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A Primer of Research on Cognitive Strategy Instruction: The Important Issues and How To Address Them

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Five types of strategy research are reviewed. (1) We argue it makes sense first to determine whether there is a need for strategy instruction. If there is, (2) development of a treatment with preliminary evaluations can follow, as can (3) formal evaluation of the resultant intervention in true experiments. As instructional need research, strategy development, and experimental evaluation proceed, two other types of research should be conducted. (4) It is important to study the acceptability of strategy interventions to educators and students. (5) Research on material modifications can provide information about how strategy benefits can be made available to students when strategy instruction is not effective or unlikely to occur. Very little strategy instruction has been evaluated in all five types of research covered here, making obvious the need for more systematic research on strategies. Observational, ethnographic, and experimental methods are required in order to address the many issues comprising comprehensive empirical analysis of any type of strategy instruction, with many recommendations made here about how to conduct informative studies.

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INTRODUCTION

There is a great deal of contemporary interest in cognitive strategy instruction as part of education, and with good reason. The evidence overwhelmingly favors the conclusions that many strategies improve academic performances and can be taught to students (Miller *et al.*, 1989; Pressley and Levin, 1983a,b). Strategy instruction is an important instructional complement to many educational contents (e.g., Mayer, 1987). This growing interest in and acceptance of it makes likely there will be a lot more research on strategy instruction.

A main purpose of this article is to summarize the types of issues addressed in research on strategy instruction. These are summarized in what we consider a logically optimal progression in Fig. 1. The case is developed here that it makes sense for research to proceed from issues at the top left of Fig. 1 to those in the lower left of the figure: (a) First, determine whether there is a need for strategy instruction. Investigate whether a strategy is now being taught or being used by students even though no instruction has occurred. (b) If students are not being taught or do not otherwise use a presumably effective strategy, there is motivation to develop instruction to promote its use. Preliminary evaluations of the instructional intervention are part of this development process. (c) Once strategy instruction is refined based on pilot testing, it can be evaluated formally in true experiments. As steps a, b, and c are carried out, (d) the acceptability of the intervention should

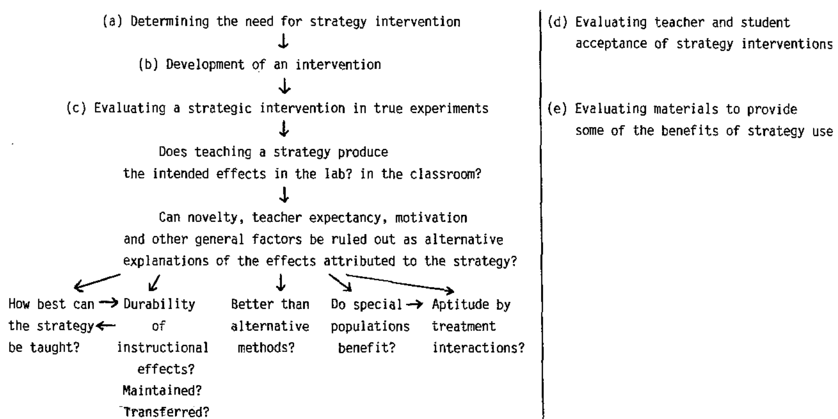

Fig. 1. Issues examined as part of comprehensive evaluation of a strategy. Left portion depicts issues in logically optimal order of investigation from top to bottom. Issues in right portion can be investigated contemporaneously with left-portion issues.

also be evaluated. In addition, (e) it may be possible to alter instructional materials so some of the hypothesized benefits of strategy instruction can be produced even when strategy teaching does not occur (e.g., memory of text can be promoted by author text reorganizations). Development and evaluation of such materials can occur anytime after a strategic intervention is identified as a potentially powerful determinant of performance. That research on acceptability and materials modification can be conducted at the same time as research steps a, b, and c is depicted in Fig. 1 by placing research issues d and e in parallel with a, b, and c. Because at least some research is occurring on all five fronts summarized in Fig. 1, this is a generally upbeat commentary. Nonetheless, none of the issues in Fig. 1 have been studied as thoroughly as needed for fully informed recommendations to be made to the educator community about strategy instruction; some of the issues have been much understudied, in particular, acceptability.

A second purpose of this review is to make clear how the various issues in Fig. 1 can be addressed. Many inadequate studies aimed at these issues continue to appear at conventions and to cross the desks of journal reviewers. This suggests a need for explicit commentary about how to study these questions so that easily interpretable results can be obtained. Thus, this article is a summary of the best "tricks of the trades" we have discovered in the course of conducting strategy research for the past 18 years (i.e., for Pressley; 2–6 years experience for each of the other authors).

We subscribe to a particular general definition of strategy. Explicit statement of it will clarify the scope of this review. A strategy is composed of cognitive operations over and above the processes directly entailed in carrying out a task. For example, since reading a book naturally requires turning pages, looking at print, and processing text left to right, these processes are not strategic. Verbally repeating a to-be-remembered vocabulary term found in a text, and generating and answering "thought" questions while reading text are strategies, since these processes are clearly beyond what necessarily must be done to decode a unit of text. Strategies achieve cognitive purposes (e.g., comprehension, memorizing). During the early stages of strategy acquisition, use is conscious and deliberate. With practice, strategy deployment becomes automatized, requiring little effort or awareness. Even so, a strategy is always *potentially* conscious and controllable (Pressley *et al.*, 1988a; Pressley *et al.*, 1985b). Examples of strategies include (a) planning, drafting, and revising an essay; (b) constructing mental images for to-be-remembered associations; and (c) constructing summaries of text to promote comprehension. The research questions reviewed here are applicable to interventions ranging in complexity from easy-to-teach, single-process strategies (e.g., rehearsal for short-term memory of phone numbers) to very complex, multiple-strategy curricula. Thus, the points made here will be illustrated with research on interventions differing greatly in complexity.

DETERMINING THE NEED FOR STRATEGY INSTRUCTION

Some students receive either formal or informal strategy instruction. Sometimes instruction is effective; other times it is not. Some students discover strategies on their own. Thus, before proposing new instruction, it is important to determine (a) what strategies students are being taught, (b) whether effective methods are being used to teach strategies, and (c) which strategies students are using on their own. Before embarking on such observational work, however, it makes sense to analyze the task of interest (e.g., Gagné and Briggs, 1979). For instance, composing an essay can be broken down into subtasks, each further decomposable. Processes for accomplishing components and subcomponents usually can be identified. These, in turn, suggest strategies for performing the task (e.g., planning-first draft-revision strategies for compositions; Flower, 1989). Armed with such analyses, researchers can gauge the adequacy of various strategies taught by teachers or used by students.

Are Particular Strategies Taught Already? Do They Make a Difference?

Strategies rarely are taught in school, and certainly not for the many tasks and on the many occasions when students could profit from learning them. Review of a few well-known studies makes the point. Durkin (1979) observed more than 10,000 min of instruction in grades 3 through 6, particularly noting comprehension instruction during reading, social studies, and science classes. Little teaching of reading comprehension strategies occurred. Applebee (1981) used a similar research tactic, observing over 13,000 min of high school instruction in order to determine the extent and quality of secondary school writing strategy instruction. He found very little teaching of how to produce coherent prose. Moely *et al.* (1986) observed over 9000 min of instruction to determine memory strategies taught during the elementary years. Although the overall rate of such instruction was not high (i.e., occurring during less than 2% of the observations made), instruction in a number of important memory strategies was detected. On the other hand, most strategies taught were very simple, with no presentations of the more complex memory strategies that can be acquired through instruction during the late grade-school years (e.g., Schneider and Pressley, 1989, especially Ch. 7).

These studies all employed the same basic methodology. The investigators observed a number of teachers for a substantial period of time, coding for teaching of strategies. The investigators had clear preconceptions based on theory or previous research about which strategies could or might be taught, and the observational methods employed were sensitive to such candidate strategies. If anything, these studies were probably biased in the direction of finding evidence of strategy instruction, and yet little was detected. Others also have failed to find evidence of widespread teaching of strategies (e.g., Goodlad, 1984; May, 1986).

The low rate of strategy instruction observed in large-scale studies may be somewhat misleading, however. Finer-grained analyses of expert teachers (e.g., Berliner, 1986; Leinhardt and Greeno, 1986) have produced evidence of systematic instruction of some strategies. For instance, Leinhardt (1987) studied a teacher who produced grade-2 students especially good at subtraction. Leinhardt carefully examined this teacher's coverage of "borrowing" during subtraction of two-digit numbers. Pre-teaching and post-teaching data were collected to document students learning how to borrow during the course of instruction. In addition, interviews were conducted and think-aloud data collected as students performed subtraction problems for the researchers.

Ms. Patrick, the expert instructor in Leinhardt's study, spent a great deal of instructional time making certain students understood the conceptual foundation of the strategic procedures comprising borrowing. There were striking objective improvements following Ms. Patrick's instruction. In fact, some students were able to borrow while solving math problems with characteristics not covered during instruction. For instance, high ability students showed some transfer to three-digit subtraction problems requiring both ones and tens borrowing. In short, expert teachers sometimes teach extremely effective strategies very well.

There is a real need for additional research about what strategies are taught in classrooms. One obvious question is whether teaching strategies well matched to curriculum demands might be an important determinant of expert teaching. Another is whether teachers who teach strategies in one content area do so across the curriculum. Although observation of teaching is certainly a preferred way to study teaching behaviors, it seems likely more efficient mechanisms could be devised. For instance, it should be possible to devise interviews providing enough information about strategy instruction for teachers to judge whether they teach particular strategies to their students. At a minimum, such questionnaire data could be used to screen teachers, to decide which ones might be worth observing in order to capture examples of naturalistic strategy instruction.

In closing this subsection, we note two additonal relevant points. First, even if strategies are taught, they may not always be conveyed exactly as designed by their creators. Better versions of a strategy may be developed by skillful teachers who adapt instruction to fit their students' needs and abilities. Of course, some teachers may misunderstand a strategy and teach an altered and less effective version than the original. Thus, in addition to determining whether strategies are taught, there is a need to determine the integrity of naturalistic strategy instruction (e.g., Harris, 1988)—that is, whether teachers are providing effective versions of strategies.

Second, teachers are not the only potential source of classroom strategy instruction. Strategic techniques could be learned through textbooks or other classroom materials. Whether this is likely has been explored recently. For instance, Ciborowski and her colleagues (Education Development Center, Inc., 1988) reviewed science and social studies texts used in grades 1 through 10 for evidence of strategy instruction. Not much was found. More positively, a new direction in publishing is to produce materials promoting strategy instruction either through in-the-text instruction (e.g., Chamot, 1987a,b) or in-the-teacher's edition guidance to the classroom teacher (e.g., Alvermann *et al.,* 1988; Bereiter *et al.,* 1989; Tuinman *et al.,* 1988). These products are so new, however, very few children have been exposed to them at the time of this writing, and no evaluations are available.

Is There Effective Teaching of Strategies?

Two theoretical camps have provided particularly important general positions in recent years about the nature of effective strategy teaching. A number of instructional psychologists have been persuaded by Vygotsky's (1978) analysis of teaching (e.g., reciprocal teaching; Palincsar and Brown, 1984). Others have followed an expansion and cognitive elaboration of neobehavioral recommendations for good teaching (e.g., Carnine and Silbert, 1979; Rosenshine, 1979, 1983), with Roehler and Duffy's (1984) "direct explanation" method for teaching reading strategies as one of the best developed examples of this approach. Despite different conceptual foundations, Vygotskian and cognitive-behavioral descriptions of effective teaching are similar (Herrmann, 1988; Meichenbaum, 1977; Pressley, 1979; Pressley *et al.,* 1987e).

The Russian perspective is that skilled thinking develops through social-instructional interactions with more capable thinkers. In particular, the more capable thinker in a two-person (often child and adult) instructional dialogue initially controls social-instructional functioning, providing a lot of teaching and guidance as the less capable member of the dyad tries to learn a new skill. As the less capable person improves, explicit instruction is faded, with prompting given only as required. Instruction is scaffolded (Wood *et al.,* 1976) in that the more capable participant initially provides external structure which, like a scaffold, is taken down progressively as a building becomes self-supporting. The less capable contributor to the dialogue eventually internalizes the instructed operations, internalizing as well self-directive verbalizations that in structure and content resemble external directives previously provided by the more capable individual.

Direct explanation theorists place more emphasis on explicit instruction than Vygotskians do, however. In their view, good teaching includes carefully sequenced instruction of the strategic processes required to do a task (e.g., Winograd and Hare, 1988). There is "detailed, redundant instruction and explanations with sufficient examples," "asking [of] many questions and offering [of] numerous overt active practice opportunities," "giving [of] immediate, academically focussed feedback and correction," and "active monitoring of student progress" (all quotations, Winograd and Hare, 1988, p. 122). Feedback and reexplanations are tailored to the needs of the individual student.

There are many reports of strategic teaching as prescribed by Vygotskians and direct explanation theorists, although most such observations have been made outside of formal classrooms.For instance, explicit explanations followed by reexplanations are used by parents teaching memory strategies to their preschoolers (Rogoff and Gardner, 1984). Classification strategies can be passed from adult experts to child novices in this fashion (Adams, 1987). Weaving skills sometimes are conveyed from adults to young adolescents through scaffolded instruction (Childs and Greenfield, 1989; Greenfield, 1984). Moreover, this type of instruction—resembling coaching—extends beyond childhood. For instance, Schön (1987) showed that scaffolding is prominent in the development of architectural skills, professional musicianship, and professional-level counseling abilities.

What can be concluded from these observational data? Teaching sometimes occurs as specified by Vygotskian and direct explanation theorists. The case in favor of scaffolding and direct explanations as *effective* teaching methods could be strengthened substantially, however, by documentation of scaffolded teaching producing especially competent performances. Such associations have not been established in most of the observational studies to date (see Day *et al.,* 1989, for commentary), although most investigators have offered anecdotal evidence for the effectiveness of scaffolding. Moreover, whether strategy instruction usually is scaffolded is hard to say from the observations made to date. Since scaffolding demands much of teachers because it requires constant diagnosis of student difficulties and a great deal of reexplanation (Pressley *et al.,* 1987e), it may be rare, especially when strategies are taught to entire classrooms of students. In short, previous work on naturalistic methods for teaching strategies is neither as extensive nor as analytical as it could be.

What Strategies Do Students Use Already?

Strategies for Specific Tasks

Naturalistic strategy use during specific tasks has received extensive study, in particular by developmental psychologists. As an example, consider the case of memory strategies (Schneider and Pressley, 1989, especially Ch. 3). For some very simple tasks (e.g., remembering where a toy is hidden in a living room), children as young as 2 years of age display strategic behaviors (e.g., continuing to look at a hiding place from the time an object is obscured until it can be retrieved; DeLoache *et al.,* 1985). In fact, identification of strategies used by preschoolers was a prominent part of cognitive developmental research during the 1980s (e.g., Beal and Fleisig, 1987; DeLoache, 1980; Schneider and Sodian, 1988; Sophian, 1984).

The recent work with preschoolers complements earlier descriptive research on the development of more complex strategies in older children. For instance, use of rehearsal during list learning has been mapped out in detail (Schneider and Pressley, 1989, Ch. 3).

Data on naturalistic strategy use in children have been gathered in a number of ways. These include collection of diary data (e.g., mothers noting their children's memory functioning; Ashmead and Perlmutter, 1980), formal observations of memorizing behaviors in controlled tasks (e.g., children looking at the spot where an object was hidden; DeLoache *et al.,* 1985), and inferences about processes from performance patterns (e.g., inferring cumulative rehearsal on the basis of pause-time data; see Belmont and Butterfield, 1977). Self-reports of strategy use also have been collected. Although there has been debate for some time about the degree of correspondence between reported and actual strategy use (e.g., Ericsson and Simon, 1980, 1984; Nisbett and Wilson, 1977), some of these reports must reflect actual cognitive processes (see Ericsson and Simon, 1984, especially). For instance, Pressley *et al.* (1982b) elicited self-reports about elaborative strategies (e.g., images and verbal mediators) used during vocabulary learning. Individual differences in self-reported elaboration corresponded with individual differences in learning of vocabulary-definition associations. This bolstered confidence in the self-reports, as did self-reported use of elaboration correlating only with aspects of learning believed to be mediated by elaborative processing—the self-reports were discriminatively predictive (e.g., Underwood, 1975). Since so much of cognition (including internal execution of strategies) is either difficult or impossible to assess any other way, verbal self-reports of strategy use are employed widely in research aimed at determining naturalistic strategy deployment (e.g., Garner, 1988).

Researchers often do not rely on any of these procedures exclusively, however, for the most convincing cases of strategy use can be made when several indicators point to use of the processing in question. See Pressley *et al.* (1988b) for an example of how memory performance, think-aloud, and other self-report measures were combined to diagnose differences in use of a particular memory strategy.

General Strategic Tendencies

In addition to self-reports tied to particular cognitive tasks, instruments also have been developed to tap general strategic tendencies. A prominent one is the Learning and Study Strategies Inventory (LASSI) produced by Weinstein and her associates (e.g., Weinstein *et al.*, 1988); another is the Inventory of Learning Strategies designed by Schmeck *et al.* (1977; Schmeck, 1988). Both instruments consist of statements which students either endorse as typical or not typical of them.

Several factors make these scales difficult to interpret. One is the issue of criterion. Students differ in what they mean when they endorse an item like, "I learn new concepts by expressing them in my own words" (an elaborative item on the Inventory of Learning Strategies; Schmeck, 1983, p. 247). These scales also include statements describing symptoms of good and bad processing—that is, the rewards of good processing and the consequences of bad processing. In fact, the Inventory of Learning Strategies subscale (i.e., Depp Processing subscale), which is most predictive of other cognitive outcomes, is composed entirely of symptomatic descriptions (e.g., I can usually state the underlying message of films and readings, I have trouble making inferences). These inventories may be predictive of performance largely because students are aware of how well they do on academic tasks and, thus, the self-reported symptoms correlate with actual performances. Such a relationship in no way would depend on accurate awareness of the actual strategies used, however. More positively, some of the subscales on these devices are composed predominantly of items tapping strategic processing more directly (e.g., I review the course material periodically during the term, I learn new ideas by relating them to ideas I already know), with these subscales also correlating with cognitive performances, although not always as highly as subscales composed predominantly of symptoms (e.g., Schmeck, 1983, 1988). Some investigators now are adapting these instruments by eliminating items not reflecting direct reports of processing (e.g., Ames and Archer, 1988).

New instruments are being developed as well, ones directly tapping processing rather than a mixture of symptoms and processing. Zimmerman and Martinez-Pons (1986, 1988) asked students to respond to six items. Two of them were as follows:

> Teachers often assign their students the task of writing a short paper outside of class on a topic such as your family history. They often use the score as a major part of the grade. In such cases, do you have a particular method to help you plan and write your paper?
>
> Is there any particular method you use for completing your math assignments? (Zimmerman and Martinez-Pons, 1988, p. 285)

Responses to such items were scored for use with a variety of strategies: self-evaluation; organizing and transforming; goal-setting and planning; seeking information; keeping records and monitoring; environmental structuring; providing self-consequences; rehearsing and memorizing; seeking peer, teacher, or adult assistance; and reviewing tests, notes, and texts.

Zimmerman and Martinez-Pons (1988) validated their interview by correlating students' reports of strategy use with students' objective achievement as measured by teachers' ratings of academic competence and performance on standardized achievement tests. Reported use of learning strategies was associated with academic achievement. Rehearsing and memorizing activities, organizing and transforming, seeking peer assistance, and seeking information were particularly predictive of academic achievement.

General strategic tendencies also can be evaluated as students actually *perform* a number of academic tasks, as on the *Surveys of Problem Solving & Educational Skills* developed by Meltzer (1986). The *Surveys* tap the information-processing strengths and weaknesses of 9- to 14-year-olds on a variety of academic tasks (verbal and nonverbal problem-solving, reading, spelling, composition, and math). One-to-one administration permits detailed observation. There is evaluation of whether students adjust their processing depending on the demands of the task. Strategic planning, monitoring of errors, and self-correction are scored. There are measures of whether students are aware of strategies they use and can explain them. Information from both objective performance data and interview probes are combined to provide information about a number of aspects of thinking and problem-solving. Thus, for categorization items on the problem-solving inventory, students are given five geometric patterns, three sharing common features and two differing from the other three in specific details. This task requires subjects to be able to use and coordinate two sets of categorization criteria at once. Students are required both to indicate the set of three going together and explain how the three were selected. The explanations for these problems permit assessment of whether students can shift flexibly from one classification strategy to another.

Meltzer and her colleagues have done a substantial amount of work toward validating the *Surveys* (see Meltzer, 1986). For instance, Meltzer *et al.* (1990) reported on both developmental differences on the problem-solving survey and differences in performance and strategy efficiency and flexibility between normal and learning-disabled students. In general, the *Surveys* provide information about individual differences in academic performances and in processing responsible for academic performances. This instrument should be useful for those who wish to study shifts in general strategic planfulness during childhood.

In summary, information-processing analyses of how strategies can mediate performance on various academic tasks have permitted construction of instruments tapping strategic tendencies. Although most are self-report questionnaires, there is the possibility of assessing general strategic tendencies by watching students perform academic tasks and probing them about their performances, an approach illustrated by Meltzer's (1986) work. More psychometric evaluation of all these instruments would be desirable, including additional reliability information as well as more predictive and concurrent validation. As such work accumulates, confidence will increase that these devices provide telling information about general propensities to be strategic.

Determination of Whether Performance is Mediated by Strategies or Some Other Aspect of Information Processing

There are other methods for providing information about naturalistic strategy use, ones familiar to workers in information-processing (i.e., response latency, error analyses). Some of these permit very sophisticated explanations of cognitive performances. For instance, Siegler and his co-workers examined how the knowledge base sometimes mediates young children's arithmetic performances rather than strategy execution. They have generated descriptions of how children respond to single-digit addition problems (e.g., Siegler and Shrager, 1984), generate answers for multiplication items (Siegler, 1988b), and read vocabulary words (Siegler, 1988a). These descriptions were constructed mostly from inferences based on reaction times and overt indications of strategy use. Such inferences could be made because retrieving a known fact (e.g., the answer to 5×4) can be accomplished much more quickly than any strategy for computing an answer can be executed (Siegler, 1988b). Computational strategies are often easy to discern (e.g., writing down a problem to cue visual associations; Gonzalez and Kolers, 1982).

Siegler and his associates provided data illuminating why, when, and how children rely on prior knowledge vs. why, when, and how they use computational strategies. Important individual differences have been detected, ones not predictable from the child's prior knowledge status alone. For instance, perfectionist students are capable of responding to math fact probes on the basis of well-developed prior knowledge, but often carry out computational strategies just to "make sure."

Discussion and Summary

What strategies are taught and how teachers teach them can be determined through observational methods. Unfortunately, very little of this type of work has been done, so that knowledge of naturalistic strategy teaching is sketchy at best. Much more observational work is needed, especially since future studies of strategy teaching might yield substantially different outcomes than obtained in research to date: Strategy instruction is a topic of mounting interest (e.g., Jones, 1988; Jones *et al.,* 1988), and many recommendations about strategy instruction are being made to the teacher corps (e.g., Duffy and Roehler, 1989). Thus, the frequency and quality of strategy instruction might change.

The means of determining the strategies used by students are somewhat more diverse than the observational methods used to determine what is taught. Strategy use can be detected with objective measures such as reaction time as well as more subjective measures such as self-report scales. Much of the work on naturalistic strategy use has been conducted by developmental psychologists on basic tasks. Even young children use some simple strategies. Nonetheless, many sophisticated strategies are rarely used even by mature students, such as the matriculants at leading universities (e.g., Christopoulos *et al.,* 1987; O'Malley *et al.,* 1988; Pressley and Ghatala, 1988; Pressley, *et al.,* 1990; Snyder and Pressley, 1989). Much more research is needed, however, before the development and extent of strategy use are understood fully. Such work should be a high priority because the teaching of strategies can be improved if it is known what students do and fail to do in the absence of instruction. Observational-correlational studies of strategy teaching and strategy use can be helpful, especially in determining whether a particular type of strategy instruction might improve performance: (a) If it is a potent strategy, students naturalistically taught the technique should outperform those who never learn it. (b) Moreover, students using a potent strategy on their own often outperform students not using it. These are minimal-cost screening tests for the possibility that some form of strategy instruction improves academic performances (Underwood, 1975).

In short, observational studies can demonstrate a need for strategy intervention when it is determined (a) there are students either who are not taught a strategy, are not taught the strategy effectively, or do not use it on their own, *but* (b) those who use the strategy outperform those not using it. A completely logical developer of instruction would always seek to establish need before proceeding to design instruction (see Underwood, 1975).

DEVELOPMENT OF AN INTERVENTION

Some strategy instruction is devised by researchers, but much of it is developed in schools by curriculum specialists and other educators. Often, teachers extensively review and revise an intervention until there is confidence the package "works." Unfortunately, from the researcher's perspective, most educator-devised interventions never are evaluated using true experiments. To be fair to educators, however, many researchers are so determined to produce true experiments, they expend few resources developing the instruction they are testing.

Researcher and educator collaboration is a better model. The educational tasks now being addressed by strategy instruction researchers require complex strategies matched to a number of demands. They can be helped by teachers trying out theoretically derived strategies, who then provide input to the researchers about how to circumvent obstacles to school implementation. The extensive development of the University of Kansas learning strategies curriculum is one example of scientist-educator collaboration (D. D. Deshler, personal communication) as is the development of reciprocal teaching by Palincsar and her teacher associates (a method for improving reading comprehension by scaffolded instruction of four strategies; Palincsar *et al.,* 1988a,b).

An approach often denigrated for not providing cause-and-effect data can be very useful during development of an intervention. Uncontrolled pretest-to-posttest evaluations document whether people perform better following strategy instruction than before it took place. Although determining posttest peformance exceeds pretest performance does not permit concluding the intervention caused the gain, failing to find a pretest-to-posttest gain is a signal a strategy does not improve performance.

Scriven's (1967) distinction between formative and summative evaluation is important here. During formative work, the treatment is fluid, subject to change. Formative evaluation is to improve the intervention before making a summative evaluation on a now fixed treatment. The pretest-to-posttest design is especially well suited to formative evaluations. Extensive formative evaluation should produce an improved version of the intervention; a successful summative evaluation should be more likely given such a carefully developed and piloted version of the treatment. Such success is important, for single failures of summative evaluation can mark the end of interest in a treatment (e.g., Norman, 1988, p. 29).

If the task analysis is right, the strategy instruction accommodates task demands, and the researcher takes advantage of the feedback afforded by pretest-to-posttest cycles, pretest-to-posttest gains should emerge eventually. Assuming the gains are large relative to the cost of the intervention, the question remains whether they are produced by the strategy or more general factors (e.g., practice with the pretest; maturation; Campbell and Stanley, 1966). Only experimental evaluations of strategy instruction (covered in the next section) can provide such information. What is emphasized in closing this section, however, is how much can be gained from the nonexperimental pretest-to-posttest data.

Several attempts at an intervention, each a revision based on a previous pretest-posttest evaluation, should produce insights about various ways to present a strategy. Ethnography may be an especially good way to present such information (e.g., van Maanen, 1988). Ethnographic techniques include some procedures already used by strategy researchers, most notably structured observations and interviews. Ethnographers complement their carefully planned measurements, however, with informal observations and interviews (e.g., Wolcott, 1988). For example, efforts can be made to determine how participants perceive situations—what tasks and strategies mean to students. Ethnographic descriptions of formative efforts can be filled with qualitative insights about conditions favoring success of the strategy intervention in question as well as information about factors probably undermining treatment effects.

Unfortunately, strategy researchers in the past have not made it their business to produce such commentaries. Such work is definitely needed. Many insights developed during formative evaluation about the intervention might prove valuable to practitioners trying to translate the strategy into their own settings. In addition, such notes could be of great benefit to other scientists interested in replicating the treatment as exactly as possible. It is a truism that the procedure sections of many formal scientific articles on strategy instruction are not sufficiently detailed to permit a realistic replication of the treatment.

EVALUATING A STRATEGIC INTERVENTION IN TRUE EXPERIMENTS

Complete evaluation of a strategy intervention involves addressing a number of issues summarized in the bottom left of Figure 1. Each is reviewed in detail in this section.

Does Teaching a Strategy Produce the Intended Effects?

Laboratory Experiments

A minimal experimental design involves two groups of subjects formed vis-à-vis random assignment, with one receiving the instructional intervention and the other (control) not receiving it. An experimenter carries out the study with one child or at most a few children at a time in a quiet setting. There is great experimenter control. If the treated group differs from the control group in expected ways following the intervention (e.g., performance is improved), the treatment *probably* contributed to the postintervention difference. The case is strengthened if the treated and control groups did not differ on relevant performance variables before the treatment was introduced (Campbell and Stanley, 1966).

Most studies of one-to-one or one-to-a-few instruction involve brief strategy training appropriate for simple tasks (e.g., rehearsal of word lists; Ornstein and Naus, 1978). The great costs of instructing one or a few children precludes the possibility of such studies extending for more than a few sessions, let alone for months or years. A number of strategies are simple enough, however, they can be taught in one or a few sessions with benefits apparent immediately following training.

Classroom Experiments

"Classroom" experiments can involve teaching either entire 30-children classes or smaller groups (e.g., 7- to 15-child reading groups). Sometimes a researcher does the teaching; sometimes the usual classroom teacher presides over the group. The former arrangement increases the likelihood teaching will occur as conceptualized by the researcher; the latter has greater ecological validity. Most critically, however, classrooms are assigned randomly to conditions in true experiments.

Classroom studies can be very difficult to conduct. Many students must be tracked. Obstacles must be overcome for the treatment to be delivered consistently as intended—perhaps training of teachers, monitoring of day-to-day instruction, recording absentees, and obtaining data to determine how students interpret instruction. When strategy instruction is long-term (e.g., a month or a year or more), there are the concerns associated with any longitudinal study (e.g., selective dropout). In addition, schools may be hesitant to participate given the logistical difficulties of providing instruction to whole groups of students (e.g., scheduling hassles associated with getting children to the right places at the right time). Perhaps because of costs and organizational challenges, the usual classroom experimental design involves only two conditions—a strategy-instructed group and a no-treatment control condition.

Even such a simple setup can pose formidable analyses problems in the case of classrooms. Consider the units of analysis controversy (e.g., Levin, 1985). Suppose 5 classrooms with 30 children apiece participate in an experimental condition and 5 classrooms of 30 children apiece in a control condition. Should each classroom mean be used as the unit of analysis for testing differences between conditions (i.e., $df = 4$ per condition in this example) or should each student's score be the unit of analysis (i.e., $df = 149$ per condition in this case)? The more defensible statistical position is to use the classroom means so that errors are independent. This concern is often ignored, however, because low statistical power is a frequent consequence of using classroom means rather than individual scores (Levin, 1985). Perhaps such power concerns will be less prominent as researchers gain savvy analyzing data involving only a few cases per condition (Levin *et al.,* 1978).

Designing simple two-group classroom experiments, rather than three-, four-, or larger-group studies, is often an acceptable way to economize in order to deal with classroom demands. Other economies, however, completely compromise a classroom experiment. Most critically, the principle of random assignment often is compromised (for detailed analyses, see Lysynchuk *et al.,* 1989; also, Riecken *et al.,* 1974). Consider one scenario. A school district agrees to participate in a study, but is only willing if teachers can volunteer into the treatment condition. Perhaps half the teachers agree to try an experimental treatment. Classes with teachers who did not volunteer are considered the control participants, because business as usual continues in those classrooms. Even if there are no differences on critical dependent variables between treatment and control classrooms at the beginning of the study, there is no guarantee any posttest difference favoring the treatment group is due to the instructed strategy. Perhaps teachers who volunteer to try an innovation are those who have a better track record with new approaches; perhaps more enthusiastic teachers are more likely to volunteer. In short, the interpretive problems following failures to randomize are severe.

One recommendation is to abort a study in a school district in which only volunteer teachers could provide the treatment if another district might be more open to random assignment. A less drastic alternative may be possible if there are sufficient volunteer teachers and the intervention is brief. Half the volunteers could provide the treatment to their students now, with

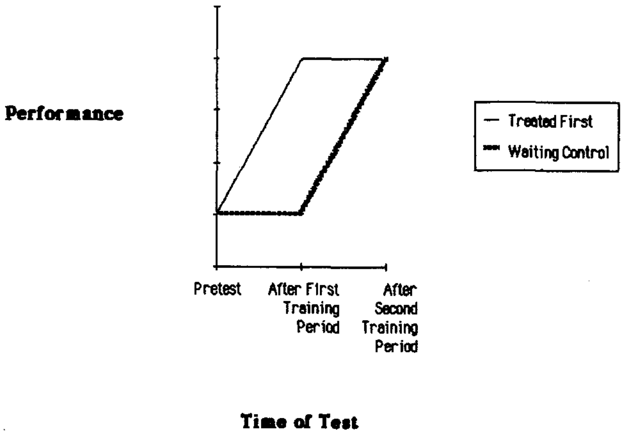

Fig. 2. Ideal pattern of outcomes in "waiting" control condition design if only the intervention affects performance.

the remaining volunteer teachers and their classrooms serving as controls in waiting (e.g., Cook and Campbell, 1979; Riechen *et al.,* 1974, Ch. 6). Both the treated and waiting classes would be assessed during the initial period of the study. Once the treatment was provided to the first-half classrooms, performance in treated classrooms should exceed performance in control classrooms. Then, the intervention would be introduced to classes in waiting. If the treatment was effective, there should be improvement in performance in these classrooms only after the intervention is introduced. The pattern of outcomes indicative of a positive intervention effect is depicted in Fig. 2. There is an important disadvantage to this design, however. All conclusions are limited to volunteer teachers; that is, there is a limit on external validity (Bracht and Glass, 1968). The advantage of moving to a school permitting random assignment of teachers to conditions would have been greater generality of conclusions.

Ruling Out Alternative Explanations for Intervention Effects

Suppose strategy-trained students outperform control condition participants. The conclusion the trained processes improve performance still may be unwarranted. The rejection of the null hypothesis could have been due to factors confounded with strategy instruction. For instance, the strategy instruction effect may reflect student reaction to novel teaching (e.g., Smith and Glass, 1987, Ch. 6) and, thus, student arousal, motivation, or interest may be heightened in the treated condition. Alternatively, teachers' expectations might change, accompanied by subtle encouragement of students or enthusiasm for the strategy, followed in turn by increased student motivation (e.g., Pygmalion effect; Rosenthal and Jacobson, 1968). If the strategy-trained participants know the goal of the study is to evaluate the strategy, they may be especially motivated to prove its worth (i.e., Hawthorne effect; Campbell and Stanley, 1966).

Control conditions could be generated to deal with these possibilities. For instance, in addition to the strategy instruction condition (subsequently referred to as A) and the no-instruction control condition, another cell could be added. This one would involve instruction of a different strategy (B), one not affecting the same performances A affects. If performance on the task presumably mediated by strategy A were equivalent in the A- and B-instructional conditions, and greater than in the control condition, nonspecific motivational factors such as novelty, Pygmalion, and Hawthorne effects might account for the positive effects relative to the control condition (e.g., Smith and Glass, 1987, Ch. 6).

Instructional experiments often do not include conditions to evaluate nonspecific motivational effects, however. This is defensible when hypotheses about treatment effects are refined to the point significant effects are expected for some dependent variables but not for others. Then studies can be designed including both dependent variables hypothesized to be affected by the treatment and those presumably not movable by the intervention. If the treatment produces gains only on variables it hypothetically should affect, the case in favor of motivation is reduced. General motivational effects would be expected to be pervasive, affecting performance on most or all dependent variables. For more extensive discussion of such instructional group by task interactions and their interpretation, see Cole and Means (1981, Ch. 4) and Rohwer (1976).

An example of this approach was provided by Lysynchuk *et al.* (1990) who conducted a follow-up investigation of Palincsar and Brown's (1984) reciprocal teaching, a treatment comprised of scaffolded teaching of four reading comprehension strategies. Reciprocal teaching promoted standardized reading comprehension relative to a control condition, with the effects produced by reciprocal teaching limited to comprehension measures (i.e., standardized vocabulary performance did not improve with 13 days of comprehension instruction). This pattern of differential effects heightened confidence that the comprehension gains in the treatment condition were not reducible to nonspecific motivational influences.

Economy is always a factor to consider in experimental design, but is even more so in the case of long-term classroom instruction. A number of strategy instructional procedures being developed take weeks, months, and even years to implement. Research on such procedures requires longitudinal experiments, with measurements taken for the duration of the treatment. The costliness of this approach should be obvious, as should the advantages of the two-group design compared to more complex alternatives. Since longitudinal analyses permit collection of many dependent variables, long-term studies can include both measures presumed to be affected by the intervention and those believed not to be affected by it. Collection of the latter variables can help to rule out motivational explanations of instructional effects on the former variables. This tactic for evaluating potential alternative motivational explanations of treatment effects will almost always be cheaper than adding longitudinal control groups.

Within-Condition Analyses of Processing

Only mean differences between strategy instructional and control conditions have been discussed in this section thus far. Analyses of within-condition differences in processing can be revealing, however, using many of the process measures covered earlier in the discussion of naturalistic strategy use. Suppose the task is the study of text. Students can be observed for overt indication of study and restudy (e.g., page-turning for review, making notes). They can sometimes tell investigators about their reading plans. Alternatively, students might be asked to watch a video tape of themselves reading material, commenting on what they *were* doing as they read (e.g., Marx *et al.,* 1985). These measures of process are probably most helpful if gathered both before and after an intervention, so pre- to postintervention shifts in processing can be detected.

If greater within-condition use of a process is manifested by students who perform more capably than other students in the condition (either strategy-instructed or control), it is correlational evidence that use of the process improves performance. If the correlation is maintained when other potential explanatory relationships are controlled statistically (e.g., between intelligence and performance), support increases for the hypothesis within-condition performance differences are due to differences in use of the process.

What about between-condition differences in processing measures? Suppose performance in the instructed condition exceeded performance in the control condition following training but not before. If there were also strategy-processing differences between trained and control conditions following training but not before, the case would be strengthened that strategic processing determined the end-of-study performance difference.

Alternatively, if there is no treatment effect, within-condition analyses can be revealing about the null effect. If there were pretest-to-posttest increases in trained processes in the instructed condition, but no corresponding shifts in the control condition, processing changes produced by the intervention do not mediate the performance indexed by the dependent variable.

If there is no instructional effect on performance or pretest-to-posttest shifts in strategic processing, there are several possible explanations. Perhaps subjects learned the instructed processing but were not motivated to use it. They may perceive it as difficult, boring, or childish. This can be determined by offering incentives for subjects to perform the task as taught (cf., Bandura, 1965). If processing shifts with introduction of the incentive, the motivational explanation would be supported; failure of the incentive to shift processing would suggest instruction did not produce learning of the intended processing. Another possibility is subjects were using the trained processes before instruction was provided (see Rohwer, 1980a,b). In this case, pre-intervention processing measures are invaluable. Great use of the instructed processing before the intervention occurs is a clear signal that instructing the process may not shift processing or performance very much.

Summary

Initial evaluation of a strategy's effectiveness can be determined in laboratory and classroom experiments. An especially compelling case can be made for a strategy affecting processing as intended when there are post-treatment improvements only on variables hypothesized to be affected by the strategy. More generally positive effects (e.g., all variables are affected by the strategy instruction) may be due to novelty or other general motivational effects. Within-condition process analyses often provide revealing information complementing tests of mean differences between strategy-instructed and control conditions.

What Are Effective Methods for Teaching Strategies?

Suppose a strategy is known to improve some aspect of performance when students use it—that is, it proves potent in true experiments such as those described in the last subsection. Designing instruction so students use the strategy consistently but appropriately is no small challenge. That some expert teachers provide scaffolded instruction and extensive direct explanations of strategic procedures (reviewed earlier) is motivating research on how to improve teaching of strategies, as are the Vygotskian and cognitive-behavioral theories of good instruction described earlier. New products being offered by publishers also make obvious the need for evaluation of strategy instruction guided by print media.

Scaffolding and Direct Explanation

Two experiments testing specific recommendations about "how" to do strategy instruction are reviewed here. The general designs of these studies could be used to evaluate many hypotheses about doing strategy instruction well.

Duffy *et al.* (1987) studied "direct explanation" of strategies. This method emphasizes explicit explanation of mental processes involved in executing strategies. Explanations are reiterated over instructional episodes with emphasis on when the strategy should be used and how it can be applied. Student practice of strategies follows, with additional explanations provided in response to difficulties encountered by pupils. Much explanation is provided by teachers thinking aloud, describing their own invisible mental processes to students as they show them how to accomplish important educational tasks (Duffy *et al.*, 1986). Direct-explanation teachers assume students will misunderstand initial instruction. The teacher's job is to provide additional input to improve students' comprehension of the strategy. Strategy use is shifted gradually from being teacher-directed to being student self-regulated. The goal is students using instructed strategies on appropriate occasions.

Duffy *et al.*'s (1987) experiment included two conditions. In both, grade-3 teachers taught the skills and strategies usually included in grade-3 reading. In one group the teachers used direct explanations to inculcate these skills; in the other, teachers taught reading skills as they usually would. Consistent with Duffy *et al.*'s (1987) hypothesis, direct explanation teachers produced greater reading achievement gains during a year of instruction than did control teachers.

Bereiter and Bird (1985) studied teaching of four reading comprehension strategies: restatement, involving rephrasing of difficult texts into simpler terms; backtracking when comprehension problems are encountered; anticipatory questioning, involving formulation of expectations about to-be-read content; and problem attack skills to deal with difficulties in text. Three conditions in the experiment are relevant here. In each, teaching occurred in groups of 6 to 8 children. In one, the four strategies were modeled and explained by the teacher. Instruction in this condition resembled Duffy *et al.*'s (1987) direct-explanation procedure. The second condition involved behavioral modeling of the strategies. The third was oral and written exercises designed to stimulate the four strategic processes. In general, the modeling-and-explanation instruction produced greatest use of the strategies and greatest gains in reading achievement.

Print Curriculum That Drives Strategy Instruction

There are a growing number of published products intended to guide strategy instruction in the classroom. Many either have not been tested at all in true experiments or evaluation has been minimal (e.g., data in one or two experiments conducted by the developer of the materials; for commentary, see Mayer, 1986; Pressley *et al.,* 1984b; Pressley *et al.,* 1989a). Sometimes lessons are scripted for teachers; sometimes teachers provide only brief explanations to children, with the teacher assigning workbook exercises intended to promote strategy use. This is a very different model of teaching than scaffolding and direct explanation, one worth evaluating given widespread distribution of these materials.

Many such products will require multiple-year, longitudinal evaluations since they are intended to be implemented over a number of years of schooling. There is opportunity here. For the first time, packages exist to promote long-term strategy instruction. There are also challenges. Evaluating these products can be complex and costly. This is so even with only two experimental groups—for instance, one experiencing a strategy-embellished basal reader series and the other receiving conventional basal teaching. One way to carry out this experiment would be to start two groups of children at each grade level covered by the curriculum, one receiving strategy-embellished basals and the other conventional texts. These two groups would be followed for a number of years. This would permit fine-grained assessment of the effects of strategy-enriched basals (e.g., What are the effects of 1, 2, 3, or more years of instruction begun at grades 1, 3, or 5?). However, it would require combined longitudinal and cross-sectional analyses to interpret (e.g., Baltes *et al.*, 1979), and the maintenance of a number of longitudinal samples—a difficult and expensive venture!

Summary

Experiments are needed in which strategies taught are held constant, with the method of teaching systematically varied. Assessments of how different teaching methods affect long-term use of strategies would be especially informative, for effective teaching should produce long-term, appropriate use of strategies. Thus, issues addressed in this subsection partially overlap those covered next in the discussion of instructional factors affecting strategy durability.

How Can Durable Strategy-Instruction Effects Be Produced?

Some important reviews (e.g., Brown *et al.*, 1983) portrayed continued use of strategies following instruction as a form of litmus test for strategy instruction. True ownership of a strategy presumably should be reflected by consistent, appropriate use of instructed procedures. Although we also view durability as important, our position is that issues of maintenance and transfer should be deferred until it is established that the strategy instruction in question can promote performance at all. Unfortunately, many strategy-instructional interventions are not effective even when students are under strong instructional control to use them. Such strategies are not worth maintaining or tranferring, nor is study of their maintenance and transfer likely to be enlightening. Studying maintenance and transfer of strategies that do promote performance when students are under strong instructional direction to use them should be a high priority, however.

Maintenance

The maintenance issue is whether students continue to use a strategy for situations practiced during training. For instance, if students are taught some reading-comprehension strategies during reading class, will they continue to use the strategies in reading class? Although researchers and theorists are making progress in determining how to increase maintenance (e.g., Borkowski *et al.*, 1987), three types of maintenance failures are still common.

1. Sometimes students do not recognize that a strategy they have learned could be used in a new situation. If so, students sometimes use it given slight prompts (hints) to do so (e.g., Gick and Holyoak, 1980, 1983; Ross, 1984). Little is known about whether it is possible to predict when such prompting will be effective and how much prompting is required in order to activate strategy use, although the amount of prompting required is expected to vary inversely with developmental and intellectual levels (see Day *et al.*, 1989; Day and Hall, 1988; Rohwer, 1973; Vygotsky, 1978).

2. Many maintenance problems reflect student transformation of a strategy to the point it is no longer recognizable or effective (e.g., Harris, 1988). Thus, a complex text summarization strategy involving hierarchical outlining of ideas in text (e.g., Taylor and Beach, 1984, reviewed earlier) might be transformed (distorted) to copying of salient nouns encountered in the text. Important research issues include determining how to reinstate original strategic operations when a student drifts from them and determining how to prevent drift.

What compounds the difficulties associated with studying drift is that some forms of it are good. Strategy instructors generally do not expect students to use strategies exactly as taught, but expect personalization of the techniques. This is part of coming to own them. The diagnosis (Flavell, 1972) complications introduced by such beneficial strategy drift are severe, however. Objective performance can be very high, and students can claim they are executing a strategy. Nonetheless, overt behaviors might suggest they are processing in a fashion different from what was taught.

An important challenge is to determine how to encourage adaptive modifications and to discourage maladaptive transformations. A starting point might be studies of performance when students are using personalized versions of strategies compared to when they are forced to execute strategies as instructed. At a minimum such research should provide information about the types of positive and negative modifications possible. Strategy drift is closely related to an issue covered earlier, teacher modifications of a strategy. Studying these problems in tandem would make sense, especially since some student modifications may reflect teacher transformations.

3. Sometimes students fail to maintain a strategy because they do not like it, perhaps believing any gains produced by the strategy are not worth the effort. At times they are right; the strategy is not worth owning. For example, the SQ3R (*S*urvey, *Q*uestion, *R*ead, *R*ecite, *R*eview) method of reading comprehension is not very effective and is difficult to execute, and thus, many students do not use it even when they know it could be applied to what they are reading (Forrest-Pressley and Gilles, 1983). Other times, students

have not had opportunities to experience gains produced by a strategy, and hence do not realize it is worth owning. Simply hearing an explanation about how a strategy can be executed, or even trying it out, does not provide information about a technique's potency. Often what is required is successful test performance following strategy execution (Pressley *et al.,* 1984c). The impact of test success is especially great when students can compare strategically mediated successes with more typical performances (e.g., Pressley *et al.,* 1988b).

Generalization

Generalization is use of a strategy in a situation different from the training situation. Generalization is usually considered more demanding than maintenance, first of all because it requires recognizing the new situation is somehow similar to the strategy training context, enough so the trained strategy could be applied. Second, strategy transfer often requires some adjustment of the technique. Because transfer is more demanding than maintenance, questions about transfer should logically be delayed until it is determined how and whether strategy maintenance can be achieved. If maintenance does not occur, generalization is unlikely.

The most typical outcome in studies of generalization is transfer failure, especially if there is no explicit attempt to train for transfer (e.g., Borkowski *et al.,* 1987; Brown *et al.,* 1983). Thus, "Will this strategy transfer?" is the wrong question; rather, the question should be, "How can strategy instruction be designed so this strategy is used generally and appropriately?" For instance, increasing students' understanding of strategies they are learning has been identified as one factor affecting strategy transfer (e.g., Borkowski *et al.,* 1987). Increasing understanding of where and when particular strategies can be applied profitably seems particularly critical.

Evidence a component or components promotes generalization can be produced in experiments including two types of conditions, one in which strategy instruction includes the hypothesized critical factor(s) and one with comparable strategy instruction except for the lack of the theoretically critical factor(s) (e.g., O'Sullivan and Pressley, 1984). The potency of the factor in affecting transfer is confirmed if there is generalization in the embellished instructional condition but not in the unembellished condition, or at least, transfer is greater in the embellished condition. What is not known with this design in the case of multiple-component embellishments, however, is which parts of the embellishment contributed to the transfer obtained. When some components are very expensive (e.g., require substantial instructional time) or are particularly important theoretically, it makes sense to determine in subsequent studies whether they really contribute to transfer produced by the package. This can be done by conducting additional two-group experiments comparing the full package with the package minus the component of interest. If the component is critical to generalization—at least in the context of the mix of components comprising the full treatment (e.g., Campbell, 1969), use of the strategy on transfer tasks should be lower in the condition lacking it.

Determining Whether Maintenance or Transfer Is Maximized

Typical designs for assessing strategy durability effects are depicted in Table I. These designs involve administration of strategy instruction to one group at time 1 with task A, but no comparable instruction to a control group at time 1 on task A. Studies of maintenance include an evaluation at time 2 subsequent to time 1 (top panel, Table I). No instruction is provided at time 2 with subjects in both conditions required to do task A. A significant difference between the trained and control conditions in favor of trained students on task A at time 2 is indicative of maintenance. Studies of transfer (bottom panel, Table I) involve an evaluation with another task B, one also mediated by the strategy instructed for task A. Again, no instruction is provided in either condition when task B is presented. Performance differences between trained and control conditions on task B are taken as evidence of transfer.

Although used widely, these designs are not as informative about maintenance and transfer as ones including one more condition (summarized in top panel, Table II), one in which the strategy is explicitly instructed at time 2. If time 2 performance in the previously trained condition is not only greater

Table I. Typical Designs for Assessing Strategy Durability

Maintenance	Time 1	Time 2
Trained	Strategy instruction given before task A performed	Task A performed without instruction
Control	Task A performed without instruction	Task A performed without instruction
Assuming Trained A performance > Control A performance at time 1, then greater task A performance in the Trained condition than in the Control condition at time 2 is indicative of maintenance.		
Transfer	**Time 1**	**Time 1**
Trained	Strategy instruction given before task A performed	Task B performed without instruction
Control	Task A performed without instruction	Task B performed without instruction
Assuming Trained A performance > Control A performance, then greater task B performance in the Trained condition than in the Control condition is indicative of transfer.		

Table II. Improved Designs for Assessing Strategy Durability

Maintenance	Time 1	Time 2
Trained 1	Strategy instruction given before task A performed	Task A performed with instruction
Trained 2	Strategy instruction given before task A performed	Task A performed without instruction
Control	Task A performed without instruction	Task A performed without instruction

Assuming Trained 1 and 2 task A performance > Control task A performance at time 1, then greater task A performance in the Trained 2 condition than in the Control condition at time 2 is indicative of maintenance. If Trained 2 task A performance at time 2 is equivalent to Trained 1 task A performance, then maintenance is maximum.

Transfer	Time 1	Time 1
Trained	Strategy instruction given before task A performed	Task B performed with strategy instruction
Trained 2	Strategy instruction given before task A performed	Task B performed without instruction
Control	Task A performed without instruction	Task B performed without instruction

Assuming Trained 1 and 2 task A performance > Control task A performance, then greater task B performance in the Trained 2 condition than in the Control condition is indicative of transfer. If Trained 2 task B performance is equal to Trained 1 task B performance, transfer is maximized.

than control performance but is also comparable to performance in a group given strategy instruction at time 2, maintenance is maximized—that is, performance is as high as when subjects are under strong instructional control to use the strategy at time 2. Studies of transfer can be made more informative by adding a condition with strategy instruction explicitly given on task B (bottom panel, Table II). If task B performance in the task A-trained condition is better than control performance and is also comparable to task-B performance in the task-B trained condition, transfer is as great as it could be (i.e., when students are explicitly taught how to use the strategy on the transfer task and directly instructed to do so). See Pressley and Dennis-Rounds (1980), Pressley and Ahmad (1986), and O'Sullivan and Pressley (1984) for examples of these designs.

Components Potentially Affecting Durability

A number of components have been hypothesized to affect maintenance. Sometimes the to-be-added ingredient is simple—practicing strategy execution until it is automatic may increase maintenance since it makes strategy use easier and, thus, more appealing (cf., Guttentag, 1984). Other times the additional component is complex, with a number of potential operationalizations. Such is the case for adding motivational components to instruction. Children can be made aware of short-term extrinsic gains following strategy execution (e.g., quicker task completion), longer-term gains (e.g., better grades), or very long-term gains (e.g., people doing school tasks well do better in life). Students can be encouraged to believe they can do well, especially by using the right strategies (e.g., Borkowski *et al.,* 1988; Clifford, 1984; Reid and Borkowski, 1987). They can be taught to self-reinforce contingent on strategy use (e.g., Bandura, 1976). See Borkowski *et al.* (1990) for a review of the many ways motivational elements can be added to strategy instruction.

Sometimes the additional component is teaching another strategy in conjunction with the main strategy of interest, one making obvious the potency of the main strategy. One such strategy is self-testing, since it can produce metacognitive information about other strategies (viz., how well the main strategy of interest works), information making clear why it makes sense to continue using the main strategy (Pressley *et al.,* 1984a, 1985a). For instance, Schön (1987) and Neuringer (1971) explicitly argued students should be taught to self-test the effectiveness of new procedures they try and to monitor carefully whether a new procedure facilitates goals being pursued. Irene Gaskins teaches the bright underachievers at Benchmark School (a private school near Philadelphia initiating ambitious strategy instructional programming; e.g., Gaskins, 1988) to perform "mini-experiments," self-tests of new strategies learned in the school. Self-testing of strategies by both children and adults has promoted strategy maintenance in a number of formal experiments (see Ghatala, 1986, for a good summary; also Pressley *et al.,* 1984a, 1985a; Pressley *et al.,* 1988b; Schneider and Pressley, 1989, Ch. 7).

Generalization is expected if students learn when and where to use strategies they are acquiring (e.g., O'Sullivan and Pressley, 1984), perhaps by practicing strategies in multiple settings (e.g., Stokes and Baer, 1977) or by experiencing a variety of worked examples of strategy application (e.g., Cooper and Sweller, 1987). Generalization is also hypothesized to vary with whether students know how to evaluate their need for strategy use (e.g., through self-questioning to determine whether they are doing well on an academic task; e.g., Meichenbaum, 1977).

In addition to single-component theories of maintenance and transfer, some contemporary models of teaching specify transfer as the product of a number of factors in interaction, a package of critical variables. Thus, the strategy training program devised by Donald Deshler and his associates at the University of Kansas (e.g., Deshler *et al.,* 1984) includes modeling and explaining of strategies by teachers as well as memorizing and rehearsing strategies by students. The utility of strategies is fully explained. Students are taught to monitor the gains following strategy use and to use self-coping statements like, "I can handle this," and "I'm doing well here." Kansas students are instructed explicitly to try to generalize strategies they are learning. Instruction occurs in diverse settings with the instructor gradually loosening control over the student by reducing cues to use strategies. In short, the Kansas approach seems to incorporate all of the elements of direct explanation plus some of the generalization methods proven potent in studies of cognitive behavior modification (e.g., Meichenbaum, 1977).

Summary

The problem of how to produce durable strategy use should be given much additional research attention. The "typical" designs (see Table I) for assessing strategy durability are not enough. These designs can only answer whether transfer occurs at all. Even the improved designs in Table II only address whether transfer is as great as it could be. Once an intervention proves successful in a Table I-typical or a Table II-improved design, evaluation of components can follow. Componential work permits evaluation of theoretical positions (e.g., metacognitive information about where and when to use a strategy is critical to maintenance) and pragmatic questions (e.g., whether self-instruction is worth the instructional time required to develop it). Such analytical work on durability has been conducted infrequently, however, but should be a high priority given its potential payoffs.

Is This Strategy Instruction Better Than Alternative Instruction Aimed at the Same Goal?

Reading comprehension, composition, spelling, vocabulary acquisition, and problem-solving have been taught in one form or another for the entire history of education. Not surprisingly, teaching tactics are already in place for all these educational goals. There is little incentive to adopt new strategy instruction (and to absorb associated teacher-training and materials costs) if a new method is not more effective than approaches already deployed. For example, during the late 1970s and early 1980s, a number of experiments were performed to evaluate keyword-mnemonic techniques for vocabulary learning (e.g., Pressley, 1977; Pressley and Levin, 1978; relevant evidence reviewed in Pressley *et al.,* 1982a). Following demonstrations of keyword method superiority relative to various no-strategy control conditions, the technique was contrasted with semantic-context methods of vocabulary learning. Since semantic-context approaches to vocabulary learning are prescribed regularly by educators, the keyword method was evaluated relative to generation of synonyms for to-be-learned vocabulary, construction of sentences with new vocabulary used correctly, and analyses of sentences containing new vocabulary used correctly. The keyword method proved superior in every semantic-context approach comparison (Pressley *et al.,* 1987c, summarized the evidence).

Such experiments are "horse races," establishing the more effective instruction, the faster horse. Nonetheless, interpretive ambiguity can follow such a race. In one condition, the new strategy instruction is present, and conventional instruction is not; in the other condition, conventional instruction is present, and new instruction is not. A difference in a two-condition horse race can be caused by one strategy facilitating performance or by the other one depressing it (i.e., Did horse A win because its training was superior or because horse B's training was deleterious or inadequate?).

Sometimes the horse race must be substituted for a strategy instruction vs. no-strategy control contrast, since a noninstructed control condition is impossible. Consider the evaluation of a reading strategy curriculum implemented over a number of grade levels. Denying reading instruction to a group of children so they could serve as no-instruction controls would be unethical. Nonetheless, the new curriculum could be evaluated relative to conventional instruction. Whenever possible, however, the horse-race design should be expanded to include a no-strategy control condition. Doing so increases substantially information produced in an experiment. In particular, a no-instruction control permits simultaneous evaluation of new strategy instruction vs. old strategy instruction, new strategy instruction vs. no-strategy control procedures, and conventional instruction vs. no-strategy control procedures.

Do Special Populations Benefit from Strategy Instruction?

Special educators are very interested in strategy instruction since lower-functioning students do not routinely use many sophisticated cognitive processes and strategies, even though at least some strategic processes can be taught to them profitably (e.g., Campione *et al.,* 1982; Harris and Graham, 1988; Pressley *et al.,* 1987b; Pressley *et al.,* 1989b). One concern is whether handicapped students can be taught to use a strategy to improve performance. It is often possible to use the same experimental designs employed with normal subjects. Other times there is a catch. A special populations researcher often does not have the luxury of large numbers of subjects, making it difficult to use conventional experimental designs requiring many subjects per condition. This problem is ever more telling the rarer the special population in question. One mechanism for coping with this situation is the $n = 1$ design (e.g., Hersen and Barlow, 1976; Kazdin, 1982) with as many replications (subjects) as possible. See Kazdin (1976) and Levin *et al.* (1978)

for discussion of analysis methods when there are several replications of the $n = 1$ setup, as well as discussion of other designs for use with limited numbers of subjects. See Graham and Harris (1988a), Graham and McArthur (1988), and Wilson (1986) for examples of $n = 1$ studies with special populations.

The many successes of strategy instruction with special populations (see Ceci, 1987; Brooks *et al.,* 1984) encourage more work. Extensive strategy instructional curricula are being developed for special populations, with favorable evaluations fueling additional research and development (e.g., Learning Strategies Curriculum from the Kansas Learning Disabilities Institute; Ellis *et al.,* 1987a,b). Virtually all experimental questions relevant to strategy instruction with normals (Does it work? Can the strategy be taught in classrooms? Is it maintained? Transferred?) are also relevant to strategy instruction with special populations. See Pressley *et al.* (1990c) for a review of these questions and how they relate to one another in the special case of learning-disabled students.

Even if a treatment is effective with a population studied in initial validation research (e.g., children of normal intelligence), it may not produce gains for special populations. Thus, there is often substantial motivation to study the population generality of effects produced by strategy instruction in some detail. The next subsection is concerned with a particularly analytical approach for doing so.

Are There Aptitude by Treatment Interactions in Gains Following Strategy Instruction?

Aptitude by treatment interaction (ATI; Cronbach and Snow, 1977) research is conducted to determine whether some forms of instruction benefit students with particular characteristics more than other students. ATI research is expensive, involving both correlational and experimental components. Consider a simple case of two experimental groups, one treated and one control. In both conditions, individual differences are assessed (e.g., intelligence, expertise level for some content, short-term memory capacity). The ATI hypothesis is the relationship between an individual difference (aptitude) variable and an outcome variable as a function of condition. That is, if outcome were plotted as a function of the individual difference, the slopes of the plots would vary in the treated and experimental conditions. In order to have sufficient statistical power to detect potential between-condition differences in performance and aptitude associations (Cohen, 1977), it is necessary to have much larger *n*s per treatment than to detect a simple mean difference between two conditions (Cronbach and Snow, 1977). ATI studies often include 50 to 100 subjects per condition. The expenses can be justified if an investigator has a well-formulated hypothesis about how treatment effects may be constrained by some aptitude, an hypothesis theoretically or pragmatically important.

As an example, consider a study reported by Pressley *et al.* (1987a). Children from 6 to 12 years of age were required to learn a series of concrete sentences (e.g., The toothless man slept on the orange couch) so they could recall the entire sentence given its subject as a prompt (e.g., man). Control subjects were left to their own devices to learn the sentences any way they wanted; treated subjects were taught to create images depicting the meanings of the sentences exactly. The interaction hypothesis was the imagery instruction would be more potent for children with relatively greater short-term memory capacity. This was expected because imagery construction demands a great deal of short-term capacity: To-be-imagined sentences must be held in memory. Images of the sentence constituents must be constructed and then combined into a scene capturing the meaning of the sentence (cf., Pressley and Levin, 1978). Children with greater short-term memory should be more likely to be able to carry out and coordinate these constituent processes, and thus, be more likely to benefit from imagery instruction.

One-hundred-thirty-four children were divided equally between the imagery-instructional and no-strategy control conditions. Three short-term-memory assessments were made as was a general verbal ability assessment. As expected, the relationship between short-term capacity and performance was substantially higher in the imagery instructional condition than in the control condition (i.e., expressed as Pearson correlations, rs = 0.71 and 0.40, respectively), even with the effects of age and general verbal ability controlled statistically (i.e., second-order partial $r = 0.54$ in the imagery condition vs. 0.19 in the control condition). A median split of the data was made on the basis of short-term memory. Imagery instruction positively affected performance for children in the top half of the short-term capacity distribution, but had a negligible effect on the bottom half of the distribution. Cariglia-Bull and Pressley (1989) recently reported a similar interaction when children *read* sentences they were asked to learn, rather than heard them as in Pressley *et al.* (1987a).

More aptitude by strategy-instruction interaction research will be conducted in the next several years, largely because there are clear hypotheses about how differences in learner processing abilities should be associated with learner success. Examples include the following: (a) There are many potential strategy by short-term memory difference interactions besides the one investigated by Pressley *et al.* (1987a). Some of these emanate from neo-Piagetian theory (e.g., Case, 1985). Others follow from observations in the mainstream information-processing literature. For instance, Guttentag (1984;

Guttentag *et al.,* 1987) believes children higher in short-term capacity are more likely to maintain capacity-demanding strategies than are children who have less short-term memory. High-capacity-demanding strategies can presumably be executed with less effort by children with relatively greater capacity. (b) Some students already engage in strategic processing, whereas others do not, or at least engage in relatively little of it. Strategy instruction should be most likely to affect the performances of students not already using the trained processes (e.g., Rohwer and Bean, 1973). (c) Only people who possess extensive prior knowledge should benefit from reading-comprehension strategies entailing activation of prior knowledge (Hasselhorn and Körkel, 1986). Similarly, students high in prior knowledge of mathematical concepts and mathematical ability benefit more from mathematics instruction requiring students to interrelate mathematical concepts (Swing and Peterson, 1988). (d) Kurtz and Borkowski (1987) proposed and demonstrated metacognitive differences between children (i.e., differences in what they know about how the mind functions) to predict differences in maintenance of trained reading strategies.

In short, many ATI hypotheses are now a specific type: particular strategies are hypothesized to be more effective for more able learners, ones possessing greater short-term capacity, prior knowledge, or metamemory. This contrasts with a classical notion about strategy instruction, that it has greater impact on low-achieving learners than high achievers because less able students do not naturally produce the strategies better students produce. Although such production deficiencies (Flavell, 1970) occur for some simple strategies (see Schneider and Pressley, 1989), the general production deficiency hypothesis fails on two counts: there are many strategies even competent and mature learners do not possess; and many strategies can only be taught to more capable learners.

Although the search for ATIs has been frustrating in the past (see Cronbach and Snow, 1977), process-oriented strategy-instruction researchers have had some success in validating theory-guided predictions about who benefits from particular forms of strategy instruction. See, for example, Rohwer (1980a,b) as well as some of the studies cited earlier in this subsection. This history of informative research fuels enthusiasm for more such work even though it is expensive.

Summary of the Experimental Evaluation of Strategy Instruction

The strengths of true experiments are well known. Properly controlled studies of strategy instruction permit conclusions about whether the instruction promotes performance. Alternative explanations that might explain uncontrolled pretest-to-posttest gains can be ruled out, including the potential effects of history, maturation, testing, regression to the mean, and selection into treatment. If subject mortality is about equal in the various conditions of an experiment, there is additional reason for confidence that any postintervention differences between conditions are due to the strategy instruction. If all subjects believe they are being treated, the possibility of Hawthorne effects is reduced.

On the other hand, researchers have not been successful in devising schemes to guarantee all aspects of validity. If an experiment is conducted with some particular population, there is always the possibility the treatment effects observed would only be obtained with the population in question. Often reports of the nature of complex interventions are less complete than they could be; it seems certain that at least sometimes scientific writeups fail to include information about factors affecting the success of treatments. Complex strategy interventions may work only if there is a particular mix of components. Although there is substantial theory about what should be included in such mixes, there is little systematic work comparing various blends of components, largely because such work would be cumbersome and extremely expensive. For example, given only two components, each of which could be put in a treatment in either a high or low dosage or not at all, there would be nine possible blends. The number of combinations is astronomically higher when treatments involving 5, 10, or 15 components are considered, especially since the amount of each component might not even be fixed, but rather would vary with the learner and the situation! In short, not all questions about all forms of strategy instruction could possibly be addressed in true experiments.

In fact, some strategy interventions probably will never be evaluated in true experiments at all. Consider one example. Irene Gaskin and her associates at Benchmark School are developing a multiple-year, across-the-curriculum, strategy-instruction treatment for bright underachievers (e.g., Gaskins, 1988). A true experiment would require at least 3 to 5 years, the duration of the total program. A true experimental evaluation of this treatment would also require random assignment of schools to treatment conditions, since the intervention is implemented across the entire school, day, and curriculum. Given we cannot identify a single true experiment on strategy instruction in which schools have been randomly assigned to different treatments for 3 to 5 years, it seems unlikely the entire Benchmark model will be examined in such a study, even with respect to the most fundamental experimental question in the lower left corner of Fig. 1—whether the treatment works at all. The same pessimistic conclusion probably holds for other multiple-year interventions, such as the new strategy-enriched basal readers (e.g., Alvermann *et al.,* 1988; Bereiter *et al.,* 1989; Tuinman *et al.,* 1988).

Other approaches to evaluation are going to have to be used in such cases. The most likely candidates are quasi-experimental designs, involving comparisons between units not assigned to "treatment" and "control" conditions randomly (e.g., Cook and Campbell, 1979; Cronbach, 1982; Riecken *et al.*, 1974). Assuming there are thorough pretests of participating students documenting that samples in treated and control schools are roughly comparable at the beginning of a study, postreatment differences favoring students in the strategy-teaching schools over control students would be supportive of the strategy-instructional model. Unfortunately, however, some alternative interpretations of the data could not be ruled out (e.g., perhaps students seeking and gaining admission to special programs are more highly motivated than control students not enrolled in enrichment opportunities) Even so, quasi-experimental evaluation is better than no evaluation at all. The inability to conduct true experimental evaluations of some important, large-scale strategy packages might be easier to accept by facing up to some additional shortcomings of true experiments, covered next.

Pursuing the Perfectly Controlled Instructional Experiment

Suppose a decision has been made to conduct an experimental study to evaluate whether a strategy promotes some peformance. When the strategy is taught, it will probably be instructed, illustrated, and practiced with sample materials. Conventional experimental design would mandate control subjects be exposed to these same materials for the same amount of time. Or should they? Perhaps illustrating and practicing the strategy takes a while. Maybe the illustrative and practice materials ordinarily would be processed much more quickly by noninstructed children. If so, nominal equation of materials-exposure time for trained and control students might produce functional differences between the conditions, with trained children meaningfully and alertly engaged for the entire time and controls bored by overexposure to the sample and practice materials.

With some very simple strategies, it is possible sometimes to deal with problems of nominal vs. functional equation by adding control conditions. Thus, for the situation outlined in the last paragraph, subjects in one control condition might be exposed to sample and practice materials for the same amount of time as strategy-trained subjects. Subjects in a second control condition could be permitted to process the sample and practice materials for as long as they ordinarily would do so. With large-scale interventions, however, there are many procedural details to be formally equated in order to achieve identity between strategy-trained and control conditions except for the strategy instruction *per se*. The add-control-conditions approach is prohibitively expensive and cumbersome with long-term studies.

How is this problem resolved? For any given experiment, usually one or a few control conditions are designed, ones matched as well as possible to the strategy-training condition. Often these control condition(s) do not eliminate all intervention vs. control confoundings. More positively, many important strategy instruction interventions are tested in a number of experiments relative to a number of different control conditions. When an intervention consistently produces positive effects relative to a variety of challenging control conditions (e.g., Pressley *et al.,* 1987c), the straightforward conclusion is that strategy instruction is effective. Unfortunately, follow-up studies do not always occur after a strategy-intervention vs. control difference favoring the instructional condition, especially when the treatment is extensive and expensive, with any procedural difference between trained and control conditions potentially accounting for the between-condition performance differences. Two important principles follow in evaluating strategy-instruction experiments:

(a) Do not expect absolute comparability between instructed and control conditions. Many two-condition experiments evaluating complex interventions are going to involve a number of differences between the treated and control conditions besides the strategy-instructional elements. Given the inevitability of such confoundings, it makes no sense to discard (disregard) studies when *any* confoundings are found, especially if they are minor, such as the practice materials exposure problem discussed earlier.

More problematic is when there is a sense that all little nuisance variables favor the instructed condition. Even this situation is sometimes acceptable, at least when the intervention is not so extensive that follow-up studies are unlikely. Why? As mentioned earlier, interventions failing a first test are often forgotten. Thus, there is heuristic value in a successful intervention vs. control contrast. A successful (albeit "loaded") first test is much more likely to be analyzed in follow-up studies than a nonsignificant intervention vs. control comparison, with three likely outcomes in the follow-ups: (1) Generally effective interventions should easily survive follow-up scrutiny. (2) Some interventions will prove potent, given only certain combinations of the nuisance variables. That is, their effects will be context specific. (3) Interventions working only when all of the nuisance variables are biased in their favor will fare poorly in subsequent studies.

The real value of a "loaded" first test is it maximizes the likelihood that interventions fitting the second case will be discovered. If perchance the first test was conducted with values of nuisance variables not associated with intervention benefits, the tendency to ignore treatments failing a first test might preclude discovering that the treatment does in fact work some of the time.

(b) Because any given study was conducted in a particular setting with a particular population, there is no guarantee the positive strategy intervention vs. control difference obtained would occur if other particulars held. Follow-up studies are always necessary to determine if an obtained effect is robust across settings and populations (e.g., Bracht and Glass, 1968). Thus, the second recommendation is to plan programmatic research, with replication of the most important treatment vs. no-strategy instruction contrasts.

Given real-world exigencies (e.g., funding agencies often do not support replication work; journals often will not accept replication experiments), this is not always an appealing possibility. Replication contrasts, however, can be included as part of experiments addressing new issues. Thus, keyword vs. no-strategy control comparisons were made in studies aimed at elucidating the development of imagery skills (e.g., Pressley and Levin, 1978), determining when people monitor strategy efficacy (e.g., Pressley *et al.*, 1984c), and establishing conditions fostering transfer of effective strategies (e.g., O'Sullivan and Pressley, 1984; Pressley and Dennis-Rounds, 1980). Notably, the replicated contrasts varied substantially with respect to a number of nuisance variables (e.g., rate and mode of presentation). It is sometimes simultaneously possible to advance to new problems and to make additional intervention vs. control comparisons.

See Cronbach (1982, especially Chs. 4 and 8) for additional commentary on many of the themes raised in this subsection.

EVALUATING ACCEPTANCE OF STRATEGIES BY STUDENTS AND TEACHERS

Change can be extremely difficult to accept and is often resisted by students and educators (e.g., Fullan, 1985). For instance, Margolis and McGettigan (1988) summarize some of the reasons for teacher resistance to new strategies introduced into a curriculum:

> They [teachers] may perceive recommended strategies as (a) dependent on more support than is available (Heron and Harris, 1987), (b) contingent on special training they lack (Madden and Slavin, 1983), (c) incompatible with the needs of other students (Madden and Slavin, 1983), (d) time consuming (Martens, Peterson, Witt, and Cerone, 1986), (e) unlikely to produce desired results immediately, (f) only partially understood, and (g) imposed on them without their substantive participation. (p. 16)

Duffy and Roehler (1986) encountered similar problems of implementation in their research on strategy teaching. Specifically, they observed that teachers often experience difficulties presenting and explaining strategies. Sometimes teachers felt strategies should not be added to the curriculum because they were not included in district mandates. Moreover, teachers often noted teaching of strategies is more demanding than other forms of teaching, such as relying on drill-and-practice sheets. See Pressley *et al.* (1989a) for a thorough review of these and other impediments to strategy instruction.

In addition to reasonable concerns, there are some irrational points of view. Some perceive strategies as threatening or even subversive. For instance, there have been public accusations that strategy curricula brainwash children and are consonant with a "New Age" approach to one-world government. One example is an attack on publications produced by the Association for Supervision and Curriculum Development (Association for Supervision and Curriculum Development, 1988). Given such potential for perceived difficulties with strategies and strategy instruction, it makes sense to determine whether particular types of strategy teaching are acceptable to students and educators.

Treatment acceptability studies are now common in clinical psychology (e.g., Elliott, 1988). Cognitive strategy researchers, however, only are beginning to do this type of research. For example, Harris *et al.* (1988) asked over 200 special education teachers about the acceptability of teaching self-monitoring (i.e., teaching children to record and chart work completed) and self-instruction (e.g., teaching children to prompt themselves to stay on task, to cope with frustration, and to self-reinforce academic progress). The teachers read scenarios describing academic problems some special education students experience, including both serious (e.g., severe academic deficiency and behavioral acting out) and mild (e.g., attentional) problems, all of which could be handled by teaching either self-monitoring or self-instruction. Teachers then evaluated whether self-monitoring or self-instruction would be acceptable as part of treatment, with concrete examples of implementation for both mild and severe problem levels. In general, the teachers viewed both self-monitoring and self-instruction positively.

An extensive teacher acceptability study was recently completed at the University of Western Ontario (Rich and Pressley, 1989, 1990). The study tapped teachers' perceptions about eight reading-comprehension strategies known to be effective (Pressley *et al.*, 1989b) and six curricula incorporating reading strategy instruction. The participating teachers were interviewed, with each intervention described in detail, including a summary of its known effects. Once the nature of a treatment was understood, each teacher was probed about its acceptability. Although most of the interventions received positive evaluations, there were large differences in acceptability, with some treatments viewed much more positively than others. For instance, prior knowledge activation was considered the most acceptable of the individual strategies; one basal reader series incorporating strategies received higher marks than other publisher products designed to promote use of strategies.

Nonetheless, the acceptability studies just described do not fill some very large gaps in the strategy-instruction literature. Little is known about teacher or student reactions to strategy instruction as they try it. More positively, both Palincsar (personal communication, November, 1988; Palincsar *et al.*, 1988b) and Deshler (personal communication, November, 1988) and their associates are trying to find out how teachers' and students' acceptance of their interventions (reciprocal teaching and the Kansas Learning Strategies Curriculum, respectively) change with experience. For instance, in a study in the Wichita schools, Frank Kline (personal communication, November, 1988), a member of the Kansas Learning Disabilities Institute team, has generated data indicating that teachers continue to use the Kansas approach once they start. In fact, the Wichita teachers are making great efforts to extend and refine their strategy teaching. Students also seem accepting of strategy instruction. For instance, Pressley *et al.* (1984c, 1988b) observed clear student preference for a novel, powerful strategy compared to a familiar, less potent approach following one opportunity to compare the new and old strategies.

Much work needs to be done to determine how best to conduct research on teacher and student perceptions of interventions and shifts in those perceptions as a function of experience. Some standard interviews exist for collecting evaluative information about instructional innovations (e.g., Hord *et al.,* 1987), and these may be helpful, especially if modified slightly to be sensitive to dimensions important in strategy instruction. For instance, Deshler and Kline are using Parker and Griffin's (1974) *Stages of Concern Questionnaire,* tapping a number of concerns at different points in the adoption of an innovation. The acceptability instruments used by Harris *et al.* (1988) and in the Western Ontario study might be modifiable as well to study acceptability of particular strategy treatments following long-term exposure and experience with strategies.

Nonetheless, less standard interviews might even be more informative. Mishler (1986) for one has argued persuasively that social scientists can collect especially incisive interview data by interacting with others (in this case, students and teachers) as research collaborators rather than as the objects of research (notably, Palincsar and the Kansas group are consistent with this collaborator approach). Thus, rather than asking for ratings or rankings of potential determinants of acceptability considered critical by the researcher, more open-ended questions might be more revealing—for instance, asking teachers and students to generate factors determining their willingness to try or to continue using a form of strategy instruction. Teachers could also be observed teaching strategies, with commentary collected when they thought instruction was going well and when it was not; these observations could later be discussed, with teachers providing explanations about why instruction worked well on some occasions and not others. If teachers were observed for long enough, there would be data about whether and how strategy instruction improved or deteriorated. Good follow-up interviews might be very revealing about the reasons for the changes occurring with experience. Students could be monitored when they first encountered a strategy, as they practiced it, and after they had it mastered. Such data would be revealing about difficulties experienced in learning a strategy. Students might be able to provide insights about the source of the difficulties and whether there were ways to make acquisition of the strategy easier and hence more palatable. The data produced by more subjective interviewing in response to problems experienced by teachers and students are likely to be messier to analyze than data obtained in response to standard questionnaires. Nonetheless, the personally embellished information produced by this approach is likely to be more meaningful to the educator community than the results of a rigidly standardized survey.

Informative commentary from teachers and students about strategy instruction acceptability is especially likely to the extent teachers, students, and researchers have an open and honest relationship. It helps if the teachers and students really believe the researcher wants to know both the strengths and weaknesses of the intervention. Mishler (1986) goes so far as to argue for researchers as advocates, making clear to teachers and students that the research is being conducted to determine how to improve interventions. This type of interviewing could only be done by researchers who spend a lot of time in classrooms where strategic interventions are deployed. They must be thoroughly familiar with students and teachers, their environments, needs, and constraints. Interviews with teachers and students are dialogues, with all participants determining what is discussed (e.g., van Maanen, 1988, Ch. 4; Wolcott, 1988). In fact, participant-scientists, teachers, and students might coauthor summary articles about the acceptability of strategy instruction (cf., Clifford, 1983) to maximize the likelihood teachers' and students' interpretations are represented accurately in scientific reports. At a minimum, final reports should be checked carefully by students and teachers to assure the researchers' claims are justified (e.g., Lincoln and Huba, 1985).

Beyond simple determination of teacher and student perceptions and opinions, however, researchers are interested in evaluating hypotheses about how to increase the acceptability of strategy instruction. One hypothesis enjoying some support (e.g., Crandall, 1983; Margolis and McGettigan, 1988) is teacher and student commitment occurs when students experience success using a strategy (Doyle and Ponder, 1977; Fullan, 1985; Ghatala, 1986; Guskey, 1986; Witt, 1986). Teacher and student acceptability also might be influenced by whether teachers and students have a role in determining what strategies are taught (e.g., Deshler *et al.,* 1984; Johnson and Johnson, 1987). Teacher accceptability may be increased if they are supported in acquiring technical competence needed to teach strategies (Meyers *et al.,* 1979); if assistance in implementing strategy teaching is provided (e.g., Deshler and Schumaker, 1987; Hawryluk and Smallwood, 1986); if strategy instruction is valued by administrative superiors (e.g., Coleman, 1984); and if information about strategies is presented in a comprehensible fashion (Doyle and

Ponder, 1977; Coladarci and Gage, 1984). Strategies taught without purchase of special materials may be more attractive to teachers than other interventions (e.g., Witt, 1986). It helps if the strategy is consistent with the teacher's philosophy of education and current teaching practices (Doyle and Ponder, 1977; Woolfolk *et al.,* 1977). Strategies easily incorporated into ongoing instruction should be more readily acceptable to teachers than "ecologically intrusive" strategies (Witt, 1986). Unfortunately, none of these hypotheses about factors affecting the attractiveness of interventions have been evaluated in detail. Determining how to enhance strategy instructional acceptability should be a high priority in future research, since whether strategy instruction and strategy use occur depend greatly on their appeal to teachers and students.

EVALUATING MATERIALS DESIGNED TO PROVIDE SOME BENEFITS OF STRATEGY USE

What if it proves impossible to teach some children to execute a strategy on their own, perhaps to a special population? Even if it is possible to teach the procedure, what if there is insufficient opportunity to do so? What about teachers who do not teach strategies? In all three cases, interventions are needed in lieu of strategy instruction.

(a) Materials can be modified to provide content recodings resembling those otherwise created through strategy execution. For instance, children can be given pictures completely overlapping meaning in text. If anything, such reiterative pictures improve learning even more than constructing mental images (Levin and Lesgold, 1978; Pressley and Miller, 1987). Texts can also provide paraphrased summaries of content, overviews highlighting main points. Passages can state information explicitly rather than requiring readers to infer it. See Pressley (1983) for other examples.

(b) Materials can be altered to encourage particular types of processing. One example is Scardamalia and Bereiter's (1986) "procedural facilitation," aimed at improving student composing. Cue cards are provided to prompt the steps comprising a complex writing strategy. Similarly, even if students have not mastered a story-grammar strategy for comprehending fiction (e.g., Short and Ryan, 1984), they can be provided in-the-text cues to notice setting, character, problem, action, and resolution information. For instance, texts developed by Anna Chamot (1987a,b) prompt use of story-grammar procedures and other strategies. For extensive discussion of how textbooks can be altered to provide the benefits produced by self-regulated strategy use, see Mayer (1987, Ch. 11) and Education Development Center, Inc. (1988); the latter is an especially impressive catalog of text-modification possibilities.

A very simple experimental design can be used to evaluate many material modifications intended either to substitute for strategic processing or to stimulate such processing. One group is given the modified materials; a second processes unmodified materials. Differences in performance between the two groups are assumed due to the material modifications. The track record for material modifications is good, with many modifications improving learning and other aspects of cognitive performance (e.g., Graesser and Black, 1985; Houghton and Willows, 1987; Pressley, 1983; Scardamalia and Bereiter, 1986; Willows and Houghton, 1987).

What are the long-term consequences of providing altered materials rather than strategy instruction? One view is that such aids are crutches potentially interfering with development of independent learning; an alternative perspective is that provision of optimal mediators might stimulate subsequent strategy acquisition and use. As learners realize better learning was due to mediators, they execute strategies to produce their own mediators (Pressley, 1983). No longitudinal data exist in support of either position, however. Given growing publisher interest in producing materials containing processing prompts, longitudinal study of modified material effects seems justified.

DISCUSSION

An Optimal Research Progression: The State of Research to Date

The issues reviewed here, the methods for addressing them, and some of the most important recommendations and comments made about each issue are summarized in Table III. We believe that early research about a particular strategy should focus on the nature of ongoing instruction and the prevalence of naturalistic strategy use. When strategies are not being taught or used, there may be a need for strategy instruction, especially if task analyses or analyses of expert performances suggest procedures students could employ profitably. Correlational data generated during studies of naturalistic teaching and strategy use can provide preliminary information about whether a strategy is a potent mediator of performance. If it is, those currently taught the method should perform better on tasks mediated by the strategy than those not taught the technique; those currently using it on their own should do better than those not using it. Once lack of strategy teaching and/or lack of strategy use is established and there is correlational support for the potency of the strategy, preliminary attempts at designing instruction should follow. These permit refinement of the intervention before more expensive evaluation occurs in the form of true experimental analyses. Even if a treatment produces striking effects in true experiments, however, there is no guarantee that the strategy instruction will be given to students, or stu-

Table III. Issues in Strategy Instruction Research, Methods for Addressing Those Issues, and Comments and Recommendations About Issues and Methods

Issue	Methods	Comments and recommendations
Determining the need for strategy instruction		
a. Are particular strategies taught already? And are there differences in students taught strategies and those who are not taught strategies?	Observation of a number of teachers for teaching of particular strategies; detailed analyses of expert teaching.	Some strategy instruction occurs already, although it is probably not extensive. Observational methods probably could be complemented by less costly approaches for data gathering (e.g., interviews). Work on teacher modifications of effective strategies is needed.
b. Is there effective teaching of strategies?	Observation of teaching for scaffolded instruction and direct explanation of strategies.	Assumption that naturalistically occurring scaffolding and direct explanation are effective, based largely on anecdotal evidence. The potency of naturalistic scaffolding and direct instruction should be assessed more formally. Whether strategy instruction, when it occurs at all, is usually scaffolded or directly explained is difficult to determine from the existing data base, although it seems unlikely that most naturalistic strategy instruction is as extensive as recommended by Soviet-inspired and direct-explanation models.
c. What strategies do students use already?	Observation of strategic behaviors, self-reports of specific strategy use including think-aloud data, objective indicators of strategy use such as latency data, and responses to questionnaires and standardized test items tapping general strategic tendencies.	Whatever process measures can be employed should be employed when attempting to argue naturalistic strategy use is occurring. When several measures of process converge to suggest a strategic process is mediating performance, there is especially strong evidence of strategy use. Standardized instruments tapping general strategic tendencies are being developed further and refined, although these instruments are already used extensively to identify students in need of strategy instruction.

Table III. Continued.

Issue	Methods	Comments and recommendations
Development of an intervention	Uncontrolled pretest-to-posttest assessments; ethnographic techniques to analyze and document strengths and weaknesses of treatments under development.	More and more extensive development work is needed with educator-researcher collaboration throughout the development process. Such work can be used to revise the intervention and advise subsequent users of the treatment about potential problems. Pilot testing can be useful for evaluating cost-effectiveness of intervention even before a true experiment occurs. More reporting of insights gained about interventions during development should be provided to practitioners.
Evaluating a strategic intervention in true experiments		
a. Does teaching a strategy produce the intended effects?	True experiments in the lab and classroom. Studies may include multiple controls to rule out alternative explanations of treatment effects such as motivation. A more economical alternative is to collect both dependent variables that should be affected by the treatment and those that should not, analyzing for condition by task interactions. Within-condition correlational analyses can bolster conclusions that follow from experimental manipulation: If subjects within a condition who use a strategy more extensively outperform subjects who use the strategy less, there is correlational support for the conclusion that strategy use positively affects performance.	Lab studies are usually practical only for simple strategies. Classroom experiments are often expensive to conduct with a number of real-world obstacles potentially interfering with true experimentation. The interpretive advantages of the true experiment are great enough to justify its expense in many cases. Experiments with two or only a few conditions can be very analytical if diverse dependent variables are collected permitting prediction of condition by task (dependent variable) interactions with this approach strongly recommended.

Table III. Continued.

Issue	Methods	Comments and recommendations
b. What are effective methods for teaching strategies?	True experiment with one condition in which strategy A is taught using methods presumed to be effective. In the other condition, strategy A is taught using alternative methods.	Very few true experiments on this problem. Data have been generated to support direct explanation of strategies as an effective method of strategy instruction, however. A real need exists for more experiments in which strategies are held constant and method of instruction is varied. Experiments in which scaffolded teaching or direct explanation of strategies are employed in conjunction with published strategy-instructional materials would be particularly timely. Work on effective methods of teaching is closely related to research on transfer and maintenance since important goals of effective teaching are maintenance and transfer
c. How can durable strategy instruction effects be produced?	True experiments on maintenance involve assessment on trained tasks sometime after instruction is completed. True experiments on transfer involve assessment on tasks other than trained tasks.	More research is needed on why maintenance and transfer failures occur, exploring whether students recognize when strategies can be used, transform effective strategies into ineffective procedures, or are not motivated to use strategies. The many recommendations for increasing generalization of strategies need to be studied as well. Designs that assess whether maintenance and transfer are maximized are recommended over more typical designs that reveal only whether maintenance or transfer occurred at all.
d. Is this strategy instruction better than alternative instruction aimed at the same goal?	True experiment with one group receiving strategy A and the other strategy B. The study is a "horse race" in the sense that it is designed to determine only which method is better. Study is considerably more analytical if at least one more condition is added, a no-strategy control condition.	Sometimes a horse race must substitute for an experiment with a no-strategy control condition (i.e., when no-strategy control would be unethical). This design is especially useful in evaluating a new intervention relative to treatments that are already available, thus, permitting assessment of whether the costs of deploying a new intervention would be justified.

Table III. Continued.

Issue	Methods	Comments and recommendations
e. Do special populations benefit from strategy instruction?	True experiments with special populations as subjects. Sometimes restricted availability of special subjects mandates $n = 1$ designs with a few replications.	When there is a different pattern of outcomes in normal and special populations, there is sometimes motivation to examine the benefits of the strategy instruction in a fine-grained fashion relative to a variable differentiating the normal and special populations. That is, outcomes produced in experiments with special populations can motivate aptitude by treatment interaction studies.
f. Are there aptitude by treatment interactions in gains following strategy instruction?	True experiment with measurement of at least one individual difference variable. An important question is whether treatment effect is similar or different in size at different values of the individual differences variable. Initial assessment is whether the relationship between the individual differences variable and performance differs in different conditions of the experiment.	Expensive in that more subjects per condition are required in order to make powerful tests of between-condition differences in size of aptitude with performance correlations than are required to compare mean differences between conditions. Given many hypothesized interactions between individual differences in processing characteristics and treatments, this design could be used extensively.
Evaluating acceptance of strategy instruction by students and teachers	Acceptability questionnaires. Interviews tapping student and teacher perceptions as they try a strategy intervention.	Little is reported about teachers' and students' perceptions of strategy instruction, although research is in progress, more with teachers than students at this point. Development and use of informal interview methods is critical for informative acceptability research. There are a variety of hypotheses about how to make strategy instruction more attractive than could be evaluated in true experiments with teacher and student perception data as dependent variables.
Evaluating materials designed to provide some of the benefits of strategy use	True experiments in which one group is given modified materials and the control group unmodified materials.	Although there is much documentation of short-term benefits provided by material modification, long-term effects not addressed to date. Clear need for it given predictions of both pejorative and facilitative long-term effects.

dents will use strategies they are taught. Thus, researchers now are considering how to maximize the attractiveness of effective strategies to educators and students. Research also is ongoing on design of curriculum materials to encourage strategic processing in the absence of instruction.

Strategy instructional research is concerned both with the evaluation of particular strategies and with how to teach strategies in general. That is, there is research on identification of potentially potent strategic procedures (e.g., specific reading strategies) and research on potentially potent methods of teaching those procedures (e.g., scaffolded teaching). The optimal temporal progression from observational studies to intervention development to experimental evaluation to studies of implementation holds in both cases, although much more research has been conducted on the identification of strategies than on the identification of effective techniques for teaching strategies. Recent theoretical analyses of alternative teaching methods should encourage additional research (e.g., Herrmann, 1988; Pressley *et al.*, 1987e).

Few strategies have been evaluated in the optimal fashion outlined in this article. In fact, we can identify no procedures researched with respect to all of the issues covered here, and it is certain no single strategy or strategy package has been evaluated extensively with respect to each of the issues summarized in Fig. 1 and Table III. For instance, strategies often are subjected to rigorous experimental tests before there is much correlational-observational data about naturalistic use and teaching of the procedures (see Underwood, 1975, for enlightening commentary). Many strategic interventions are developed without field testing and attempts to improve the intervention before submitting it to experimental evaluation. Perhaps most regrettably, many experimental psychologists and researchers testing the potency of strategies in laboratory settings have little interest in educational implementation of strategy instruction.

Some strategy interventions (e.g., multiple-year programs involving a number of components and strategies) could never be studied as depicted in Fig. 1. Informative research can be generated within these large-scale interventions, however. Correlations between student use of instructed strategies and performances can be collected; so can correlations between the amount of strategy teaching and student performance; pretest-posttest investigations are doable. Eventual dissemination of a large-scale treatment might be easier to track than dissemination of small-scale instructional modifications (i.e., it is easier to spot an elephant than a mouse).

When it can be generated, the price of comprehensive information about strategies is high. What is required are programmatic efforts taking years to complete. More positively, there are relatively few important educational strategies (Pressley *et al.*, 1990a), so it might be possible to study a large proportion of them extensively and intensively. For instance, there are fewer than a dozen reading-comprehension procedures known to facilitate children's comprehension and memory of text at all (Pearson and Dole, 1987; Pressley *et al.,* 1989b).

The motivation to do strategy-instruction research is greater now than ever before, with a real need for research directed at strategic interventions captivating to the educator community. Scientists with diverse methodological preferences (i.e., psychometric, anthropological, experimental) and conceptual interests (e.g., children's learning, adult cognition, teacher education) can play a part. There are roles for scientists whose talents are principally in the laboratory and for those who are savvy at conducting large-scale program evaluation. Teacher and scientist collaboration could produce research that is scientifically sound and educationally relevant—covering all of the issues summarized in Fig. 1.

Three Wishes and a Final Comment About Future Research

The issues and questions summarized in Fig. 1 are ones currently being investigated. Many more issues could and should be addressed. We close with commentary about three research problems that should be pursued, three wishes the researcher community could grant. If it did, work on strategy instruction would be more than sound and relevant. It would be scientifically and educationally important.

Qualitative Descriptions of Strategy Instructional Effects

There is a need for richer descriptions of strategy instructional effects than occurred in most previous studies. Both ethnographic research of students' learning and of teachers' teaching of strategies in actual classrooms is needed. A lot remains to be discovered about the hassles and joys of teaching and learning strategies and about how strategy use affects life. For instance, do students' self-esteem levels increase when they acquire strategies permitting them to negotiate tasks difficult to perform before the strategies were acquired? Is teaching more rewarding for teachers providing powerful academic tools to their students? Addressing these questions requires researchers to immerse themselves in strategy-instructional environments. The time is right for such immersion (Lincoln and Huba, 1985). Important insights might follow from it, potentially ones providing guidance for both research and practice for years to come.

Development and Evaluation of Theoretically Motivated and Empirically Informed Strategy Instruction Programs

Many strategy-instruction programs are being provided to educators. Most involve dumping a bunch of strategies into teachers' laps with little guidance as to why or how these strategies might work. Often, no such rationale is possible. Many times individual strategies in these packages have never been validated.

More optimistically, enough is now known about strategies so it is possible to develop strategy instruction packages reflecting theory refined in light of extensive research. One excellent example is reading comprehension. Much is known about specific strategies to promote comprehension (Pressley *et al.,* 1989); knowledge of when and where to apply these strategies is increasing (Pressley, Johnson, Symons, McGoldrick, & Kurita, 1989b); and there are some certainties about how to teach students to use these strategies (Pressley *et al.,* 1987e). Many multicomponent strategy instruction packages could be designed based on previous research efforts. It is a good bet such packages would be more potent than many of the intuitionally inspired comprehension instructional packages currently on the market.

Investigation of How to Teach Teachers About Strategies So They Can Teach Their Students

No systematic research has been produced on how to prepare teachers to teach strategies. More positively, groups like Deshler and his associates at Kansas, and Duffy and Roehler and their colleagues at Michigan State, have extensive experience teaching teachers about strategy instruction. These groups have built up sophisticated and well-informed informal knowledge bases about how to develop strategy-teaching skills. Would-be teachers need to learn at least some information-processing theory and need formal instruction about what strategies to teach and how to teach them (e.g., scaffolding). More importantly, however, future teachers must be provided opportunities to practice teaching strategies with feedback provided by more expert teachers. Becoming a good strategy instructor is not easy. (See Pressley *et al.,* 1989a, for detailed commentary.)

Given the demands already made on the teacher-preparation curriculum, is it realistic to believe student teachers can also be taught to teach strategies? That they can be given several years of practice doing so? Our belief is that many important educational skills can be taught as strategies (e.g., arithmetic computation, mathematical estimation, reading comprehension, composition), and thus, there is strong motivation to make room for strategy instruction in teacher preparation. Of course, another problem follows, preparing teacher educators so they can teach future teachers. Once again, the Kansas and Michigan State groups are assuming leadership roles here. For instance, the Kansas Learning Disabilities Institute offers summer institute-type training for teacher educators. The effects of such institutes deserve serious study.

Final Comment

Strategy-instruction research is off to a good start. Nonetheless, that there is little qualitative research, few ambitious strategy-instruction curricula motivated by theory and previous research, and no formal study of how to teach teachers about strategy instruction makes obvious the need for a great deal more work. The many studies of basic learning tasks alluded to here permitted important insights about how to do strategy research and what might be expected from such studies. Nonetheless, those studies also make obvious the very great reliance of strategy researchers on basic memory tasks, reading tasks not much like ones students encounter in school, and problem-solving procedures only remotely resembling ones demanded by the real-school work day. With every passing year, however, better and more ecologically valid research is conducted. Perhaps this primer of the methods now in use will stimulate more such research, and will make more obvious the important directions not now being pursued.

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