currently in use can gauge the directions and pace in which schools are moving and can change to provide a constructive, formative relation among research, policy, and practice. Such funding can aid in developing shared conceptions of educational reform that allow practitioners and researchers to assess their continual individual and collective progress based on a dynamic view of the educational system, in which standardized measures of success are only one part of the assessment strategy.[29]

Research that Fosters Systemic Partnerships for Innovation

The impetus for these changes in the culture of educational innovation must initially come from the research community. But research can play a significant role in sustainable educational innovation only by developing and testing hypotheses in the challenging real world settings where implementation problems surface. Funders and researchers must then ensure that the formulation, testing, and modification of hypotheses and designs from their inception include practitioners' perspectives and knowledge. For example, educational research in science and mathematics has gradually moved closer to enactment in contexts of authentic instructional practice. This field has progressed from laboratory studies and mass testing to detailed research in classrooms and careful analysis of students' conceptual learning. We call for expanding this trend across disciplinary areas by giving much higher priority to research on whole systems of educational practice, including schools, families, and mass media. Such research is currently rare, for all the reasons described earlier, but urgently needed.[30]

To enable conducting the mutually developed, coherent implementation studies suggested earlier as a crucial strategy for educational improvement, the scholarly community and its funders must become more reflective and self-critical about current processes and goals of educational research. Studies of education are similar in some (not all) ways to studies in the sciences, social sciences, engineering, and mathematics. Analogous to research in engineering and the social sciences, educational studies involve developing knowledge about designed, human contexts less constant in their attributes than natural phenomena. As Salomon[31] describes, educational reform is a complex, multifaceted enterprise similar to aircraft design as an activity that must embrace complexity in order to reach a solution. The situations studied by educational researchers can be seen as complex systems with sophisticated feedback and non-linear causality, similar to biological or ecological systems. Such systems can not be understood by considering pieces of the whole, and can therefore benefit from integrated system research strategies (whose development should be supported more strongly by funding agencies). Beyond independent scholarship, educational researchers also should play a role as intermediaries who enable experts in other disciplines, educational practitioners, learners, funders, and policy makers to understand each other's views of

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[29] An often cited example is the Union City, New Jersey, School District. See Honey, Margaret, Fred Carrigg, and Jan Hawkins (1998). Union City Online: An architecture for Networking and Reform, in C.Dede˚ (Ed.) *Association for Supervision and Curriculum Development 1998 Yearbook: Learning with Technology*, p.121-139, Alexandria VA, ASCD. Also Carrigg, Fred (2001). *Perspectives in Technology and Education Research: Lessons from the Past and Present,* NRC Symposium on Improving Learning with Technology, January 24-25, 2001, case study draft. It is also instructive to look at the examples cited by the press, such as the Schools of the Year Report in TIME Magazine, May 21, 2001.

[30] The authors thank Marcia Linn for contributing this comment.

[31] Salomon, G. (1991). Transcending the qualitative/ quantitative debate: The analytic and systemic approaches to educational research. *Educational Researcher,.* 20 (6), 10-18.

these complex-system perspectives.

As the field of education changes, the types of research requested and needed alter. In the 1980s, societal concern about educational outcomes led to a variety of descriptive studies designed to assess and understand problems in performance. At the start of the millennium, now that the causes underlying educational dysfunctions are better understood, practitioners and policy makers are asking researchers to focus on applied larger studies that improve practice in a sustainable, affordable, and scaleable manner. But this demand in itself, without a parallel recognition of what resources such long-term studies require, will not provide answers. Requests from the field for research results that inform practice have thus far resulted primarily in evaluative studies that provide limited evidence on whether current educational interventions are worth the cost and trouble involved in implementation. We believe these questions should instead be answered through research funding focused directly on implementing organizational and systemic studies that optimize investments in innovation and increase the quality and depth of students' learning outcomes.

In response to all these needs, we believe that a balanced portfolio of educational research would include greater emphasis than funders currently provide for these types of initiatives:

- developing a broader base of researchers and scholars capable of studying the complex situations characteristics of large-scale educational innovation in complex environments;
- building partnerships with practitioners and policy makers in the design and analysis of mutually developed implementation studies;
- creating new types of research methodologies to address significant problems of practitioners and policy makers at appropriate levels of aggregation;
- studying evolutionary conceptual frameworks based both on current challenges in educational practice and on emerging opportunities from potential innovations in technology or theory.
- developing models of innovation based on systems thinking rather than reductionist approaches to understanding the interrelationships between an educational intervention and the other processes occurring in its organizational context.
- undertaking assertive and proactive dissemination strategies that both inform a broad base of researchers, practitioners, and policy makers and provide support for those interested in adopting or extending the innovation.

Not all of these criteria are appropriate for every type of educational research, but they suggest a strategy that could be used when making competitive judgments about what scholarship to propose or what funding decisions lead to an effective research portfolio.

Conclusion

Historically, the relationship between researchers and practitioners has taken various forms, some quite close and supportive.[32,33] However, with few exceptions, researchers' strategies for inducing change in educational practice have emphasized the transfer to teaching or to policy making of results and insights conceptualized by the scholarly community. Funded projects have been driven mainly by goals of contributing to the accumulation of scholarly knowledge; disseminating this knowledge to practitioners as materials, directives, or rules has been seen as a secondary responsibility of the investigators. To enable more scaleable and sustainable research innovations, we suggest reprioritizing funding strategies to highlight innovative types of studies that empower the improvement of practice in pervasive ways. Because of these and related problems,

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[32] Kennedy, M. (1997). The connection between research and practice, *Educational Researcher*, 26(7), 4-12.
[33] Wagner, J. (1997). The unavoidable intervention of educational research: A framework for reconsidering researcher-practitioner cooperation, *Educational Researcher*, 26(7), 13-22.

insights from current research may provide valid new ideas for improvement in practice, but seldom make a general and sustainable impact on the field or accumulate into well understood, coherent strategies for implementing multiple innovations simultaneously.

Even when research is conducted by practitioners, the use of "classical" investigative strategies vitiates the impact of their findings. This intrinsically limited approach is mirrored by parallel misconceptions about a separate policy making sphere in which research on aggregated patterns and trends provides all the data needed for decision making. Funding programs geared to practitioner or policy maker research too often replicate this "micro-practice/aggregate-policy" disconnected model of educational intervention and therefore achieve little lasting impact.

Given that our nation's educational system must evolve to meet the needs of a global, knowledge-based environment, then all the groups within that system must contribute towards mutually agreed innovation strategies and outcomes. Any set of stakeholders that is not given recognition as a respected and empowered experimenter and contributor towards improvement will (wittingly or unwittingly) co-opt the change into trusted and known patterns of work and thus defeat any *significant* purpose not mutually explored. This is why the prevalent conceptualization of educational research as academic and leading to 'transfer' has direct deleterious effects on the relationship between research, policy, and practice.

The type of interaction among scholars, policy makers, practitioners, and other stakeholders in quality education that we propose undercuts the limitations of conventional research discussed earlier and lies at the heart of effective, sustainable scaling-up through the mutual evolution of goals and outcomes. By identifying what we see as major gaps in current research studies, our focus is on defining what types of funding programs and policies are needed to develop a better balanced portfolio of research activities. To a great degree, the need to revisit how research impacts practice is a reflection of a general paradigm shift in implementing innovations, focusing on 'systems' that resist or empower change. [34]

[34] See, for example, *Schools that learn*. Senge, P., Cambron-McCabe, N., Lucas, T., Smith, B., Dutton, J., & Kleiner, A. New York: Doubleday, 2000.