New Millennium Research for Educational Technology:
A Call for a National Research Agenda

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Abstract. Research to indicate unique pedagogical benefits of technology in education flourished until the mid-1980’s, when criticism of the premises and methodologies of studies made educators question the usefulness of so-called “media research.” Currently, increasing costs of maintaining technology infrastructure and evidence of low use of technology by teachers has generated new urgency for research to help provide a rationale for specific uses of technology to support teaching and learning. The new National Educational Technology Plan currently under development by the U. S. Department of Education provides an ideal opportunity for articulating a plan for meaningful research. This article describes topics and issues on which new research should focus and offers guidelines for research methods to address previous criticisms and provide results to guide future use of technology in education.

Keywords: educational technology, instructional technology, educational technology research, research methods, research agenda, technology impact studies
Introduction

Three assumptions educators often make about research on technology-based (i.e., computer-based and online distance learning) methods are that: (a) it has a commonly-held, theory-based agenda, (b) findings provide convincing evidence about the unique ways modern technologies enhance achievement and motivation; and (c) these findings shape practice in the field. The last thirty years of educational technology research notwithstanding, none of these assumptions is currently true. The authors propose that current conditions in education and society offer the combination of urgent need and unique opportunity to make all of these assumptions realities. A review and analysis of the history of recent research on electronic technologies in education reveals key issues and problems that must be addressed as we contemplate a research agenda for the next era in technology use in education.

Background on the Educational Technology Research Agenda

As Richey (1997) pointed out, agenda-building in a field as prone to change as educational technology is a “complex process … often dependent on far more than whether or not an idea is worthwhile” (p. 8). Yet the importance of what she calls a “systemic theory agenda” cannot be overstated. It “captures the intellectual climate of a field … identifies those issues currently receiving major attention … is supported by influential scholars of the day … is heeded by expert practitioners … influences the selection of articles for publication in journals of our field (and) paper proposals accepted for presentation at our national conferences … and shapes university curricula which prepare and update the professional in our field” (p. 7). Until the early-1980’s, the theoretical foundations of educational technology research were not widely debated and the research agenda based on them seemed comparatively clear. The
theory base was primarily cognitive-behaviorist, and research into technology-based methods reflected this view. In addition to numerous evaluations of computer-based and distance education applications and products, there were many experimental and quasi-experimental “media comparison” studies comparing technology-based with non-technology-based methods to establish which one was better for teaching a topic or skill. A thriving meta-analysis culture had the goal of identifying trends in findings across these studies. The question common to all these enterprises was fundamentally a cognitive-behaviorist one: “How effective is a technology in helping learners acquire desired behaviors (knowledge and skills demonstrated by an observed and measured performance)?”

Attacks on this solid bastion of traditional thinking came from two directions, one theoretical/pedagogical and one methodological. Working together they constituted an assault that effectively called a halt to media comparison studies and meta-analyses and left the field without a clear philosophical framework or workable research agenda.

The first attack came from educators, psychologists, and researchers who espoused a constructivist perspective on learning. They said that the aim of education was not to transmit learned behaviors from an expert source (an instructor or a package of instruction) to students. Instead, it was to create environments in which students generated their own knowledge with appropriate assistance or “scaffolding” from teachers. Seymour Papert (1980) was an early and outspoken representative of this perspective, which he based primarily on the theories of Piaget. Later, the Cognition and Technology Group at Vanderbilt (1991, 1993), calling on work by Vygotsky in cognitive scaffolding and Brown, Collins, and Duguid (1989) in situated cognition and cognitive apprenticeships, became a major hub of constructivist research and development.
These theorists and researchers turned the focus away from the impact of the technology being used to the impact of “anchored instruction” which technology could support.

At about the same time, Clark’s (1983, 1985, 1991) criticism of the research methods used in studying technology-based methods began a parallel attack on the traditional research agenda. Clark said that “media are delivery vehicles for instruction and do not influence learning” (1983, p. 453). He based this on analyses of past media research, which he found to have uncontrolled effects due to methods and content. For example, he found that two methods being compared often had different teachers. He concluded that any effects attributed to a technology were, in fact, caused by the methods used by the teacher or other variations in the content of the treatments. Based on these issues, he called for a moratorium on media research.

Kosma (1991) tried to address both theoretical and methodological problems by agreeing partially with Clark. Kosma said that, as long as media were viewed strictly as delivery systems, there would be no discernible difference among treatments that was not due to confounding factors. Instead, he proposed a theoretical framework that “presents an image of the learner actively collaborating with the medium to construct knowledge” (p. 179). He felt that the “symbol systems” and “processing capabilities” unique to each medium could make an identifiable and useful contribution to this collaboration. While advocating more research to clarify these unique contributions, he left the guidelines for this development and research up to the “creativity of designers” (p. 206) and those who would study their products.

Kosma’s proposal was helpful in re-directing the focus on studying the contributions of the technology-based method, rather than the technology or “medium” itself. Subsequent research such as the well-regarded *Quality Counts* study (Olsen, 1999) seemed to be heeding this line of
reasoning, asserting that it was not how much teachers used computers but how they used them that made a difference in students’ achievement.

At the same time, pragmatism born of experience with students and classrooms dictates that we modify pure constructivism as a theory basis for a new research agenda. In his call for a “post-modern agenda for instructional technology (IT),” Soloman’s (2000) insights are useful. He maintained that “The philosophical core of post-modern IT is a belief in pluralism, which can be described as respect for difference and resistance to single explanations … pluralism in our field posits that there is no single best model or theory of learning” (p. 14). He suggests that the sheer complexity of the learning context calls for a closer look at the combination of method, learner, and environmental variables that contributes to successful learning.

In Soloman’s “post-modern” view, both “scientific” and “critical” methods must be used to study this complex picture. Science is “problem-driven and goal-based,” (p. 14) and looks at whether or not objectives have been achieved. Criticism looks at the details that could account for the outcomes. Like many current theorists, Soloman clearly emphasizes the constructivist view of learning as “knowledge (being) created rather than found” (p. 14), but his acknowledgement of the need for scientific study allows for the statement of specific problems to investigate and measurable observations of changes in behaviors: two conditions the cognitive-behaviorists have always said should be the basis for establishing that learning has, indeed, occurred.

The merging of critical features of these heretofore competing theories sets the stage for a research agenda to study the benefits of technology-based methods as integral components of solutions to instructional problems, rather than as “delivery media.” While we recognize that this proposal will not (and probably should not) silence the ongoing debate about the principles and
practices of cognitive-behaviorist versus constructivist approaches to learning, we do feel that some détente in the theory debates is essential if we are to progress to the work that must follow them: crafting and studying technology applications that work in actual instructional settings.

The Need and Opportunity for a “New Millennium” Research Agenda

The need for a new research agenda has never been more urgent. Recent projects under the U. S. Department of Education’s (DOE’s) Preparing Tomorrow’s Teachers to use Technology (PT3) initiative have demonstrated clearly that the process of integrating technology effectively into education comes with a high price tag. Educators and policy-makers look to educational technology research to help supply a solid rationale for why these funds are well-spent (Ringstaff & Kelley, 2002). This is increasingly important in light of recent criticisms of the continuing investment required for technology infrastructure and the inconsistent impact and low usage by teachers, regardless of their increased training and access to resources (Cuban, Kirkpatrick, & Peck, 2001).

Cradler (2003) stresses that the recently enacted No Child Left behind (NCLB) Act, which will be influential in the next few years, requires that learning resource such as technologies “be closely aligned to state academic standards and that research document the extent to which (they) result in student achievement gains as measured by standards-based assessment” (p. 55). However, in his review of “best evidence” studies posted by the Center for Applied Research in Technology (CARET), a funded project of the International Society for Technology in Education (ISTE), Cradler found that electronic learning resources aligned to less than 40% of the California Content Standards for the major content areas. He also found a “shortage of formal controlled research” (p. 56). Even with experimental studies that reported statistical significance,
there was “very little information on the educational significance or the implications for teaching or learning” (p. 56).

In addition to the *NCLB* requirements, the U. S. DOE is currently calling for input from educators to create a new National Educational Technology Plan (http://www.nationaledtechplan.org). As Richey (1997) observed, another component of an agenda-setting process is “support of credible and authoritative people in the field … The larger the base of support, the more likely a topic is to become part of the discipline’s systemic agenda” (p. 9). The DOE’s National Technology Plan could be the single most influential force on the educational technology research agenda for the future. It is crucial that the important topics to be studied are articulated now and offered as an integral part of the plan.

**Focus and Topics for a Future Technology Research Agenda**

**Recommended Focus: Providing a Rationale for Technology Use**

In light of the pressure from policy-makers and from within the field itself, the primary task of the new research agenda seems clear. Future research must address squarely the question of why teachers should use technology-based methods. The emerging theory base demands that studies look at technologies not as delivery systems, but as components of solutions to educational problems, and that research questions be stated in a way that the contributions of methods can be examined and tested.

Roblyer and Schrum (2003) say that rationale-building research should concentrate on the “relative advantage” that technology-based methods and materials offer over other methods and materials. This concept is based on Rogers’ (1995) diffusion of innovations research that shows
potential adopters of a technology are more likely to accept it if they see clear benefits or “relative advantage” of new methods over old ones.

In his talks to groups of educators around the country, former PT3 Program Director Tom Carroll (Carroll, 2000) emphasized that some technologies in common use have obvious relative advantage that needs no research to confirm it. For example, when computer simulations save schools costs of consumable materials or allow multiple simulated experiments that are not feasible using actual environments, relative advantage should be readily apparent. However, even methods with self-evident benefits are in need of explication to realize their potential in practice. In addition, other methods have been hypothesized to have unique instructional benefits and some have popular support, but research to confirm these benefits has been inconsistent or done with inadequate controls or numbers of subjects to make findings useful to others.

**Recommended Topics: Important Issues to Address**

This dual view of technologies as both an already-accepted part of some areas of educational practice and as a potential influence in others requires a multifaceted approach to providing research that can guide both policy and practice. We propose that studies that would be most helpful are those intended to indicate: (a) that certain technology-based methods have the potential for unique and fairly consistent benefits in response to certain kinds of educational problems, (b) there are ways to implement technology-based methods already in widespread use (e.g., word processing, distance learning) that lead to increases in their impact on achievement, retention, and student satisfaction; (c) progress we are making to meet some of education’s most important goals for technology (e.g., decreasing the Digital Divide), and (d) how we can shape
and direct our technology resources to better accomplish their intended purpose. These purposes and examples of studies that address them are described briefly here:

- **Research to establish relative advantage** – In their review of findings to date on the impact of technology-based methods on achievement, Ringstaff and Kelley (2002) find that research has provided considerable evidence that directed methods such as using software tutorials for self-paced instruction and drill and practice for enhancing retention of learned skills can significantly raise scores on standardized tests. However, even this evidence is not as helpful as it could be since studies tend to focus on “a specific software or technology, severely limiting the generalizability of their results … and fail to take into account the larger classroom, school, and district context in which the technology was used” (p. 6). Ringstaff and Kelley also acknowledge that evidence of benefits frequently proposed for the following more constructivist, inquiry-based uses is also “far from conclusive” (p. 7). Research is needed to indicate that the following often-proposed benefits are possible:

  - The graphics and interaction features of modern visual technologies have unique qualities to increase and speed comprehension of unfamiliar or abstract topics by clarifying their underlying concepts.
  
  - Visual examples provided by problem solving software, simulations, and video scenarios prevent inert knowledge by linking the concepts to real-world applications. (Brown, Collins, and Duguid, (1989) describe *inert knowledge* as skills which students learn but never use because they cannot see where or how they apply to problems they encounter in real life situations.)
  
  - Online collaboration projects encourage greater appreciation of diverse cultures and improve reading and writing skills.
Studies that address these kinds of claims are beginning to appear. For example, Doty, Popplewell, and Byers (2002) looked at whether the interactive qualities of CD-ROM storybooks could result in greater comprehension for students than reading in a static format, and Funkhouser (2002-2003) explored the impact on achievement of constructivist instruction made possible by the unique capabilities of geometry software as opposed to the traditional instruction that non-electronic materials are capable of accomplishing.

• **Research to improve technology implementation methods** – A casual reading of any professional journal or education periodical reveals that many technology-based methods are already in widespread use (e.g., word processing, distance learning). Research is urgently needed to help shape the ways these technologies are being implemented in order to increase their impact on established measures of education quality: achievement, retention, and student satisfaction. Even accepted “older” technology-based solutions such as using word processing for composition writing have not been sufficiently studied in practice. In reviewing evidence on how people grade word-processed vs. hand-written products, Roblyer (1997) found reviewers who had not been trained to guard against a bias tended to give word-processed products significantly lower scores. An example of recent research into commonly-used online methods is Overbaugh’s (2002) study of how electronic mailing lists and bulletin boards could be implemented more effectively to take advantage of unique features while preventing unintended outcomes. Findings from such studies have important implications for future practice.
• **Research to monitor technology’s impact on important societal goals** – Although some technology uses in education require more research to justify them, access to technology is acknowledged as being an essential part of a quality education for citizens in today’s Information Society. We also need to know that increased access to technology is resulting as expected in greater learning opportunities and skills for all students. Studies are needed to monitor our progress in accomplishing the following kinds of important goals:
  - Technologies help underserved students gain increased access to learning opportunities.
  - Ready access to unique information sources and populations is enhancing critically important information literacy skills.
  - More use of visual technologies is increasing critically important visual literacy skills.

Useful studies in this area are often surveys such as those by Hoffman and Novak (1998) to capture the impact of race on Internet access and computer usage. Others are studies such as the one by Chisholm, Carey, and Hernández (2002), who reviewed access to and uses of information technologies by students of various ethnic backgrounds.

• **Studies that monitor and report on current technology uses to help us shape desired directions** – Finally, research is needed to shape the characteristics of our newer technology resources and guide their uses in education. Mioduser Nachmias, Lahav, and Oren (2000) examined the characteristics and usefulness of educational web sites to determine how they should be better developed and implemented in the future. Harris and Jones (1999) studied communication patterns of online mentoring to determine how this kind of activity could be better implemented to increase impact. Research is needed to help shape appropriate uses of these and many other technologies (e.g., handheld computers) beginning to be seen with increasing frequency in schools.
Criticisms of Past Research and Guidelines for Future Methods

Clark’s (1983, 1985, 1991) criticisms of the focus and methods of technology research are not the only negative evaluation it has received. The usefulness of findings in this area has long been compromised by a reliance on funding from the very groups who, as Clark pointed out, would profit most from positive findings: hardware and software companies. There is also consistent evidence of what can only be characterized as basic design and methodological flaws in many studies. Much of educational research in general is often the target of the latter criticism (Kaestle, 1993). However, technology research is, perhaps, in a rather more tenuous position than many other areas of education in that it has long been in need of a rationale sufficient to justify its high costs and complexity. Guidelines for future research are needed that recognize these flaws and offer means of remedying them.

Guidelines for Research Hypotheses

Clark (1983) cited design flaws common to individual studies (e.g., different teachers for experimental and control groups) and the meta-analyses that summarize findings across them (e.g., tendency for publications to accept primarily studies that report significant, rather than non-significant, differences). Kosma (1991) and others acknowledge that a research agenda based on showing the “overall impact of one medium over another across learners” (p. 204) will not guide us into the new millennium of technology in education. However, Clark’s criticisms and Kosma’s acknowledgement expose a far deeper failing inherent in much of educational research: flaws in the ways hypotheses are (and, often, are not) thought about and posed.

It is far from clear in many studies that researchers have considered carefully why given methods are being proposed for testing. It is axiomatic that educational research should help
address heretofore unresolved or poorly resolved educational problem(s), and that hypotheses should be derived from a theory base that leads logically to the hypothesis as a solution to the problems(s) and makes it clear why it should be tested.

Guidelines for Design Methods

In his review of recent “best evidence” technology research, Cradler (2003) alludes to several kinds of design flaws, some of which indicate underlying problems with hypotheses. He says that it often “not clear what specific research and evaluation questions the study is attempting to answer” (p. 56) or its “educational significance or … implications for teaching and learning” (p. 56). Other design flaws he cites include: small sample sizes that reduce statistical significance; measures of impact unrelated to objectives of the materials; measures of impact that are not objective and/or validated; descriptive, non-quantitative methods only with no control or comparison groups; and self-report data unaccompanied by corroborating data.

Not only must future research refrain from simplistic comparisons of materials cited by Clark, it must begin to focus on the unique qualities of technologies to make possible given methods. If a study proposes that a technology-based method is superior, it should be hypothesized to be superior because it has unique characteristics to solve a significant, unresolved problem such as low reading comprehension (as addressed by the Doty, Popplewell, and Byers (2002) study) or students’ persistent difficulties with geometry concepts, as addressed by Funkhouser (2002-2003). Studies must offer evidence – both in design and in data analysis – that the symbol systems and/or processing capabilities of a technology make possible an instructional method or learning environment that would not be possible through another (i.e., non-technology) method.
Since the NCLB Act will have a long-term impact on all future educational research methods, it is important to acknowledge the criticism of past methods implicit in its recommendations. It is clear that so-called “scientifically-based research (SRB) methods” mentioned over 100 times in the NCLB Act reflects a pervasive impatience with the failure of past research to provide objective, helpful evidence to guide practice. Cradler (2003) recommends that, in addition to using multiple data sources and validated measures, studies should set up control groups whenever possible and use valid comparison groups even if true controls are not possible.

It seems important to join those who, in the aftermath of the NCLB Act, have asserted that the failure of past research cannot be remedied solely by using experimental and/or quasi-experimental methods. Berliner (2002) reminds us that one of the largest and best-funded replication studies ever, one of different instructional models of early childhood education, found that variation was larger across different sites implementing the same program than it was across different programs. “No program could produce consistent results across sites. Each local context was different, requiring differences in programs, personnel, teaching methods, budgets, leadership, and kinds of community support” (p. 19). Thus, he says, “good science” requires not only trying to capture valid and reliable evidence of improvement, but also attempting to explain the complex set of conditions that led to improvement. As Soloman (2000) recommended, critical analysis methods must join “scientific” ones to help us understand what works, when it work, and why.

**Reporting Guidelines**

Cradler’s (2003) review of past research found many reporting flaws that are just as problematic as design ones, since they limit both interpretation of findings and studies designed
to replicate them. Reporting flaws include: insufficient information on the intervention and/or technology and other resources being implemented and the characteristics of large samples that allow disaggregation of important variables; and a disconnect between the conditions under which technologies are used and their conclusions, or between the conclusions and the data cited to support them.

Reports of research studies must address these frequently-cited shortcomings. They must be more comprehensive and informative about the methods and materials used, conditions under which studies take place, data sources and instruments, and subjects being studied; and they must emphasize coherence between their methods, findings, and conclusions.

Conclusion: Immediate and Long-term Priorities

To explore the degree to which current research may already be addressing the four kinds of needs cited here, we analyzed the purposes of articles in the 1999-2003 issues Journal of Research on Technology in Education (JRTE). Findings from this analysis (Table 1) revealed that most articles focus on evaluations of local programs or on methods of furthering technology integration or improving teacher training. Our analysis provides further evidence of what Cradler has already found. Relatively few studies are done for purposes that would help justify the costs of integrating technology-based methods.

As Table 1 indicates, most published research articles describe methods of getting students or faculty more involved with a technology (e.g., the Internet) or how to structure training and other conditions to get them more interested in using technology in general. From the perspective of the NCLB Act, this kind of research begs the question of “Why should we have students or faculty use technology more?”
<table>
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<tr>
<th>Types of Articles</th>
<th>Number of JRTE Articles 1999-2003</th>
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<tr>
<td>Type #1: Studies or reviews of unique benefits of various technology-based methods compared with non-technology-based methods</td>
<td>11 (8%)</td>
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<tr>
<td>Type #2: Studies on ways to increase impact of technologies already in common use</td>
<td>2 (1%)</td>
</tr>
<tr>
<td>Type #3: Studies on trends in usage related to achieving important societal goals</td>
<td>8 (6%)</td>
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<tr>
<td>Type #4: Studies that monitor/report current technology uses to help shape desired directions</td>
<td>5 (4%)</td>
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<tr>
<td>Type #5: Other studies, e.g., evaluations or descriptions of programs, implementation methods, or usage characteristics; types, extent, or perceptions of integration</td>
<td>87 (64%)</td>
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<tr>
<td>Type #6: Theoretical articles and position papers</td>
<td>23 (17%)</td>
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Table 1. An Analysis of Article types in JRTE from 1999-2003

It is to be expected the National Education Technology Plan now under development will provide considerable and immediate impetus to focus on these and/or similar topics and to improve the quality and usefulness of research. Hopefully, this plan also will make it easier to obtain funding from sources other than those that could be perceived as having a self-interested agenda.

However, groups Richey (1997) describes as the base of support for a technology research agenda will have the most direct, long-term impact on how well studies that support a new agenda are carried out: influential scholars and expert practitioners who guide dissertation research, select articles for publication in journals of our field and paper and proposals for presentation at our national conferences, and who shape the curricula of teacher preparation programs. These groups will help shape the next millennium of technology in education as they view proposed studies or reports of studies through the lens of whether they address one of these key, unmet needs and meet design and reporting requirements. It is they who will determine that research findings are meaningful and helpful to those who look to them for guidance.
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