# The Analytic and the Arbitrary in Educational Research

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*I wrote this article with the intention of stopping empirical research as it is currently conducted in education. To my amazement, this did not immediately happen. I was impressed by Jan Smedslund's critique of research in social psychology, and simply applied his argument to education. His claim is that most empirical research in psychology is really pseudo-empirical, and a product of conceptual confusions. I thought that some of the events surrounding this paper were instructive about how difficult it is to establish grounds for communicating with fellow-educators who worked with, I suppose, different presuppositions from mine.*

*I first read a draft of this paper to a group of educational psychologist's locally. I took a few examples of research more or less at random and tried to show, following Smedslund, that the researchers had confused genuinely empirical issues, which were not generalizable, with analytic elements that established necessary connections between the subjects of the research, and so they had produced what looked like, but weren't, empirical findings that could be generalized. My colleagues claimed that my argument only worked because I had carefully chosen examples of bad research which exemplified the confusion I was demonstrating. I argued that the confusion was true of almost any empirical generalization based on psychological research, and challenged them to give me an example of a piece of research that would be immune to the critique. They came up with the example which I use in the paper, of the "finding" that ordered lists are more readily remembered than random lists. After the article appeared, I was accused of carefully selecting my example to show my case!*

*Smedslund chose at random Bandura's "self-efficacy" research, showing that what was generalizable from it could be decomposed into analytic connections. Bandura wrote a response to this which seemed to me, with my presuppositions, to show little comprehension of Smedslund's argument. When I next raised the issue with educational psychology colleagues I was told that Smedslund's argument had been decisively dismissed by Bandura and didn't merit any further attention.*

*I may, of course, be missing some obvious flaw in the essay that follows, but, when I examine typical pieces of new educational research, Smedslund's insight seems to me to leave most research results devastated. Perhaps the problem--from my point of view--is that the kind of thinking Smedslund calls for is not a part of the training of educational researchers. None of us is good at accommodating arguments that cut away the foundations of what we have based our careers on, but I do think it is worth trying to be hospital to fundamental criticisms now and then. While Smedslund's analytic-and-arbitrary confusion may not be so absolutely devastating as he, and I, have thought, some sensitivity to the common flaw it points to could save us from a lot of bad research.*

**Introduction**

Doing research on human subjects in complex settings with the aim of improving something so problematic and value-laden as education presents the researcher with a daunting range of conceptual difficulties. The educational researcher cannot turn with any confidence to a body of educational theory for help. What is available from that direction is, rather, disputes about what an educational theory might look like, with no evident examples of such a thing on the ground to guide thinking and research (Nagel, 1969; Hirst, 1966, 1972, 1983; & O'Connor, 1957, 1972). The help offered by psychological theories about such apparently educationally relevant phenomena as, for example, "learning" is not without problems of its own. On the one hand, we have doubts expressed about the basic security of such theories, (Gauld and Shotter, 1977; Louch, 1966; Mackenzie, 1977) as in one of Wittgenstein's more notorious observations:

"The confusion and barrenness of psychology is not to be explained by calling it a "young science"; its state is not comparable with that of physics, for instance, in its beginnings. (Rather with that of certain branches of mathematics. Set theory.) For in psychology there are experimental methods and conceptual confusion. The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by" (Wittgenstein, 1963, p. 232).

And, on the other hand, we have doubts expressed about the applicability to education of research based on psychological theories. It is argued, for example, that the concept of learning embodied in psychological theories is significantly unlike any concept of learning that is proper to education (Oakeshott, 1967; Petrie, 1982). Then we have also a series of problems about moving from research findings to educational practice (Egan, 1983).

Most of these conceptual difficulties have been well rehearsed in recent years. In general they begin from the observation that the "dominant paradigm" in educational research uses methods derived from research into natural phenomena. Dispute centres on the ways in which human behavior is like or unlike other natural phenomena; or, rather, on the degree to which human behavior is a cultural phenomenon, bound up with intentions and meanings, and so resistant to being adequately dealt with by the "dominant paradigm." There is a sort of impasse in current disputes, at least until the objectors can show the researchers that there is something about the regularities of human behavior that prevent the fruitful application of the methodologies from the physical science "paradigm," or until the researchers actually come up with a relatively secure generalization or theory about human behavior that looks anything like those common in the physical sciences.

One of the crucial inconveniences of human behavior for the easy applicability of a methodology designed to deal with questions about natural phenomena is that human behavior does not come in discrete units. The dominant research methodology in educational psychology is used to establish empirical relationships between observable behaviors. A set of problems surrounds how we conceptualize and label human behaviors. Educational psychology, and psychology in general, relies heavily on concepts and labels—such as learning, motivation, development etc. etc.—which are complex and interconnected in subtle ways. Similarly even

the simplest labels for behaviors—lift, throw, open, kick, etc.—are applicable only within networks of intentions and meanings. One problem that follows from this observation is that of ensuring that the behaviors between which we wish to establish empirical connections are not already covertly tied through the labels we assign to them. Jan Smedslund (1978a, 1978b, 1979) has argued that the empirical connections established by much psychological research are already made by conceptual ties, and so the connections established are "pseudo-empirical." If his argument is sound, it would appear, on the face of it, to present yet another difficulty for the dominant "paradigm" of educational research. Smedslund argues that often what appear to be empirical findings in psychology are a product of confusing what he calls analytic and arbitrary components in the research design. If he is correct, a great deal of educational psychology research will have to be considered pseudo-empirical. The implications of his observations for the future of psychological research, and empirical research in education, are far-reaching, and merit some discussion.

**The Analytic and the Arbitrary**

By the analytic, Smedslund means things that are true as a matter of logical necessity, or by definition, or by tautology: in the Kantian sense in which the concept of the predicate is contained, although covertly, in the concept of the subject, and a denial of the statement in which they are contained would involve a contradiction. Thus "all unmarried men in Vancouver are bachelors" is true as a matter of logical necessity or by definition. We could treat the question "Are all unmarried men in Vancouver bachelors?" as an empirical question. We could design a tight survey, run it with great care, and analyse the results by the most sophisticated statistical methods. We could then announce that we had empirically established that 100% of the bachelors in Vancouver are unmarried. And, by such a procedure, we would indeed have established the truth of the proposition empirically. The empirical research is, of course, unnecessary; and we need feel no caution generalizing our results to Chicago or Paris. The connection between bachelors and unmarried men is established by analysis or definition. Smedslund's argument concerning a great deal of research in psychology is that the concepts dealt with very commonly contain a covert analytic element, an element that is made more difficult to see by their containing an admixture of arbitrary components as well.

The question, then, is whether commonly in educational psychological studies the experimenter confuses an analytic element and an arbitrary element. The analytic element guarantees connections between the two things whose relationship is being studied. The arbitrary element is genuinely empirical, but is not generalizable beyond the circumstances and subjects of the experiment. By confusing the two, educational psychologists routinely produce pseudo-empirical results, which they accept as straightforwardly empirical findings with some generalizing power.

Let us first try to clarify the kind of covert analytic component that may be found in educational research that draws on psychology as its "parent discipline." A number of people have noted the tautological nature of Thorndike's formulation of the "law of effect" (e.g. Louch, 1966)—that people tend to repeat behaviors which have pleasurable consequences. What is established here is a supposedly empirical connection between the tendency to repeat

behaviors and the warranted expectation of pleasurable consequences. But rather like the previous example, there is an analytic tie between the two. Choosing to repeat a behavior, if one can, and expecting pleasurable consequences are not independent things. What we mean by choosing to repeat a behavior is tied up in the expectation of pleasurable consequences. If we ran experiments that showed cases of people not choosing to repeat behaviors which they expect to be pleasurable, we would have to conclude that there was something wrong with the way we were identifying what was pleasurable for them. We identify a repeated behavior that is freely chosen, by the chooser expecting pleasurable consequences. Indeed, one might say that expecting pleasurable consequences could serve as part of a definition of what is meant by choosing to repeat a behavior.

Similarly, it has been pointed out before (e.g. Louch, 1966) that if we consider the list of propositions which Hilgard claimed psychologists have established with some degree of security, we will find other examples of this kind. His first proposition is that "brighter people can learn things less bright ones cannot learn" (Hilgard, 1956, p. 486). In this case, he is claiming that careful psychological research has established a secure empirical connection between brightness and ability to learn. But, again, the two are analytically tied. What we mean by brightness involves ability to learn more than is normal. It is a matter of definition, not empirical study. (It may be argued that empirical research has given precision to the degrees of brightness and what is learnable, but this will be countered below in the discussion of the arbitrary component.)

Hilgard's second proposition is that "a motivated learner acquires what he learns more readily than one who is not motivated." Here again what is meant by motivation is analytically tied to what is meant by learning more readily. One cannot identify the motivated learner independently of behaviors which exemplify more ready learning.

These propositions do not establish empirical relationships between distinct things. Rather they articulate partial definitions of the first term: e.g. a motivated learner is one who learns more readily, and who does x and who does y; a bright person is one who learns more, and does x, and does y; a bachelor is one who is unmarried, and who is a male. The point about these "findings" is not that they are obvious, or commonsense, or trivial. The psychologist could properly respond to such a point by observing that science often is involved in securely establishing things which seemed obvious or trivial, but that once securely established scientifically such findings can prove crucial steps to powerful theories with great explanatory power. The point here is more challenging. It is that such findings are not empirical findings at all; what are being presented as the results of empirical research are "findings" which are in part matters of logical necessity or analytic truths.

And the arbitrary component? All of the experiments which supported Hilgard's propositions did not yield identical results. Some bright children learned some things better than others, and on different days might have scored somewhat differently. The causes of these individual differences between subjects on tests and between the performance of subjects on different days are enormously varied. If an experiment involves learning random seven digit numbers, and one of the numbers is that of your telephone, you will obviously remember it better than any of the others. Such contaminations of experimental results due to arbitrary factors is

compensated for by, amongst other things, having sufficiently large groups of subjects. A large part of the methodology of educational research involves techniques of controlling for such arbitrary contaminants.

Let us imagine a research program that seeks to establish an empirical connection between ease of learning and degree of organization in lists. (I take this example because after rehearsing this argument with a group of educational psychologist colleagues, this was chosen by them as a clear case of an empirically established generalization). A useful product of such research might be information about how to organize lists so that they might most readily be learned. After some initial experiments, one might conclude as a first generalization that ordered lists are learned more easily than random lists. The first thing we need to observe about this conclusion is that it is another of those pseudo-empirical findings. The analytic component involves the necessary connection between order and learning. A detailed definition of learning would imply or involve notions of order—the structure of the human mind, important for what can be learned and how, and what is conceived of and recognized as ordered are not distinct things. Order, meaning, and learning are analytically tied. If people in our experiment learned random lists more readily, we would scan the lists for an hitherto unsuspected order. What we mean by ordered, then, is analytically connected with what we can more readily recognize and learn.

To return to the earlier example: if one of the elements learned readily was a random seven digit number, we might wonder what unsuspected order or meaning it had for that subject. On discovering that it was the subject's telephone number, we would be satisfied. But a great deal of the irregularity in experimental results is due to such arbitrary elements. Usually they are not as simple or dramatic as in this example. What one cannot do with these arbitrary elements is generalize from them. The fact that subject x learned a random seven digit number readily does not allow us to generalize either about that subject's ability to learn random seven digit numbers nor about anyone else's ability to learn them, or to learn that particular number.

So in the case of this simple experiment about learning lists, we have an analytic tie which guarantees that we will establish a strong positive correlation between orderedness in the lists and ease of learning. We have also, however, an arbitrary element which guarantees that what will count as ordered for one subject will vary in indeterminate ways from what counts as ordered for another. By confusing the two, by not distinguishing the analytic component from the arbitrary component, we treat the results of our study as an empirically established connection, which we may then seek to generalize.

The analytic component, however, generalizes absolutely. The arbitrary component cannot be generalized at all. We do not need an experiment to establish the analytic component. And the arbitrary component is immune to control.

**Implications of the Argument**

What range of educational research is affected by this argument? It would seem to affect all empirical research that aims at theory building and even establishing empirical generalizations. Let us consider Smedslund's general argument, then return with it to one further example. He

implies that the language of educational psychological research uses concepts among which there exists a network of analytic connections. This empirical research needs to draw on theories articulated in terms of these concepts, yet is itself unable to generate such theories. Where, then, are the theories to come from?

In Smedslund's account, the crucial mistake in psychology has been its adoption of the physical sciences model. His concern is not so much the adoption of the methodology, but rather the attempt to build similar kinds of generalizations, theories, and laws by means of the methodology. In his view, psychology might better see itself as a discipline more like geometry than physics. As he puts it:

The conceptual framework of psychology, as embodied in ordinary language, is anterior to and structures our everyday dealings with people, and also our scientific interpretations of data and our theorizing, in the same way as, e.g., the conceptual framework of geometry is anterior to and structures our dealings with space. There may, therefore, exist a parallel between the task confronting contemporary psychology and the task that confronted the Greek scientists in antiquity, beginning with Thales. . . . In both cases, what is required is an analytic effort to explicate and systematize the intuitively and unreflectively known conceptual framework used in everyday life. The validity of the outcome is to be proved logically rather than empirically (Smedslund, 1979, pp. 1, 2).

This is a rather radical proposal. It seems to suggest that theory generating in psychology, and educational psychology, become attached to epistemology. What might such theories look like? Smedslund gives some examples, of what he calls "common-sense theorems" or "ordinary language theorems." Consider the following:

1. If a person performs an act, he both can do it and tries to do it. Conversely, if he does not perform an act, he either cannot do it, but tries; or can do it, but does not try; or neither can do it nor tries to do it.

2. If a person wants x and gets x, he will get some satisfaction from this. If a person gets no satisfaction from getting x then it is not x that he wants..

3. If a person wants x and knows that act A will lead to his getting x and he can do A and no other want or knowledge interferes, then the person will do A.

4. If a person wants x and does A and gets x and believes that A always leads to x, then the next time he wants x he will again do A.

These "theorems" seem to be universally valid. Perhaps they could be tightened up a bit, but as they stand they describe relationships which must hold among things like "wanting," "acting," "satisfaction," and so on. One might compose an indefinite set of such sentences which describe similar relationships among other terms. They are relationships which would always be supported by empirical tests; but the empirical tests would not establish their truth. They are analytic truths, and it would be a conceptual confusion to try to establish them empirically, just as it would be to establish a geometrical theorem empirically. They are sketches of some of the simpler logical relationships which exist in our natural language among certain terms common

in some branches of psychology. The above theorems for example, cover the "law of effect," and go beyond it in scope.

Smedslund has demonstrated how what are taken as straightforward empirical research programs in psychology conclude with findings that are directly derivable from sets of such common-sense theorems. They are confused, however, by being mixed with a set of arbitrary components. He has demonstrated this on an article on social psychology, selected "blind" on the basis of criteria that would establish it as significant among researchers in the area. Also, he has shown that all of the supposedly empirically established conclusions supporting Bandura's "Self-efficacy: Towards a Unifying Theory of Behavioral Change" can be derived from thirty six "commonsense theorems."

As geometry carries us beyond our intuitive grasp of special relationships, so a formal discipline of psychology might be expected to carry us beyond our intuitive grasp of the relationships within and among such general concepts as learning, motivation, development, and so on. As geometry provides a set of useful theoretical axioms on which the builder and navigator can rely, so may the formal-psychologist establish a set of psychological axioms on which the educator, and educational researcher, can rely. What seems to remain for empirical research is the establishment of limited and local findings, applicable to particular subjects, at particular times, in particular circumstances.

Now this conclusion might seem only to echo a major trend in educational research of the last couple of decades. We have seen somewhat similar seeming conclusions in the disillusionment of, for example, the Aptitude-Treatment-Interaction (A.-T.-I.) research program stimulated by Cronbach, (1957, 1975) leading to Snow's conclusion that while the search for general theory in such areas is perhaps impossible, we may still hope to establish A.-T.-Is. with local applicability (Snow, 1977). Also the disillusionment with Piaget's general theories has followed the realization that much of their plausibility derived from mixing what we can call analytic and arbitrary elements: acquisitions that are empirically established as occurring in a particular sequence turn out on analysis to be such that the later acquisitions logically presuppose earlier ones (see, e.g. Brainerd, 1978; Hamlyn, 1978; Phillips and Kelly, 1975) and a close examination of the data shows ad hoc contrivances to deal with the considerable arbitrariness (Brainerd, 1973, 1977). So perhaps Smedslund's argument may, after all, appear only a dramatic way of reaching conclusions that are embedded in much present educational research practice. Many researchers see their task as elucidating particular problems, clarifying trends, exposing particular regularities in specific contexts, and so on, not establishing general theories or even particularly powerful empirical generalizations. I think Smedslund's argument threatens to cut deeper. Another example might at this point help us to explore his argument further.

**Objectives and Performances**

For an example I wanted to consider an educational prescription based on some empirical research. I thought a place to find such things relatively unselfconsciously stated was in a textbook on curriculum, and so I more or less randomly flicked through one that I had to hand until I hit what seemed like a straightforward empirical matter. In David Pratt's Curriculum: Design and Development I found a section on aims with a sub-heading "Empirical evidence." The evidence is in support of the claim that setting clear and precise goals is likely to enhance students' learning.

Pratt begins by citing Mace's "classic, although rudimentary study" (Pratt, 1980, p. 142) of 1935. A group of students working on computation problems were given a specific daily standard to surpass. This group performed better over a ten day period than did a group who were simply instructed to "do their best to improve." Pratt then cites a set of more recent studies that support and extend Mace's finding, including a set which have led to the expansion in industrial settings of "management by objectives." He further cites Duchastel and Merrill's 1973 survey of studies using objectives and Melton's 1978 survey of such studies. Both of these surveys indicate the usual inconsistencies that result when similar studies are conducted in slightly different ways with different subjects in different settings. But by looking for general trends among them it is possible to conclude that the use of objectives in many cases seems to improve students' performance.

What are we to make of this empirical evidence, however, if we look at it through Smedslund's lens? Mace's study, however rudimentary, surely shows an empirical connection between specific goal setting and performance. Here is the graph that shows his results: (the Alternative Group A line has been added, and should be ignored initially) -

*[the graph is missing in this online version of the paper]*

(Mace, 1935, p. 21).

Where would we look for analytic connections in a case like this? As a first sortie, we might consider the connections between being set goals and having a sufficient motive to achieve the goals set. We recognize casually that there are degrees of energy that are normally elicited by various kinds of challenge or incentive, and that people normally expend energy appropriate to the challenge they are faced by. What is considered an appropriate amount of energy is normally the minimum required to achieve the goal set. What would we say if someone expended greater energy than is required to achieve a set goal? We would say that there is some additional challenge operating, of a kind to be explained by that individual's history or cultural context.

We might begin to convert such an initial sortie into common-sense theorems:

If P is challenged to perform T in S, and P can perform T in S and the challenge evokes sufficient motivation to perform T in S, and no other circumstances intervene, P will perform T in S.

If P is challenged to perform T + n in S, but the challenge evokes sufficient motivation to perform only T in S, and no other circumstances intervene, P will perform T in S. (If P performed

T + n in S, it would mean that the challenge evoked sufficient motivation to perform T + n in S, or that some other circumstance intervened to provide the extra motivation.)

Let us return to Mace's findings with these proto-common-sense theorems, or tautologies, or analytic truths. They begin to sketch some necessary connections between challenge, motivation, and performance. These are not empirical matters; they are matters that turn on what the terms mean. They point us to consider what motivation is evoked by the challenges in Mace's experiment. In general it can be concluded that providing a precise challenge evokes motivation to perform what one is challenged by, if one can perform it and the circumstances are such that one is sufficiently motivated to perform it. The two groups in Mace's study differ in the challenge that is posed to them and the motivation the challenges evoke. Group B, we may say, is being challenged to perform T + n while being given sufficient motive only to perform T.

While lacking Mace's raw data we can assume that it is possible, even likely, that the worst performing subject in group A performed less well than the best performing subject in group B. That is, what challenge evokes what degree of motivation in what subject is a matter to be explained in terms of their individual histories, cultural backgrounds, neuroses, etc. The best performing subject in group B, let us say, performed T + n. In this case we will search for an explanation in terms of the peculiarities of this subject; we will, that is, look in this subject's personal history for the intervening circumstance which provides the added motivation to perform more than is motivated by the challenge itself. This is the realm of the arbitrary.

The averaging out of the scores for the two groups suggests a positive correlation has been established empirically between the setting of goals and performance. In such cases the arbitrary element is likely to be very large: that is, what counts as an adequate motive to perform particular tasks in particular circumstances will vary considerably for different people. The averaged-out results, however, sketch roughly the confusion of the analytic tie between challenge, motivation, and performance and the array of arbitrary elements. The genuinely empirical part has been disguised by the averaging procedure. What is genuinely empirical is precisely the product of the arbitrary differences in performance which result from the individual differences in the way particular challenges evoke different degrees of motivation and these produce different performances. What can be explored empirically is just what challenges most, or least, motivate what individuals in what circumstances to perform in what ways. What cannot be explored empirically is whether setting a challenge in appropriate circumstances will evoke the motivation to perform a task. What is perceived as generalizable from Mace's results is what is a product of the analytic component. The arbitrary component—what challenges motivate what individuals in what circumstances—is not generalizable; it yields data about those individuals in those circumstances only.

It might seem that the studies cited by Duchastel and Merrill and by Melton are indeed finding what particular kinds of challenge evoke what degrees of motivation in particular subjects in particular circumstances, and that their research designs are implicitly built on an axiom such as is sketched above. And perhaps Mace too presupposed such an axiom and his study performs the empirical task of showing that the challenge involved in setting precise objectives is more productive than the challenge involved in the exhortation to do your best to improve. From

such studies one may hope to discover empirically what particular kinds of challenge motivate most people to perform best. The problem with Mace's and the other studies is that they have not in fact excluded the analytic component from their research designs. What would we make of Mace's study if we reversed the performances of the two groups in the results? We could have produced precisely such a result if we provide group A with a gradually improving standard that is, over the period, lower than that performed by group B (alternative group A standard). Would we then say that we had discovered that exhorting subjects to do their best to improve produced better performances than setting progressively higher standards for them to achieve? We would say only that in the particular circumstances, the exhortation to improve presented a more stimulating challenge than did the specific standard prescribed. What Mace's study allows us to say is that in certain circumstances setting precise objectives can enable us to stimulate better performances than does the vague exhortation. What we cannot generalize from this and the other studies is that setting precise objectives will stimulate better performances than will giving vague exhortations. In as far as particular precise objectives represent T + n and vague exhortations represent T, then this generalization follows from our common-sense theorems. The problem with the research studies we have is that they do not deal simply with empirical questions, but they mix in also some form of the question about whether presenting a challenge to achieve T + n in circumstances where subjects are able to perform T + n and are sufficiently motivated to perform T + n will stimulate better performances than a challenge to perform T in similar circumstances. The generally positive results are due to the presence of the analytic component and the enormous diversity is due to the morass of arbitrary elements involved.

**Conclusion**

This very rationalist enterprise proposed by Smedslund pushes "arm-chair" inquiry back into realms once thought securely claimed by empiricism. The plausibility of Smedslund's notion of psychology-as-geometry rather than psychology-as-physics will in part turn on the production of many more detailed analyses such as those he and his colleagues have carried out. The challenge following this article—T + n—is to convert the sketches concerning learning and organized lists and objectives and performance into sets of precisely formulated common-sense theorems. The challenge seems sufficient to motivate someone to perform the task in some circumstances, but sufficient to the article the challenge it started with—in this case merely T.

From Smedslund's perspective, psychologists and educational psychologists have been, as it were, doing geometry the hard way—as an empirical study with imprecise and unreliable measuring instruments. It is time to begin doing it Euclid's way—working out the necessary relationships among our phenomena and establishing a set of axioms that can then be applied to the diversity of the world. The future of educational research, in such a program, would see a dramatic convergence of the skills of the philosopher and those of the empirical researcher.

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