



Response to Comments

Clinical Study: A Pursuit of Responsibility as the Basis of Education Research

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My initial contribution to this discussion (“Will a Clinical Approach Make Education Research More Relevant for Practice?” in this issue of *Educational Researcher*, pp. 412–420) focused on the question of whether a clinical research practice might make education research more relevant for practice. The characteristics of a clinical approach are as follows:

1. An overlap in the roles of the researcher and the professional. Researchers of education are practicing teachers, just as medical researchers are doctors.
2. A focus not on theory as separate from practice but on theory-informed practice.

Indeed, as David Labaree correctly concluded in his response article (in this issue of the journal), my answer to the question of whether education research should become a clinical study is “yes.” I mentioned four reasons why I believe a conversion is necessary.

1. *Active engagement with the object of study.* Active engagement shapes the frameworks through which researchers perceive the reality of education. Working with students results in a sharpening of the senses, enabling researchers to “see” more—in the same way that art connoisseurs are able to see more aspects of pieces of art. Active engagement results in tacit knowledge, which is the basis of how education is conceptualized and studied.
2. *A focus on personal skills.* An important part of the knowledge necessary for making education work is personal skills. Such skills are acquired through working with students. All human qualities are at stake, and of these, the relational and the emotional, among others, are crucial. Educational study should focus not only on knowledge that can be severed from people but also on the development of people who have knowledge and skills. Knowledge about a skill, without the skill itself, is empty.
3. *A view of education as a moral practice.* Any way of conceptualizing and researching education requires decisions that are, in essence, normative. Researchers should thus take responsibility for the ways in which they conceptualize education and for the ways they inform the field with the results of their studies.

4. *Recognition that education consists of a large number of cases that are both similar and unique.* Generalized knowledge that functions as a mirror to explore concrete situations is not likely to be a top-down construction. A constant dynamic interplay between the particular and the universal is necessary, both for constructing theory and for deepening and advancing theory and practice.

By referring to medical studies, I demonstrated that clinical research is an accepted research practice in the scientific disciplinary domain. Formal research is fueled by the practical experience of clinical researchers. The disciplinary standard that calls for engagement with one’s object of study actually goes for any discipline: Cell biologists work with cells, and medical researchers work with patients. In the same way, education researchers need to work with students. My contribution can be read as a plea for integrating connoisseurship (which is the result of intense engagement with the object of study) and practical wisdom into the study of education as we know it.

My analysis is not intended to characterize Labaree as a defender of detached science. Most education researchers, Labaree included, understand that scientific detachment is impossible and even undesirable. The reason Labaree’s article figures so centrally in my contribution is that Labaree provides useful concepts for describing the role of many researchers and the purpose of research as it is conducted in many places. Labaree and I live in different countries, but his description of the roles of teachers and researchers also applies to my situation.

Before I go into more detail in responding to the comment articles in this issue of *Educational Researcher*, I would like to express my deep gratitude to the editors and to the authors who have taken the time to comment on my article. Although I recognize that they do not necessarily agree with my views, I highly appreciate the opportunity for discussing both my concerns and my remedies with these leading international researchers.

In the following sections, I will discuss each of these contributions, starting with Labaree’s.

Labaree: “The Dysfunctional Pursuit of Relevance in Education Research”

David F. Labaree (in this issue) welcomes my efforts to clarify the issues surrounding the relevance of education research, but he

profoundly disagrees with me. Labaree's main point is that the different orientations of teachers and researchers should be dealt with not by combining the two into a single role—the clinical researcher—but by promoting fruitful dialogue between teachers and researchers. He advances three reasons: (a) It seems impractical for all researchers to split their time between school teaching and education research; (b) universities are not eager to pay scholars to teach in primary or secondary schools, and school districts are not eager to pay teachers to do research; and (c) the skills involved in being an expert teacher or an expert researcher are so strikingly different that it would be difficult to achieve mastery in both at the same time. I discuss each of these arguments in turn.

Dialogue Between Researchers and Teachers

Is it true that the research–practice gap can be dealt with by fruitful dialogue between researchers and teachers? By referring to Polanyi's theory of knowledge, I showed that intense engagement with the object of study is essential to learning to understand it. Unfortunately, Labaree does not address this issue at all. Teacher–educators have demonstrated that practical experience in the classroom shapes one's perception. All research methods and all testing procedures are based on the way in which researchers conceive education. Can there be any good reason for a researcher of education *not* to be a connoisseur? The tacit knowledge that connoisseurs acquire is a skill that cannot be simply transferred in a dialogue, any more than a dialogue can transfer the skill required to play piano. Cochran-Smith and Lytle (1990, 1999) are right when they argue that active teaching can offer special insights into the knowledge production process that those studying someone else's practice cannot provide. Researchers who are not teachers simply cannot experience all essential aspects of education.

Having said this, I acknowledge the value of an interchange between “outsiders” and “insiders.” Insiders are unlikely to consider the structural features and causes of social practices or the norms that they unwittingly internalize and use in communication and action; these practices and norms are the taken-for-granted context of the insiders' lives (Lester, 2005). The value of an outsider's view, however, does not contradict the value of professional development. Dialogue-based interactions between researchers and practitioners can, of course, yield interesting results, but dialogue is unlikely to offer researchers sufficient understanding that will enable them to contribute to the advancement of the study of education.

Paying Scholars to Teach and Teachers to Research

A bigger picture is necessary to address this point. We have grown accustomed to a separation of knowledge and action, in which one professional group acquires knowledge about education via research and the other professional group is expected to apply this knowledge in schools. In the early days of education research, such a separation was not considered natural, as Dewey's laboratory schools showed. In the beginning of the 20th century in the Netherlands, the editors of academic journals were either practicing teachers or teacher–educators, or they were otherwise involved in the practice of education (Depaepe & Bakker, 1998). At the highest levels of secondary education, teachers with

doctorates were not an exception. Then, along with the rise of detached science, a separation between knowledge and action occurred. Teachers lost control over their work; outsiders prescribed how they should conduct their teaching (Elbaz, 1997). The result was an increasing set of demands imposed by outsiders that led to a process of intensification of teachers' workload (Apple, 1986). The status of teachers declined. At present, the practice of teaching, the nerve center of quality, is low in the hierarchy, resulting in an inverse relationship between one's status in the academy and one's involvement with teachers, schools, and teacher education (Lanier & Little, 1986; Zeichner, 1995, 1999). This has impeded, rather than facilitated, good educational practice. In the Netherlands, we are confronted with a serious teacher shortage. It would not be difficult to explain to politicians and school districts in the Netherlands that the separation between knowledge and action has been destructive to the profession of teaching. There is ample justification for paying teachers to do research, if only that the teaching profession needs more career perspectives to remain attractive for highly skilled people. Zeichner and Noffke (2001) mention a list of positive effects of teachers doing research, for instance, a renewed feeling of pride and excitement about teaching, a revitalized sense of self among teachers, and the engagement of intellectual abilities and the importance of that capability to teachers' professional lives. At the same time, researchers being more active in (local) communities would strengthen the role of the academy across the political spectrum—a win–win situation.

Expert Teachers and Expert Researchers

A clinical researcher indeed needs dual mastery. This is also the case in other forms of clinical study, such as medicine. Not just anyone can become a researcher of education! Yet, as Labaree's article demonstrated, there are many skilled teachers who are pursuing doctoral degrees in education. Apparently, they want to achieve mastery in both sets of skills, not simply one at the cost of the other. A new generation seems to be ready to transcend the present dualistic structure.

At the same time, the present generation of researchers has been socialized in the dualistic structure. These researchers do not have the skills of a teacher, and—for most of them—acquiring these skills would be no small undertaking. Consequently, dialogue with practitioners is the only way for researchers to gain knowledge about the practice of education. In the short term, dialogue must be encouraged. Over the longer term, however, the separation between knowledge and action needs to be rectified if true progress is to be achieved.

On the Pursuit of Relevance

Ultimately, Labaree disagrees with me on what he considers to be a more fundamental point: the definition of “relevance.” Of relevance, he asks, “Useful to whom and for what?” He argues that earnest efforts to make research relevant can make it useless by focusing on short-term results that are narrowly measured, instead of studying consequences across a longer horizon and a broader scope. He believes that if education researchers fail to contribute to theory they become engineers or product developers instead of scholars. The distinctive value of scholarly research dissipates

quickly when it is transformed from being use inspired to use driven. Therefore, he considers the pursuit of relevance to be dysfunctional.

My contribution, in essence, is about the nature and the goal of education research in light of the theory of science. I have tried to find an “objective” way of addressing education, a way that acknowledges the nature of education. I want to research education in ways that do not single out some aspects while neglecting other essential aspects. The study of education must be about education in its fullness, not about a rational residue. I did not propose to measure the relevance of research, nor did I argue in favor of the pursuit of short-term goals.

The central question is: Who decides what is relevant? In our present research practice, many judgments about relevancy are made, for example, by reviewers, program committees, and program evaluators. Such judgments are a distributed activity across universities. At present, the practitioner’s voice is almost completely absent from this activity. In clinical research practice, however, the voice of the practitioner is automatically included. A clinical academic community judges relevancy in light of its normative mission of education, using the same structures as we usually do.

Labaree is afraid that the distinctive value of scholarly research will dissipate when it segues from being use inspired to use driven. Is this fear justified? Clinical medicine shows us that use-driven research actually leads to concrete knowledge. Many people owe their lives to treatments that appear to be effective even though nobody yet understands why. The “why” remains an important question for further investigation, but not if investigation results in an emptiness that is detached from the reality of the object of study. A reconciliation of abstract knowledge and concrete action does not dissipate the distinctive value of scholarly research, but it defines the way in which the value of scholarly research can be expressed.

Wiliam: “What Should Education Research Do, and How Should It Do It?”

Dylan Wiliam (in this issue) illuminates and deepens my argument in favor of clinical research. He provides an analysis that underlines the necessity for a change in education (effectiveness) research, and he draws useful connections between my analysis and other literature.

Wiliam questions the general value of effectiveness studies by drawing attention to the example of class-size reduction programs. The bottom line of his argument is that a detached approach can provide only part of the story. Effectiveness studies present information about what has happened in the past. As a result, these studies provide a picture that is actually misleading. Therefore he believes that rather than limiting educational research to the pursuit of knowledge and the development of theories, a more appropriate focus would be that of actually moving people to action. This implies a change from *what is correct* to *what is good*. Making education work requires practical wisdom. Wiliam refers to the Aristotelian concept of *phronesis*: the practical wisdom to act well by the successful integration of general principles with detailed consideration of the specificities of the particular case in question. *Phronesis* requires knowledge of the particular circumstances and moves people to action, but it is not product oriented. The goal is

doing well. Wiliam concludes that this is the value of clinical research: It transcends knowledge from general findings because it requires the ability to interpret these general principles in the light of a specific context of practice. Wiliam and I are in agreement. I referred to a publication in which this Aristotelian analysis also occurred (Kessels & Korthagen, 1996). Rather than use the word *phronesis*, I chose the word *connoisseurship* (Eisner, 1979), but I consider the two to be (almost) equivalent. Flyvbjerg (2001), with whom Wiliam agrees, is correct that *phronesis* cannot be acquired by qualitative research. *Phronesis*, or practical wisdom, is actually a skill that is learned by building up experience by acting in real life. Reflection on the consequences of one’s action is essential. Qualitative research entails reading about the experiences of others. This can be a powerful instrument for reflection, but it is not the practical wisdom itself.

Wiliam continues his response by mentioning different modes of inquiry that use different sources of evidence: reasoning, observation, representation, dialectic, ethical values, and practical consequences. He emphasizes that each choice for a mode of inquiry must be defended in light of what is good for education. The last one, the Singerian mode, acknowledges ethical values and practical consequences. As I understand Wiliam, he holds that the Singerian approach is necessary to prevent researchers from actually disseminating the kind of half-truths that he illustrated with the example of research on class-size reduction. It implies that researchers should not simply disseminate research results as if they applied everywhere but should work out whether the application of a certain insight to a particular situation is indeed likely to result in an improved situation. In the Singerian system of inquiry, the practical consequences of research are also considered (see also Lester, 2005).

Wiliam’s emphasis on the way in which the application of research plays out in concrete situations clearly complements my analysis. During the past decades, the Netherlands has experienced several national educational innovation programs that were inspired by research. The results of these innovations were disappointing (Parlementair Onderzoek Onderwijsvernieuwingen, 2008). Although I do acknowledge policy makers’ responsibility, I also believe that the role that education research has played in the Netherlands needs to be further clarified. If abstract knowledge from the literature is applied to situations that are not strictly equivalent to the original environments, a series of let-downs may result. Wiliam emphasizes that researchers not only should know the results of research but also should be able to judge the value of a certain study for a specific situation. They need practical wisdom. This implies that researchers should be involved in research as well as in practice. It implies that the study of education should become a clinical study.

Finally, Wiliam presents Nonaka and Takeuchi’s (1995) model that clarifies the complementary roles of tacit and explicit knowledge. First, this model assumes that researchers are socialized (in education, by being immersed in practice as teachers)—that they are engaged in socially meaningful activity. To make sense of what they experience, they need language. This results in conceptual knowledge. Along with the concepts they have developed, they conduct formal research. The results of their studies further shape their practices, which leads to new experiences, new

tacit knowledge, new concepts, new studies, and new practices. This model thus adequately describes the scope of the clinical researcher.

Lagemann: “Education Research as a Distributed Activity Across Universities”

Ellen Condliffe Lagemann (in this issue) agrees about the importance and necessity of clinical research. While emphasizing that education needs to be studied in different ways, she warns against a romance with “scientific” research in education. She distinguishes between problem-finding, problem-solving, and translational research, and she believes the clinical aspect is most essential for problem finding, which can be both formal and informal. She holds that schools of education should focus on the kinds of research that either derive directly from “the doing” of education or seek to improve “the doing” of education. This requires both problem-finding and translational research. Translational research focuses on transforming the findings of scientific research into tools that link knowledge to actual circumstances. (Is this what Wiliam calls a Singerian approach?) Schools of education need to be essential places where ideas about education meet the realities of practice, and she regrets that some schools of education have become quite removed from the world of educational practice. At the same time, she emphasizes that the scientific research method cannot meet all of education’s needs. If we in the education research community persist in applying pure scientific research methods, the notion of science will become distorted, and we will unwittingly contribute to the continued demeaning of much that is important in education.

The term *research* seems to have a very broad meaning in Lagemann’s contribution, for she includes both formal and informal research. As I understand her article, “the doing” of education also counts as informal research. When Lagemann talks about science, I assume she means a positivistic approach, and she warns that the value of this approach should not be overestimated.

Lagemann talks about “problem-solving research.” I am not so sure that we can expect research to directly solve problems in education. Problems often have to be managed, as Lampert (1985) has so clearly explained. Lampert emphasized that the personality of the teacher is essential for managing problems. It all depends on what Lagemann means, exactly, by “problem” and “research.”

My comment on Lagemann’s warning is that in a clinical approach, research techniques may be used that formerly belonged to the positivistic domain. These techniques, however, are not used in a positivistic knowledge paradigm. They are considered to be ways of looking that reveal and conceal aspects of reality. As Wiliam noted (in keeping with the Singerian view), the appropriateness of a particular way of looking must be defended in light of a particular purpose (see also Lester, 2005). When researchers feel responsible for the way in which the results of studies are applied, they are inclined to more critically evaluate the limitations of certain positivistic research techniques. At the same time, there can be good reasons why a certain “positivistic” design has been selected. Thus, in a clinical approach there is no romance with “scientific” research.

Along with her support for the clinical approach, Lagemann has two reservations about my article. First, she believes that the

dichotomy between the concepts that typify the work of researchers and those that typify the work of teachers is not universal. She knows teachers who become researchers who are entirely capable of seeing the world in ways that are analytical, intellectual, universal, and theoretical, and she knows many researchers who are committed to the question of what is good for students.

I agree with Lagemann that various teachers are entirely capable of seeing the world in ways that are analytical, intellectual, universal, and theoretical. Also, all the researchers I know are genuinely committed to what is good for children. My point was not that the dichotomy described by Labaree is never trespassed; Labaree also tries to unite the two worlds as much as possible. At a certain point, however, he runs up against the definition of the role of researchers and the purpose of research as defined in our present research practice (but Lagemann led me to realize that the term *disciplinary standards* expresses better what I mean than does the term *research practice*). This is where my contribution starts. I have shown that our present conception of disciplinary standards is derived from dualistic notions, and I have explained why a nondualistic conception of our disciplinary standards is likely to contribute more to the advancement of the study of education. My article thus should be read as a plea for a change of disciplinary standards.

Lagemann’s second doubt has to do with the fact that she does not believe that disciplinary research can accommodate the normative, the personal, the particular, and the experiential, for disciplines offer rule-governed ways to identify patterns and meanings across multiple situations. To put it bluntly: A reference to one’s feelings is simply not accepted in the academy.

This is a nice point. I agree with Lagemann that academic research deals with explicit knowledge, not with tacit knowledge. It deals with the upper- and lower-right quadrants in Figure 1 of Wiliam’s article (p. 437 in this issue), not with the quadrants on the left. In *The Structure of Scientific Revolutions*, in which Thomas Kuhn (1970) also refers to Polanyi’s concept of tacit knowledge, Kuhn defends himself against some readers who have felt he was trying to make science rest on unanalyzable individual intuitions rather than on logic and law (p. 191). Yet, as Polanyi showed, tacit knowledge is part of *all* sciences, including the very hard ones. A paradigm shift occurs as a result of the fact that researchers have come to realize that their interpretations are based on tacit frameworks that they have started to question. The demand for explicit reasons for believing something to be true is a noble one, but as Polanyi explained, even the best researcher cannot avoid the reality that the giving of these reasons will never be complete. At a certain point, it is the researcher himself or herself who *embodies* the reasons (Grene, 1969, p. 115; Polanyi, 1958). In Wiliam’s article, Figure 1 shows that tacit knowledge comes from socialization and internalization. Therefore Polanyi stressed that socialization with the object of study is important. It shapes one’s tacit knowledge. The experience with the object of study invites researchers to invent language to make sense of their work. Sophisticated tacit knowledge enables researchers to see more than outsiders do and fuels the explicit part of the investigation via formal research.

Each research design is always based on tacit assumptions. For instance, the evidence-based movement presupposes that there is a “what” that works. I am anxious to know what happens with

these researchers after they have adopted practical responsibility for the learning of students for quite some time, and how the results of their studies work out in practice. Will they still believe there is a “what” that works? Will they still think it appropriate to look at education in causal or causative terms? Will they still believe that teachers and policy makers need only consume the knowledge from research?

Such a discussion may take place after education researchers have come to realize that the first standard of any disciplinary approach refers to engagement with the object of study. Socialization in the world of education is likely to contribute to the sophistication of the tacit tenets of the researcher’s knowledge. Therefore I have suggested including educational experience in our disciplinary standards. The academy need not buy into personal feelings or intuitions, but it needs to assure that researchers are properly prepared for the study of education.

Noffke: “Research Relevancy or Research for Change?”

Susan E. Noffke (in this issue) draws connections between clinical research, action research, and other kinds of practitioner research. Although she applauds the parallel between medical practitioner-researchers and teachers who do research, she asks why there is a need for the medical model of clinical research in discussing education research. Are there not arguments in the education research community that address the same issues?

My answer is clear: Yes, there are. There are Dewey and Vygotsky, who both saw continuity between thought and action and between scientific knowledge and everyday knowledge (Biesta, Miedema, & Berding, 1997; Moll, 2001; Van Oers & Wardekker, 1997). There is the sociocultural revolution that emphasizes that knowledge is constructed in meaningful social activity (Lave & Wenger, 1991; Palincsar, 1998). Strangely enough, many social constructivist researchers have applied these views to the learning of pupils, but not to their own learning as researchers. Apart from social constructivist views of learning, there is the literature criticizing the positivist knowledge paradigm, resulting in the acceptance of alternative research paradigms (Shulman, 1997). I therefore concluded that the idea of scientific detachment is generally superseded, both in the theory of science and in the thinking of many education researchers. I noticed, however, that the definition of the role of the researcher and the purpose of research is still derived from scientific detachment. Therefore, I called for innovation in our disciplinary standards. My work thus elaborates on the work of other education researchers.

Another reason I have called for innovation in our research practice (meaning the disciplinary standards for doing research), and not just for some kind of new approach to research, was that when new practice-oriented research paradigms, such as design-based research, are applauded by the research community, the fact that many researchers are not sufficiently prepared for this kind of research (because they are not teachers) is often neglected. This results in a situation in which new approaches are fit into old structures. From the outside, it seems that an innovation has taken place, whereas in reality, research as usual continues.

I referred to the medical studies for two reasons. As I explained, I wanted to include in the discussion those researchers who reject the

difference between hard and soft science (Feuer, Towne, & Shavelson, 2002). Therefore I explained how a “hard” science, such as medicine, partly draws on “soft” knowledge, namely, the case knowledge that practitioners acquire for medical diagnosis. The evidence-based research movement in education uses medical research as a model. My message to this movement is that there is much more to medical studies than randomized controlled trials. Clinical research in medical studies provides a very provocative example of how the integration of theory and practice, as well as the integration of different modes of knowing and accompanying sources of evidence, can contribute to the advancement of the study. Academic hospitals demonstrate that making practitioner research and academic research the antithesis of each other is not appropriate.

The term *clinical* literally means of or connected with a clinic, or of or related to direct examination and treatment of patients. This term thus has a medical connotation, which could make it a less appropriate term for education research. Yet Toulmin (2001) uses this term in a broader sense, referring to the purpose of improving concrete situations, while drawing on reasonableness (as opposed to rationality), rather than to the purpose of collecting assured knowledge. The term *clinical* in a broader sense has been used by other education researchers (Zaritsky, Kelly, Flowers, Rogers, & O’Neil, 2003), which is why I have chosen the term *clinical study*. However, the term *phronetic research* (Wiliam, this issue) is also appropriate. Even Cochran-Smith and Lytle’s term *teacher–research* will do, as long as it is clear that the concept of teacher–research is not limited to the category of classroom research and to teachers researching only their own practice. Which term is used counts less than the conclusion that experience as a teacher is an indispensable disciplinary standard for truly academic educational research.

I do not understand why Noffke has the impression that I accept the framework that distinguishes applied research from pure research. Clinical research is both, as Wiliam’s Figure 1 shows. It is an iterative process of gathering knowledge through research and learning from action. Those responsible for improving a concrete situation cannot avoid the institutional conditions of the situation. Therefore a clinical study is always research for change.

Noffke ends her response by asking what academic researchers make of the political economy of knowledge production in this era of changing universities. “Our efforts in education at creating new avenues for relevancy are enacted in a global context of neoliberalism (Hursh, 2008) that has a major manifestation in a culture of performativity (S. Ball, 2003)” (p. 431). Noffke adds, “Nothing less than the lives of children and those who care for them is at stake” (p. 431). It is for these reasons that I believe our pursuit should not be for relevance. Our pursuit should be for *responsibility* as one of the most basic values on which objectivity rests (Bulterman-Bos, Terwel, Verloop, & Wardekker, 2002; Newell, 1986). Thus I propose to include responsibility for students and for concrete educational practices in the disciplinary standards of the study of education.

A Conversion Toward Education as a Clinical Study

Let me end with some short notes about how I believe the conversion from a detached study to a clinical study should take place:

1. The new generation of education researchers should also be educated as teachers in a certain subject matter or in a field of learning, just as medical researchers are trained as doctors first.
2. The perspective of a clinical study might be threatening for those researchers who have no training as teachers or for those who have deliberately left this profession. Nonetheless, it is my view that a clinical structure should be strongly encouraged for the next generation of researchers. Researchers and practitioners should engage in dialogue for implementing this change on an equal basis.
3. During the transition from detached to clinical, each academic forum that reviews articles or grant proposals should publish the number of practitioners involved, with the intention that, after one generation, the whole forum should consist of clinical researchers.
4. Academic conferences should not only invite participants to discuss papers but should also provide access to the community of practitioners. This may occur via excursions or multimedia presentations. International conferences should allow participants to learn more about the local practice of education and the local educational system.
5. Experienced teachers who enter graduate school should be considered active agents of change. Their complaints should be weighed in a community of future clinical researchers and present faculty. The reform of education research should be the focus of these communities of learners.
6. Established researchers who are interested in becoming teachers need to be allowed to achieve that goal.
7. Faculties of education need to connect with the schools in their communities, or they should found their own laboratory schools where the insights developed at the university are applied in practice.

A move toward the classroom can also be experienced as threatening to the status of researchers. Although I am convinced that it will only result in the increased recognition that accompanies a broader acceptance, I would like to refer to the famous scientist Jacob Bronowski. As he so vividly explained in his book *Science and Human Values* (1956), real scientists do not care about status, nor do they bend for authority. They are independent and free.

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