

# Improving Computer Skill Training: Behavior Modeling, Symbolic Mental Rehearsal, and the Role of Knowledge Structures

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Effective computer skill training is vital to organizational productivity. Two experiments ( $N = 288$ ) demonstrated that the behavior modeling approach to computer skill training could be substantially improved by incorporating symbolic mental rehearsal (SMR). SMR is a specific form of mental rehearsal that establishes a cognitive link between visual images and symbolic memory codes. As theorized, the significant effects of SMR on declarative knowledge and task performance were shown to be fully mediated by changes in trainees' knowledge structures. The mediational role of knowledge structures is expected to generalize to other training interventions and cognitive skill domains. Our findings have the immediate implications that practitioners should use SMR for improving the effectiveness of computer skill training.

Effective skill training is vital for enhancing workplace performance. Motorola, for example, estimates that every dollar spent on training produces \$30 in productivity gains within 3 years (D. Kirkpatrick, 1993). A recent meta-analysis (Arthur, Bennett, Edens, & Bell, 2003) indicates that the effect of organizational training interventions on productivity is stronger than effects previously observed for other managerially controllable interventions such as performance appraisal, feedback, management by objectives, and various psychologically based interventions. Employers and human resource practitioners increasingly seek cutting-edge training techniques that are based on solid scientific foundations as a source of competitive advantage. Training has long been a central concern of applied psychology (e.g., Latham, 1988; Tannenbaum & Yukl, 1992), and substantive progress is underway toward strengthened theoretical underpinnings (Salas & Cannon-Bowers, 2001).

Computer skill training in particular has been ranked as the most important issue in human resource development by a national survey (Bassi, Cheney, & Buren, 1997). Of the more than \$55 billion spent annually on formal training activities by U.S. organizations, computer skill training is the most frequent type of training provided—more frequent than supervisory training, communication training, and sales training (Galvin, 2001). Investing in more and better computer skill training has been named one of the most profitable avenues toward achieving the productivity and quality gains sought by organizations investing in information technology (Adler, 1991). Computer skills are “essential for a wide range of occupational pursuits” (Bandura, 1997, p. 434). Prolifer-

ation of information technology places an increasing premium on the effectiveness with which organizations prepare their employees to use computer software applications in conducting useful business functions (Landauer, 1995; Sichel, 1997).

Various training methods are currently used to teach computer skills (Galvin, 2001; Gattiker, 1992), but their strengths and weaknesses, and reasons underlying their relative effectiveness, remain insufficiently understood. One consistent research finding is that the behavior modeling approach to computer skill training, in which trainees watch a model demonstrate computer skills and then the trainees reenact the modeled behavior, is more effective than alternative methods such as computer-aided instruction (Gist, Rosen, & Schwoerer, 1988; Gist, Schwoerer, & Rosen, 1989), lecture-based instruction (Compeau & Higgins, 1995; Simon & Werner, 1996), and self-study (Simon & Werner, 1996). Given the strong track record to date of modeling-based computer skill training, there is much promise in further developing this class of techniques. The present research is concerned with developing theory-based computer training interventions that are cost effective to implement in practice.

The behavior modeling technique is based on social cognitive theory (Bandura, 1986, 1997), which argues that observational learning is “largely an information processing activity in which information about the structure of behavior and about environmental events is transformed into symbolic representations that serve as guides for action” (Bandura, 1986, p. 51). Social cognitive theory posits that observational learning results in “knowledge structures representing the rules and strategies of effective action” that “serve as cognitive guides for the construction of complex modes of behavior” (Bandura, 1997, p. 34). Social cognitive theory has been successfully applied to a broad spectrum of skill domains, including educational, clinical, athletic, health, decision and policy making, supervisory behavior, and adoption of technological innovations such as applications of computer technology in the workplace (Bandura, 1997). However, the purported key role of knowledge structures as a mediating mechanism between ob-

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servational learning and training outcomes has received minimal empirical attention to date.

The importance of knowledge structures is reinforced by recent literature on expertise, learning, and skill acquisition (e.g., Anderson, 1994; Eichenbaum, 1997; Glaser, 1990; May & Kahnweiler, 2000). This literature embraces a developmental view in which knowledge proceeds from an explicit declarative form resulting from being told or shown how to use a skill (the verbal phase), through knowledge compilation and chunking resulting from mental or physical practice (the associative phase), to a final compiled or proceduralized form characteristic of expert performance (the autonomous phase). Early stages of knowledge acquisition are characterized by slow and effortful information processing, whereas later stages are characterized by relatively smooth, automatic, and effortless performance. Throughout this process, individual chunks of information are theorized to become increasingly interconnected and organized into knowledge structures, sometimes called mental models, cognitive maps, or schemata (Aarts & Dijksterhuis, 2001; Glaser, 1990; Johnson-Laird, 1983; Kozlowski et al., 2001; Kraiger, Ford, & Salas, 1993; Rouse & Morris, 1986; Rowe & Cooke, 1995). Social cognitive theory is highly consistent with this developmental account of skill acquisition, specifically identifying the establishment of knowledge structures as a key mechanism governing observational learning (Bandura, 1986, 1997).

In the present research we investigate whether modeling-based computer skill training can be improved further by incorporating either of two techniques previously shown to be effective in supervisory skill training: symbolic mental rehearsal (SMR) and reciprocal peer training (RPT). Why should SMR and RPT generalize from supervisory skill training to computer skill training? The nature of the skill set used by individuals using computer application software such as word processing, databases, spreadsheets, and electronic mail to perform workplace tasks is thought to be a blend of cognitive and perceptual-motor competencies (Buffardi, Fleishman, Morath, & McCarthy, 2000; Card, Moran, & Newell, 1983; Dix, Finlay, Abowd, & Beale, 1993; Willingham, 1998). Supervisory skills also have a substantial cognitive component but have greater social and interpersonal components and less of a perceptual-motor component than computer skills (Snyder & Stukas, 1999; Sternberg & Kaufman, 1998). Despite these differences, in the present research we seek to exploit an apparent commonality between the supervisory and computer skill domains: Both skill sets have a cognitive component, which is likely to be susceptible to influence by training interventions that alter relevant knowledge structures. Below, we introduce SMR and RPT and develop the theoretical rationale for why these training interventions are expected to influence trainees' knowledge structures.

### Symbolic Mental Rehearsal (SMR)

SMR refers to a class of training interventions in which, after observing a model performing a target behavior, trainees are instructed to engage in two information-processing activities: (a) *symbolic coding*, the process by which trainees "organize and reduce the diverse elements of a modeled performance into a pattern of verbal symbols that can be easily stored, retained intact over time, quickly retrieved, and used to guide performance" (Decker, 1980, p. 628), and (b) *cognitive rehearsal*, "the process in

which individuals visualize or imagine themselves performing behaviors that previously were seen performed by another individual" (Decker, 1980, p. 628). According to Bandura (1986), SMR works by inducing trainees to "transform what they observe into succinct symbols to capture the essential features and structures of the modeled activities" (p. 56). Such symbols serve as guides for action, and "play an especially influential role in the early phases of response acquisition" (Bandura, 1986, p. 56).

In a typical application of SMR to supervisory training, trainees first view a model's performance of the desired behavior, are next instructed to mentally associate each of the main components of the behavior with summary verbal descriptions called *codes* (symbolic coding) by writing key words onto coding sheets provided, and are then asked to visualize themselves reenacting the observed behavior (cognitive rehearsal) using the verbal codes as guides (e.g., Decker, 1980). As an example, in Decker's study, the demonstrated behavior was the use of assertiveness skills in refusing unreasonable requests, and examples of the rules extracted by trainees from the videotaped model were "understand your need," "do not want to lend," and "calmly repeat a negative reply without justifying it" (p. 629). Such modeling-based interventions have been termed retention processes (Decker, 1982; Decker & Nathan, 1985), coding and symbolic rehearsal (Bandura & Jeffery, 1973; Decker, 1980, 1982), and symbolic coding and cognitive rehearsal (Bandura, 1986). The present research uses the specific term *symbolic mental rehearsal* to differentiate such interventions from other forms of mental rehearsal that do not explicitly involve either behavior modeling or symbolic coding (e.g., Driskell, Copper, & Moran, 1994; Feltz & Landers, 1983). Adding SMR to modeling-based training has been shown to improve the effectiveness of supervisory training (Decker & Nathan, 1985). In computer skill training studies, behavior modeling workshops typically present summaries of key learning points (e.g., Gist et al., 1988, 1989) but do not explicitly encourage, instruct, or allow extra time for trainees to actively encode or mentally rehearse the information.

A key determinant of both the accessibility of declarative knowledge and the execution of procedural knowledge is the strength of knowledge encoding in memory (Anderson, 1994). The process within SMR of creating an association between verbal summary codes and mental images of action elements is a form of mnemonic encoding, which Hasher and Zacks (1979) would consider an effortful (as opposed to automatic) encoding operation. Therefore, it is unlikely to be done spontaneously by trainees unless they are explicitly instructed to do so. Such effortful modes of learning have been shown to result in knowledge structures that are more highly organized and more accessible to intentional cognitive strategies compared with those acquired through effortless (automatic or implicit) learning (Roberts & MacLeod, 1999).

In SMR, behavioral observation and symbolic coding are followed by mental rehearsal that explicitly relies on these summary verbal codes of the target behavior as a cognitive guide. Two meta-analyses of research outside the context of behavior modeling concluded that mental practice in general has a significant effect on performance, and the effect tends to be stronger for tasks that have a greater cognitive component (Driskell et al., 1994; Feltz & Landers, 1983). Both meta-analyses favored a cognitive symbolic account, in which mental practice assists in the establishment of a schematic knowledge structure useful for regulating behavior, over the alternative attentional and motivational ac-

counts. Providing further support for the cognitive symbolic account, [Vogt \(1995\)](#) reported three experiments that demonstrated that observation, imagery, and performance share a common “event-generation” process in which mental practice serves as a bridge between perceptual inputs and motor responses. Because coding and rehearsal processes may enable trainees to more readily develop knowledge structures needed to perform the cognitive perceptual–motor tasks involved with computer use, we developed the following hypothesis:

*Hypothesis 1:* Symbolic mental rehearsal will increase declarative knowledge and task performance when added to modeling-based computer skill training.

### Reciprocal Peer Training (RPT)

Behavior modeling workshops for supervisory training often include RPT in which each trainee is instructed to take turns (a) assuming the role depicted by the model by performing the demonstrated behaviors for peers to observe and (b) providing social reinforcement (advice, feedback, or encouragement) to peers while they perform target behaviors ([Decker & Nathan, 1985](#)). Social cognitive theory ([Bandura, 1986](#)) regards such reciprocal peer training as an opportunity to improve the quality of skill reproduction by enabling peers to reduce discrepancies between modeled actions and their own actions on the basis of peer observation and feedback. Behavior modeling as implemented in computer skill training has not included RPT ([Compeau & Higgins, 1995](#); [Gist et al., 1988, 1989](#); [Simon & Werner, 1996](#)); the nearest thing is computer-provided feedback on whether the executed tasks were performed correctly (e.g., [Gist et al., 1989](#)). This is very different from RPT because it does not afford trainees the cognitive elaboration benefits of explanation, nor does it allow trainees to engage in vicarious learning by observing someone else perform the target behavior.

Because RPT involves social interaction, it may seem intuitive that it would benefit training for supervisory behaviors because of their interpersonal nature. However, why might RPT prove beneficial in augmenting modeling-based training for noninterpersonal computer skills? Outside the behavior modeling context, learning benefits of RPT have been found in noninterpersonal domains such as mathematics ([Webb, 1982](#)), science ([Okada & Simon, 1997](#)), problem solving ([Chi, De Leeuw, Chiu, & Lavancher, 1994](#)), and engineering ([Dossett & Hulvershorn, 1983](#)). A cognitive perspective on cooperative learning suggests that the learning advantages of RPT stem from cognitive restructuring of the information ([Shuell, 1988](#)). Several studies indicate that the major benefits of cooperative learning derive from giving and receiving explanations (e.g., [Fantuzzo, Riggio, Connelly, & Dimeff, 1989](#); [Slavin, 1983](#); [Webb, 1982](#)), and students who gained most were those who provided explanations to others ([Chi et al., 1994](#); [Webb, 1989](#)). [Chi et al.](#) provided evidence that self-explanation led to the acquisition of more correct mental models. Because RPT may be linked to the establishment of effective knowledge structures needed to perform computer usage behaviors, we hypothesize the following:

*Hypothesis 2:* Reciprocal peer training will increase declarative knowledge and task performance when added to modeling-based computer skill training.

## Experiment 1

### Method

#### Participants

A total of 193 student volunteers (42% women and 58% men) completed the experimental procedure. Participants were recruited on a voluntary basis from an introductory computer course at a large state university in the eastern United States to participate in a training program regarding Microsoft Excel. As motivational incentives, students received extra credit points by participating in the experiment and were promised and later given confidential feedback on their performance compared with their peers. In addition, the skills provided by the training were useful for completing a term project. Participants' ages ranged from 18 to 44 years. Most (86.5%) of the 193 participants reported that they had never used the software program or had used it less than 1 hr/week.

#### Training Procedure

Participants were randomly assigned to 1 of 20 training workshops offered on three consecutive Saturdays and were informed that the goal of the experiment was to understand how people acquire computer software skills. Participants were not told that different training conditions were being tested. Participant characteristics were not significantly different across training conditions in pretest questionnaire measures of age, gender, computer experience, or spreadsheet experience. The average number of trainees in a workshop was 10.

Two professional trainers (nonauthors) were hired to deliver the various forms of training using scripts developed by the authors and pretested in a pilot study. Before the experiment, the trainers visited the training site several times to become familiar with the facilities and materials and to practice the script for each training condition. The trainers were blind to the hypotheses of the experiment. Except for a one-paragraph introduction of the software interface included in the script and presented by trainers, conceptual explanations and behavior modeling demonstrations were delivered entirely by videotapes, which were held constant across all training conditions. The trainers provided limited assistance when trainees requested it, which was restricted to guiding trainees through the steps of the training script without providing direct conceptual or procedural instruction. In each computer lab, a trainer welcomed participants and directed them to an available computer. After all participants were seated, the trainer closed the door and started the workshop. Following the prepared scripts, trainers first introduced themselves, distributed and collected pretest questionnaires, and then implemented the assigned training condition. Trainers used stopwatches to control the time for each step in the training procedures using timing guidelines specified in the training scripts. After training procedures were completed, each trainee filled out a posttraining questionnaire, took a declarative knowledge test (5 min) and a hands-on task performance test (25 min), and was thanked and dismissed.

The commercial videotape used in all training conditions consisted of five segments: basic formatting (8 min), formulas (8 min), functions (12 min), advanced formatting (15 min), and advanced formulas (15 min). In each segment, the same male model demonstrated the specific steps needed to carry out target behaviors. At the end of each segment, the model summarized key learning points of the segment. Each trainee had access to a computer during the practice and task-performance testing periods of the workshop. An exercise file containing the same rows and columns of initial numbers as presented in the video, which had been preinstalled on each computer, was used by trainees for hands-on practice.

#### Experimental Conditions

A 2 × 2 factorial between-subjects design was used to manipulate the SMR and RPT treatments, yielding the following experimental conditions:

control (no SMR, no RPT), SMR only, RPT only, and SMR plus RPT. These four treatment conditions were identical with the exception of varying the SMR or RPT interventions. To rule out the rival hypothesis that any treatment effects were merely due to the additional time required for the SMR or RPT interventions, a second control group was created by adding hands-on practice time such that overall training time was equal to the longest treatment group (SMR plus RPT). The experiment was designed to systematically counterbalance the effects of possible differences in trainers, computer labs, days, and times of day. There were no significant effects of trainers, computer labs, days, or times of days on any of the study variables.

*Control groups (no SMR, no RPT).* This condition consisted of having trainees observe the training video and then engage in hands-on practice. At the beginning, trainees in this condition were instructed to work individually and direct questions only to the trainer. Trainees were then informed that there would be two sessions before a test, each session consisting of watching a 30-min video lesson and practicing the demonstrated skills on the computer. The duration of 30 min of observation for one lesson is consistent with that of previous studies (Compeau & Higgins, 1995; Gist et al., 1989). The first 30-min lesson contained the first three video segments (basic formatting, formulas, and functions) and the second 30-min lesson contained the last two video segments (advanced formatting and advanced formulas). Trainees watched the first 30-min video lesson, practiced for 15 min, watched the second 30-min video, and practiced for another 15 min. It was decided from the responses of a pilot study that 15 min was adequate for nearly all participants to reenact the behaviors presented. This condition did not include any SMR or RPT processes. Total time for this condition was 150 min, including 30 min of hands-on practice. As for all treatment conditions, the remaining 120 min were used for introduction, pretest and posttest questionnaire administration, video observation, declarative knowledge test, and task performance test.

Including SMR and RPT interventions added 45 min to the total training time for the SMR plus RPT group compared with the control group. To examine the rival explanation that any treatment effects were merely due to extended training time per se, and not because the time was used for SMR or RPT, we created a second control group that was equivalent in total training time to the longest treatment group (SMR plus RPT) by allocating the additional 45 min to hands-on practice. Specifically, trainees in this version of the control group had an additional 23 min of hands-on practice after the first session and an additional 22 min after the second session. Thus, this "longer practice time" control group was exactly the same as the "regular practice time" control group except for the additional practice time. Total time for the longer practice time group was 195 min, of which 75 min were for hands-on practice.

*SMR group.* In addition to observation and hands-on practice, this condition included the SMR processes of symbolic coding and cognitive rehearsal. At the beginning, trainees in this condition received blank papers labeled with section headings for summary activities. Next, for the first 30-min video lesson, the tape was played and paused at the end of each of the three segments. As instructed, during each pause, trainees summarized the computer operations that had been presented by writing down key points of the demonstration under the appropriate section heading. Two min were given for this symbolic coding process after each segment. After the summary of the third segment, trainees practiced the demonstrated computer skills on the computer for 15 min. After the hands-on practice, trainees watched the second 30-min video lesson. As before, the tape was paused at the end of each of the two segments, and trainees performed symbolic coding for 2 min during each pause. After the final symbolic coding activity, trainees again had 15 min of hands-on practice. Examples of symbolic codes written by trainees included "all formulas begin with the equal sign," "use cell references in a formula," and "to copy the formula to next cells, drag from the lower right corner of the cell." On the completion of the hands-on practice, trainees cognitively rehearsed their own summary before taking the test. Consistent with how cognitive rehearsal has been

practiced (Decker, 1980, 1982), trainees were requested to relax and mentally picture themselves performing the computer operations step by step while reviewing the summary. Five min were allowed for cognitive rehearsal, and then the test was administered. Total time for this condition was 165 min, which included 30 min of hands-on practice and 15 min of SMR activity.

*RPT group.* In addition to observation and hands-on practice, this condition included the RPT components of demonstrating the target behavior to a peer and providing feedback and reinforcement to a peer while he or she performed the behavior. After the pretest questionnaire, trainees were paired to form teams of two trainees randomly grouped by the trainer. Trainees next watched the first 30-min video lesson and practiced the demonstrated actions on the computer by participating in a role play. Before the role play started, each team decided who would initially assume the role of hands-on demonstrator and who would assume the role of observer. Using the computer, the demonstrator reenacted the behaviors presented on the video while explaining the procedural steps. The observer provided social reinforcement to help the demonstrator finish the demonstration correctly. After 15 min, team members reversed roles and repeated the RPT processes. Thus, each trainee performed the role of demonstrator for 15 min and the role of observer for 15 min.

Trainees next watched the second 30-min video and again practiced the observed skills via RPT. This time, trainees reversed the order of who first played a role of demonstrator or observer. That is, whoever initially assumed the role of demonstrator first in the first session began by assuming the role of observer in the second session. As in the first session, trainees reversed roles after 15 min. Thus, the role-play activity of reenacting the modeled behaviors gave each trainee 30 min of hands-on practice in total. It should be noted that trainees in the peer-training conditions received social reinforcement in the form of feedback, guidance, and praise during their 30 min of hands-on practice, which was not the case of those in non-peer-training conditions, who practiced individually (for 30 min except for those in the longer practice time version of the control group). From an experimental design standpoint, we felt that by holding constant the total practice time to 30 min, this approach more closely equalized the treatments than if trainees in the RPT condition were given another 30 min for individual practice. Total time for this treatment was 180 min, of which each trainee received 30 min of hands-on practice and served as peer trainer for 30 min.

*SMR and RPT group.* In addition to observation and hands-on practice, this condition included all SMR and RPT activities. Trainees in this condition performed the symbolic coding process at the end of each segment of the video in the same way as trainees in the SMR condition did. After the first and second 30-min video lessons, pairs of trainees took turns training one another as was done in the RPT condition. As in the SMR condition, trainees conducted cognitive rehearsal of the summary before the test. Total time for this condition was 195 min, which included 15 min of SMR activities, 30 min of hands-on practice, and 30 min serving as peer trainer.

## Measures

Training outcomes were assessed using measures of declarative knowledge and task performance, the two most commonly examined outcomes in training research according to Colquitt, LePine, and Noe's (2000) meta-analysis of 106 training studies spanning 20 years. In addition, trainees' affective reaction to the training was measured, as recommended by D. L. Kirkpatrick (1987) and Kraiger et al. (1993), to assess whether effects of training on declarative knowledge or task performance may be attributable to unequal training quality across training conditions rather than the type of training intervention per se. Age, gender, computer experience, and spreadsheet experience were measured to control for pretraining individual differences. Manipulation checks were measured for the SMR and RPT interventions.

*Declarative knowledge and task performance.* The declarative knowledge measure consisted of 10 multiple-choice test questions regarding the concepts and features needed to use the software appropriately. The items were developed from the video and accompanying material. The items included questions about copying a cell, using a formula, performing a calculation, and adjusting the size of a column or row. The score was the total number of correct answers out of 10. The hands-on task performance measure consisted of 12 computer tasks that each required several steps. Examples include entering a formula in multiple cells, using functions to calculate total and average amounts, copying the format of a cell, changing the formats of numbers, and centering the alignment of the title. Each trainee saved the test result in a designated directory on completion of the test. Each task was scored with 1 point for totally correct answers, .5 for partially correct answers, and 0 for incorrect or missing answers. Thus, possible scores ranged from 0 to 12. The grading of the answers was handled by the spreadsheet program module developed through several stages of programming and accuracy verification. Pilot testing of this grading program showed over 98% consistency with the average scores of two human graders.

Internal consistency measures such as Cronbach's alpha are not meaningful for the measures of declarative knowledge or task performance because each of their items taps a different facet of the cognitive skill domain (e.g., Pennington, Nicolich, & Rahm, 1995), and the items are regarded as composite (also called aggregate or causal, in contrast to reflective) indicators of the construct. High item intercorrelations are not necessarily expected among composite indicators, and low internal consistency does not threaten construct validity or attenuate estimated relationships as with reflective measures (Bollen & Lennox, 1991; Law, Wong, & Mobley, 1998).

*Affective reaction.* Trainees' affective reactions were measured using four items with an 11-point Likert-type scale (0 = *completely disagree*, 5 = *neither agree nor disagree*, 10 = *completely agree*): "I am satisfied with the training program," "I enjoyed this training program," "I would recommend this training program to others," and "Overall, I am satisfied with the quality of the training program that I have just received." The reaction measure showed an internal consistency reliability (Cronbach's  $\alpha = .93$ ).

*Computer and spreadsheet experience.* General computer experience ("How long have you used computers?") was measured on a 1–5 scale: (1 = *less than a month*, 2 = *1–6 months*, 3 = *7–12 months*, 4 = *1–3 years*, 5 = *more than 3 years*). Spreadsheet experience was measured on a 0–5 scale: "Have you ever used any spreadsheet program such as Excel, Lotus 1-2-3, or Quattro Pro?" (0 = *no*). "If you have used a spreadsheet program, how many hours do you work with it in a typical week?" (1 = *less than 1 hr*, 2 = *1–3 hr*, 3 = *4–10 hr*, 4 = *11–20 hr*, 5 = *more than 20 hr*).

*Manipulation check for SMR.* The manipulation of SMR was checked by comparing the number of trainees who actually performed any kind of summary activities during their training workshops. More specifically, all the papers either distributed by the trainers or self-supplied by trainees were collected and examined to see how many trainees actually created some sort of summary in different training conditions. The checks made for SMR showed that, although there was a varying degree of completeness, all the trainees ( $n = 80$ ) in the training conditions that included the SMR component performed symbolic coding, whereas only 6 of 113 trainees (5%) in the other training conditions without the SMR component created a summary using their own papers,  $\chi^2(1) = 170.00, p < .001$ .

*Manipulation check for RPT.* The degree to which trainees engaged in RPT was assessed with an eight-item measure included in the posttraining questionnaire. An 11-point Likert-type scale anchored by 0 = *completely disagree* and 10 = *completely agree* was used. All trainees were asked to indicate if they had interaction with other trainees by responding to questions such as, "I explained how to use Excel to another trainee during this training session," "I encouraged another trainee as he or she learned how to use Excel," "Another trainee explained how to use Excel to me during this training session," and "I got encouragement from another trainee as I learned how to use Excel." The peer-interaction measure showed an internal consistency reliability (Cronbach's  $\alpha = .98$ ). Trainees in the RPT conditions reported very high levels of interaction with other trainees ( $M = 8.48, SD = 1.53$ ), whereas non-RPT trainees reported very low levels of interaction with other trainees ( $M = 1.67, SD = 2.73$ ). This difference was significant,  $t(191) = 19.82, p < .001$ .

## Results and Discussion

Table 1 presents intercorrelations of Experiment 1 variables and Table 2 reports mean scores for declarative knowledge, task performance, and affective reaction for each treatment group. A comparison of the short and long versions of the control group showed no significant differences in either declarative knowledge,  $t(75) = 1.09, ns$ , or task performance,  $t(75) = -1.44, ns$ . Because this result indicates that any significant treatment effects are unlikely to have resulted from differences in total training time per se, the two control groups were pooled for subsequent analyses. Analysis of covariance (ANCOVA) was conducted in which the covariates of age, gender, general computer experience, and spreadsheet experience were entered first to control for pretraining individual differences before testing the significance of treatment effects. This ANCOVA (Table 3) produced mixed support for

Table 1  
Intercorrelations of Experiment 1 Variables

Variable	<i>M</i>	<i>SD</i>	1	2	3	4	5	6	7
Training outcome									
1. Declarative knowledge	6.99	1.78	—						
2. Task performance	7.73	2.53	.38*	—					
3. Affective reaction	8.17	1.82	.04	.04	—				
Trainee characteristic									
4. Age	21.70	3.49	.03	-.03	.19*	—			
5. Gender	0.58	0.50	.07	-.03	-.08	-.09	—		
6. Computer experience	4.13	1.03	.16*	.30*	-.08	-.03	.01	—	
7. Spreadsheet experience	0.78	0.86	.02	.25	.06	.00	-.01	.26*	—

*Note.* Declarative knowledge and task performance scores are the total number of correct answers out of 10 and 12, respectively. Affective reaction scores are on a scale of 0 (*negative*) to 10 (*positive*). Gender was coded 0 (*female*) and 1 (*male*).

\*  $p < .05$ .

Table 2  
*Experiment 1 Means and Standard Deviations of Training Outcome Variables by Experimental Condition*

Experimental condition	<i>n</i>	Declarative knowledge	Task performance	Affective reaction
Control				
Regular practice time version	35			
<i>M</i>		6.66	7.34	7.98
<i>SD</i>		1.76	2.93	2.46
Longer practice time version	42			
<i>M</i>		6.19	8.20	7.83
<i>SD</i>		1.97	2.33	1.96
Combined	77			
<i>M</i>		6.40	7.81	7.90
<i>SD</i>		1.88	2.63	2.18
Symbolic mental rehearsal (SMR)	40			
<i>M</i>		7.48	8.19	8.49
<i>SD</i>		1.78	2.40	1.34
Reciprocal peer training (RPT)	36			
<i>M</i>		6.78	7.36	8.10
<i>SD</i>		1.48	2.53	1.82
SMR and RPT	40			
<i>M</i>		7.83	7.43	8.46
<i>SD</i>		1.38	2.45	1.37

Note. *N* = 193.

Hypothesis 1: SMR had a significant positive effect on declarative knowledge (Cohen's  $d = .46$ ), but no effect on task performance ( $d = .32$ ). RPT had no effect on either declarative knowledge ( $d = .07$ ) or task performance ( $d = .01$ ), failing to support Hypothesis 2. The null findings for Hypothesis 2 cannot easily be attributed to insufficient statistical power because the sample size for Experiment 1 ( $N = 193$ ) is estimated to be sufficient to correctly detect a true medium effect for Hypothesis 2 using a .05 level of significance with probability .81, above the customary power level of .80 (Cohen, 1988).

The interaction effects between SMR and RPT were nonsignificant for both declarative knowledge and task performance. There was no significant effect of SMR, RPT, or their interaction on trainees' affective reaction, indicating that the training conditions were not of substantially different quality as perceived by trainees. General computer experience had a significant effect on declarative knowledge and task performance, and spreadsheet experience had a significant effect on task performance but not on declarative knowledge (Table 3). Dropping the covariates from the ANCOVA and rerunning the analysis did not alter the significance of the treatment effects; dropping the treatment variables from the ANCOVA and rerunning the analysis did not alter the significance of the covariates.

Our sample included trainees ranging in age from 18 to 44 years, with a range of prior general computer experience. There is a possibility that including nontraditional students (e.g., older students with less general computer experience) might have altered our findings. To check this possibility, we found that including the interaction of age and experience as an additional covariate explained no incremental variance in either declarative knowledge,  $F(1, 182) = 0.10$ , *ns*, or task performance,  $F(1, 182) = 0.17$ , *ns*. Similarly, because nearly 14% of trainees reported having some spreadsheet experience, we verified that dropping these observa-

tions from the sample did not change the significant effect of SMR reported in Table 3.

Why did the SMR manipulation significantly influence declarative knowledge but not task performance? One interpretation is that SMR exerts no true influence on the underlying skill acquisition that the task performance measure is meant to assess. This would raise the question of why SMR significantly increased declarative knowledge. A plausible alternative explanation is that a ceiling effect may have suppressed variance of the task performance measure used, masking a true effect. Consistent with this account, both trainers reported having informally observed that some trainees completed the performance task in less than 10 min, some took the full 25 min allocated for task completion, and some took varying amounts of time between 10 and 25 min. By not sufficiently limiting performance time, the task performance measure may have failed to discriminate adequately between medium- and high-skill participants who took very different amounts of time to finish, yet achieved similar task performance scores in the end. In Experiment 2 we sought to remedy this shortcoming and probe deeper into the underlying knowledge structures theorized to mediate the effect of SMR on training outcomes.

Hypothesis 2, that RPT would lead to improved training outcomes, was not supported, despite sufficient statistical power. One possible explanation is that it may be difficult to realize the benefits of cooperative learning in a short-term study. A meta-analysis by Slavin (1983) found that RPT is more likely to produce positive effects in longer studies than in shorter ones. Another possibility is that RPT was not effective because both peers were novice users of the target program. It may have been more effec-

Table 3  
*Experiment 1 Analysis of Covariance*

Source of variation	<i>SS</i>	<i>df</i>	<i>MS</i>	<i>F</i>
Declarative knowledge				
Covariate				
Age	0.58	1	0.58	0.21
Gender	5.41	1	5.41	1.92
Computer experience	17.73	1	17.73	6.28*
Spreadsheet experience	0.03	1	0.03	0.01
Treatment				
Symbolic mental rehearsal (SMR)	52.33	1	52.33	18.54***
Reciprocal peer training (RPT)	7.97	1	7.97	2.82
SMR × RPT	0.02	1	0.02	0.01
Error	516.55	183	2.82	
Total	607.98	190		
Task performance				
Covariate				
Age	0.65	1	0.65	0.11
Gender	0.72	1	0.72	0.13
Computer experience	62.53	1	62.53	10.90**
Spreadsheet experience	35.09	1	35.09	6.12*
Treatment				
Symbolic mental rehearsal (SMR)	4.40	1	4.40	0.77
Reciprocal peer training (RPT)	12.84	1	12.84	2.24
SMR × RPT	0.81	1	0.81	0.14
Error	1049.92	183	5.74	
Total	1198.90	190		

\*  $p < .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

tive if each trainee had been teamed with a user who was more experienced in the use of the target program. However, this arrangement is prohibitively expensive in practice when the training involves a large number of users, thus limiting its applicability. Furthermore, many studies on cooperative learning randomly assigned novice students to teams and still found significant benefits for the students in the treatment groups compared with their counterparts in control groups (e.g., Fantuzzo et al., 1989; Greenwood, Delquadri, & Hall, 1989). In fact, Hinds, Patterson, and Pfeffer (2001) reported that novice-instructed novices learned better than expert-instructed novices in a circuit wiring task, because the abstractness of experts' knowledge organization may make it difficult for them to convey their superior knowledge to novices. Also, our design sought to isolate the effects of RPT from possible effects of introducing additional sources of knowledge beyond the common video presentation (e.g., from more knowledgeable peers). Another possible explanation is that variations in the quality of interaction, feedback, or practice among RPT trainees (which were not measured in the present research) may have dampened the effects. A different potential explanation for why RPT was nonsignificant is that trainees were not able to successfully recognize which behaviors in the modeling displays were target skills and separate them mentally from nontarget behaviors (e.g., Jentsch, Bowers, & Salas, 2001). This would not appear to be very likely in the present context because essentially all behaviors presented in the videos concerned target skills. Finally, RPT may truly not be effective for computer skill training.

Another potential limitation of Experiment 1 is that training outcomes (declarative knowledge and task performance) were measured after training only, without the benefit of a pretraining measure to establish a baseline for comparison. The use of pretraining measures of knowledge or performance can permit a more precise assessment of gains made by individual participants and can be especially valuable when random assignment of participants is impractical or when random assignment fails to statistically equalize pretraining knowledge across treatment groups. (Campbell & Stanley, 1963; Cook & Campbell, 1979; Kenny, 1975). Several potential drawbacks exist for using pretests, however. They raise the risk of invalidity resulting from an interaction between testing and treatment (e.g., Cook & Campbell, 1979, p. 68). Campbell and Stanley concluded that it is usually preferable to omit pretests in a randomized control group design "unless there is some question as to the genuine randomness of the assignment" (p. 26). Furthermore, in training contexts where new subject matter is being introduced for the first time to novices (such as the present context), pretests might create undesired side effects by increasing evaluation apprehension or resentful demoralization, and meta-analytic evidence suggests that standard deviations of knowledge assessment measures (e.g., declarative knowledge and task performance) are likely to increase from pretest to posttest, violating homogeneity assumptions required for properly estimating effect sizes (Carlson & Schmidt, 1999).

Training studies commonly use the posttest-only randomized control group design (e.g., Colquitt & Simmering, 1998; Day, Arthur, & Gettman, 2001; Gist et al., 1989), as we did in Experiment 1. To guard against the possibility that randomized groups were unequal in baseline knowledge, Experiment 1 captured pre-treatment self-report measures of general computer and spreadsheet experience (neither of which differed across treatment

groups), which were included as covariates before testing hypothesized treatment effects (Table 3). In the present context, the advantages of full pretest measures of declarative knowledge and task performance appear to be outweighed by potential disadvantages and risks. As a result, in Experiment 2 we did not use pretest measures. Rather than blindly avoiding the use of pretest measures, we encourage researchers to weigh the pros and cons in each specific situation.

## Experiment 2

In Experiment 1 we found evidence that SMR increases training effectiveness. A significant effect was found for one measure of training effectiveness (declarative knowledge) but not the other (task performance). Experiment 1 also did not provide direct evidence confirming the theory that knowledge structures mediate the effect of SMR on training outcomes. In Experiment 2 we further investigate the effectiveness of SMR. To improve the sensitivity of the task performance measure, a 15-min time limit was imposed for task performance testing, instead of the 25-min time limit used in Experiment 1. Declarative knowledge and task performance were measured both immediately posttraining and 10 days later. A measure of trainee knowledge structures was used to tap into the underlying mechanisms theorized to mediate the effects of SMR on training outcomes. Self-report measures were captured to rule out the two rival explanations that the effect of SMR on training outcomes was due to attentional arousal or increased motivation.

### *Immediate Versus Delayed Training Outcomes*

Whereas Experiment 1 only measured training outcomes immediately after training, Experiment 2 additionally examined the delayed benefits of SMR for computer learning. This was done to rule out the possibility that the resulting skills are only weakly established and therefore subject to a rapid rate of decay (e.g., Arthur, Bennett, Stanush, & McNelly, 1998). Previous observational learning studies found that delayed (by 1 week) retention of modeled behavior was significantly aided by symbolic coding, cognitive rehearsal, or both (Bandura & Jeffery, 1973; Bandura, Jeffery, & Bachicha, 1974; Jeffery, 1976). To the extent that SMR generalizes to the computer training domain, these previous findings suggest that the effects of SMR should last beyond immediate trainee performance.

*Hypothesis 3:* Adding symbolic mental rehearsal to modeling-based computer skill training will improve declarative knowledge and task performance measured both immediately after training and after a delay of 10 days.

### *Knowledge Structures in Cognitive Skill Acquisition*

Although the present research embraces the emerging point of view from social cognitive theory and cognitive skill acquisition theory that knowledge structures are key mechanisms underlying the effects of observational learning, methods to directly measure such cognitive structures have only been developed fairly recently (Kraiger, Salas, & Cannon-Bowers, 1995; Salas & Cannon-Bowers, 2001). Experiment 2 directly tests the purported mediating role of such knowledge structures. Various approaches have

been proposed to measure knowledge structures and mental representations (Christensen & Olson, 2002; Goldsmith, Johnson, & Acton, 1991; Mathieu, Heffner, Goodwin, Salas, & Cannon-Bowers, 2000; Naveh-Benjamin, McKeachie, Lin, & Tucker, 1986). Of these, one prominent technique is structural assessment, which reveals how an individual cognitively represents the relationships among key concepts that constitute a knowledge domain (Day et al., 2001; Goldsmith & Kraiger, 1997; Kraiger et al., 1993; Kraiger et al., 1995; Rowe, Cooke, Hall, & Halgren, 1996).

Using structural assessment, a measure of an individual's knowledge structure is obtained by measuring his or her judgments of the degree of relatedness between all possible pairs of key domain concepts and analyzing them using the Pathfinder scaling algorithm to infer a network representation (Schvaneveldt, 1990). As a frame of reference, an expert knowledge structure can be assessed, and a measure of how similar any given individual's knowledge structure is to that of the expert can be calculated using Pathfinder (Goldsmith & Kraiger, 1997). Knowledge structure similarity can then be used as a yardstick of how well the current expertise of a trainee approximates that of a domain expert. Although the psychometric properties of structural assessment measures are not yet fully established (Dorsey, Campbell, Foster, & Miles, 1999), there is evidence that Pathfinder networks represent knowledge structures of conceptual domains better than multidimensional scaling (Acton, Johnson, & Goldsmith, 1994). Furthermore, Day et al. (2001) reported a link between knowledge structures measured using structural assessment and acquisition of a cognitive skill with both cognitive and psychomotor components (playing a complex video game). Because our theoretical rationale for why SMR is expected to influence training outcomes in a computer skill context is due to its influence on relevant underlying knowledge structures, we hypothesize the following:

*Hypothesis 4:* The similarity of trainees' knowledge structures to that of an expert will mediate the effect of symbolic mental rehearsal on declarative knowledge and task performance.

#### *Alternative Explanations*

Two meta-analyses of mental rehearsal in general support a cognitive symbolic account consistent with the present research (Driskell et al., 1994; Feltz & Landers, 1983), even though there remains a dearth of research that attempts to directly assess the impact of mental rehearsal on knowledge structures, as is done in the current study. However, two alternatives to the cognitive symbolic account that still need to be considered are the attentional account and the motivational account. The attentional account argues that mental practice may simply work by enhancing trainees' arousal level during training, which could make them more attentive to the information presented, rather than specifically promoting organized knowledge structures concerning the imagined skill (Feltz & Landers, 1983). The motivational account asserts that mental practice could inadvertently create a Hawthorne-like pseudo-motivational effect (Driskell et al., 1994) that may increase trainees' motivation to learn the skills presented in the training (Driskell et al., 1994; Weiner, 1990). Such increased attention or motivation may influence training outcomes irrespective of any influence on how knowledge is cognitively organized.

Social cognitive theory (Bandura, 1986) acknowledges that observational learning in general may involve attentional and motivational processes that operate distinctly from the symbolic retention processes theorized to establish knowledge structures. Experiment 2 addresses these two alternative mechanisms using self-report measures of attention and motivation.

### *Method*

#### *Participants*

As in Experiment 1, participants were recruited on a voluntary basis from an introductory computer course. A total of 95 students (58% women and 42% men) completed the experimental procedure. Participants' ages ranged from 18 to 26 years. As before, most (92.7%) participants reported that they either had never used Excel or had used it less than 1 hr/week.

#### *Procedure*

Two trainers, a professional instructor (not one of the two involved with Experiment 1) and one of the authors, led trainees through the training protocols following the same procedure as Experiment 1 to introduce themselves, distribute and collect pretest questionnaires, and then implement the assigned training conditions using prepared scripts and stopwatches. After the training protocols were completed, each trainee filled out a posttraining questionnaire, took the declarative knowledge and task performance tests for immediate learning, took the structural knowledge assessment test, and was thanked and dismissed. To assess delayed learning, the same set of declarative knowledge and task performance tests was administered again, without warning, 10 days later in class. To avoid encouraging intentional efforts to find answers during the intervening period, trainees were not informed beforehand that they would be tested again later. The videotape differed from that used in Experiment 1 because of a new release of the software. However, the same demonstrator covered very similar contents, and the same vendor supplied the tape. As before, the tape consisted of five segments, each of which focused on one specific topic: formulas (11 min), advanced formulas (15 min), functions (10 min), advanced functions (11 min), and formatting (13 min).

#### *Experimental Conditions*

Two conditions were examined that were identical except for the SMR intervention. Trainers, computer labs, days, and times of day were counterbalanced across training conditions to control for any potential confounding effects. Participants were randomly assigned to one of eight training workshops offered on 2 consecutive days. There were no significant effects due to trainers, computer labs, days, or times of day on any of the study variables. The average number of trainees in a workshop was 12. There were no significant differences among training conditions in the participant characteristics of age, gender, computer experience, or spreadsheet experience.

*Control group (no SMR).* Trainees in this condition watched the first two video segments for 26 min, practiced the demonstrated skills individually for 15 min, watched the remaining video segments for 34 min, and practiced for another 15 min. Trainees in this condition were not asked to perform any symbolic coding or cognitive rehearsal activities. Total time for this condition was 165 min, including 30 min of hands-on practice. As is true of the other condition in Experiment 2, the remaining 135 min were used for introduction, pre- and posttest questionnaire administration, video observation, declarative knowledge test, task performance test, and structural knowledge assessment.

*SMR group.* For the first two video segments, the tape was played and paused at the end of each segment. During each pause, trainees in this condition summarized the computer operations that had been presented by



the video by writing down key points of the demonstration on a supplied paper. Consistent with Experiment 1, 2 min were allotted for this symbolic coding process after each segment. After the summary of the second segment, trainees practiced the demonstrated computer skills on the computer for 15 min. After the hands-on practice, trainees continued with the remaining three segments of video instruction and demonstration. The tape was paused at the end of each of the three segments, and trainees performed symbolic coding for 2 min during each pause. After the final symbolic coding activity, trainees again had 15 min of hands-on practice. On completion of the hands-on practice, trainees cognitively rehearsed their own summary for 5 min. Trainees were asked to repeat the mental rehearsal as many times as possible and record the number of times they were able to mentally rehearse the key learning points in the 5 min allotted. Total time for this condition was 180 min, including 30 min of hands-on practice and 15 min of SMR activity.

### Measures

Declarative knowledge was measured using 13 multiple-choice questions and task performance was measured using 11 hands-on computer tasks. Because of the release of a new version of the Excel software and updated training materials, both measures were modified from those used in Experiment 1. Seven declarative knowledge questions were reused from Experiment 1. One question was deleted because it no longer applied. Two questions were modified for the updated material. Four new questions were added. Three task performance items were deleted from the previous measure and 2 new items were added. Affective reaction was measured the same as in Experiment 1, showing an internal consistency reliability (Cronbach's alpha) of .92. The manipulation of SMR was verified in three ways. First, as in Experiment 1, the number of trainees who performed any kind of summary activities during their workshops was compared. All trainees ( $n = 48$ ) in the SMR condition performed symbolic coding, whereas only 1 of 47 trainees (2%) in the control condition created any written summary,  $\chi^2(1) = 23.77, p < .001$ . Second, trainees in the SMR condition recorded the number of times they were able to perform the cognitive rehearsal activity. Whereas trainees in the control condition were not given time to cognitively rehearse the skills, trainees in the SMR condition reported having cognitively rehearsed the presented skills 4.08 times on average. Third, four items on the posttraining questionnaire asked participants in both the control and treatment groups to self-report the extent to which they engaged in SMR on a scale from 1 (*strongly disagree*) to 7 (*strongly agree*) with a neutral midpoint of 4: "I had the opportunity to summarize the key aspects of demonstrated computer operations," "I had the opportunity to symbolically process the presented information," "I had the opportunity to mentally visualize the demonstrated computer operations," and "I had the opportunity to mentally practice the demonstrated computer operations." These items had Cronbach's alpha reliability of .93. Results indicate that trainees in the SMR treatment group scored significantly higher than those in the control group on this SMR manipulation check,  $t(93) = 2.18, p < .05$ .

Trainees' knowledge structures were measured using structural assessment (Goldsmith & Kraiger, 1997). Sixteen central concepts (e.g., auto-sum, cell address, formula, operator, best fit) covered by the training were identified, and a questionnaire was created asking respondents to self-report the relatedness between all pairs of concepts, for a total of 120 comparisons [(16 × 15)/2], on a 7-point relatedness scale (7 = *highly related*, 1 = *not at all related*). Consistent with prior research (Goldsmith et al., 1991), trainees were encouraged to glance at the complete set of concept pairs to find highly related and unrelated pairs to serve as anchors and were asked to provide quick and intuitive judgments of relatedness for all pairs. Most trainees completed the assessment within 15 min. Using Pathfinder, the rating data from trainees and experts were transformed into network structure representations. The similarity of each trainee's knowledge structure to that of the composite expert was computed using Gold-

smith and Kraiger's (1997) closeness measure, which is a set-theoretic method for quantifying the similarity between two networks sharing a common set of nodes. Similarity can vary from 0 to 1 and is proportional to the ratio of the number of common links between two nodes and the total number of links in both (Kraiger et al., 1995). Typical reported values of similarity (closeness) are .16–.23 (Kraiger et al., 1995) and .31–.33 (Day et al., 2001).

To establish a referent expert knowledge structure, two expert users who were blind to the study hypotheses completed the same structural assessment questionnaire. Both experts had more than 7 years experience with the Excel software and had extensive job experience in the computing environment. Although many studies have used a single subject-matter expert to construct a referent knowledge structure, as in other measurement contexts it is thought to be more valid to use a combination of experts to form an accurate and robust true-score referent structure (Day et al., 2001). When multiple experts are used, there is evidence that a mechanical combination constructed by averaging SMR similarity ratings provides more robust and stable predictions than a referent structure that is based on expert consensus (Day et al., 2001). Therefore, we used mechanical aggregation of expert judgments as opposed to expert consensus: The two expert ratings were averaged to derive a referent composite structure. Acton et al. (1994) reported that the degree of convergence between multiple experts need not be high for such a mechanical aggregate to be preferred over searching for a single ideal expert structure.

The Pearson correlation between the two experts' concept relatedness ratings was .64 ( $p < .001$ ). The structural similarity between the two expert ratings computed by Pathfinder was .26 ( $p < .05$ ). The similarity scores for each expert's knowledge structure to the mechanically averaged referent structure were .54 ( $p < .001$ ) for Expert 1 and .42 for Expert 2 ( $p < .001$ ). Hypothesis 4 was tested using the mechanically aggregated referent structure but was also corroborated using each expert individually as a referent structure to rule out the possibility that findings may be due to idiosyncrasies of the aggregation method.

Self-report measures of attention and motivation were introduced to rule out the alternative explanation that SMR influences training outcomes because of its effect on attention or motivation instead of knowledge structures. Self-report items on the posttraining questionnaire used a 7-point scale from 1 (*strongly disagree*) to 7 (*strongly agree*) with an anchored neutral midpoint of 4 (*neither agree nor disagree*). Attention was measured using the following four items (Cronbach's alpha = .92): "I paid close attention to the video demonstration," "I was able to concentrate on the video demonstration," "The video demonstration held my attention," and "During the video demonstration, I was absorbed by the demonstrated activities." Motivation was measured using the following four items (Cronbach's alpha = .91): "The training provided information that motivated me to use Excel," "The training helped me see the usefulness of Excel," "The training increased my intention to master Excel," and "The training showed me the value of using Excel in solving problems."

### Results and Discussion

Table 4 presents intercorrelations of Experiment 2 variables and Table 5 shows the means and standard deviations of declarative knowledge, task performance, knowledge structure similarity, and affective reaction by experimental condition. When converted to a common scale, the mean of immediate declarative knowledge scores was very similar to that of Experiment 1 (70.7% vs. 70.5%), and the mean of immediate task performance scores was slightly lower (64.7% vs. 62.9%), possibly reflecting the increased time pressure (Table 5). An ANCOVA was conducted in which the covariates of age, gender, general computer experience, and spreadsheet experience were entered first to control for pretraining individual differences before testing the significance of treatment

Table 4  
Intercorrelations of Experiment 2 Variables

Variable	M	SD	1	2	3	4	5	6	7	8	9	10
Training outcome												
1. DK: immediate	9.18	2.37	—									
2. DK: delayed	8.21	2.55	.70*	—								
3. TP: immediate	6.92	2.83	.55*	.61*	—							
4. TP: delayed	6.04	3.14	.58*	.60*	.72*	—						
5. KSS	0.16	0.08	.37*	.49*	.43*	.41*	—					
6. Affective reaction	7.02	2.06	.27*	.38*	.24*	.34*	.14	—				
Trainee characteristic												
7. Age	18.60	1.34	-.00	.03	-.02	.09	-.08	.19	—			
8. Gender	0.42	0.50	.06	.11	.11	-.01	.19	.10	.03	—		
9. Computer experience	4.61	0.69	.16	.10	.25*	.16	.28*	.10	-.12	.17	—	
10. Spreadsheet experience	0.60	0.63	.15	.15	.09	.22	.21*	.10	.09	-.03	.23*	—

Note. DK = declarative knowledge; TP = task performance; KSS = knowledge structure similarity. Declarative knowledge and task performance scores are the total number of correct answers out of 13 and 11, respectively. Knowledge structure similarity is on a scale of 0 (no points of similarity) to 1 (100% similarity). Affective reaction scores are on a scale of 0 (negative) to 10 (positive). Gender was coded 0 (female) and 1 (male). The measures for computer and spreadsheet experience are the same as those used in Experiment 1. \*  $p < .05$ .

effects. The ANCOVA fully supports Hypothesis 3, indicating that SMR had significant effects on all four training outcomes (Table 6). The effect sizes (Cohen’s  $d$ ) were .60 (immediate declarative knowledge), .54 (delayed declarative knowledge), .51 (immediate task performance), and .56 (delayed task performance). All four observed effect sizes exceed the *medium* effect size of .50, which Cohen (1988, p. 26) argued is “large enough to be visible to the naked eye.” As in Experiment 1, SMR did not influence the affective reaction measure,  $F(1, 93) = 0.06, ns$ , offsetting the rival explanation that a difference in the quality of training implementation across treatments was responsible for the observed training effects of SMR. Computer experience had a significant effect on knowledge structure similarity and task performance but not on declarative knowledge. None of the other covariates were significant (Table 6). Dropping the covariates from the ANCOVA and rerunning the analysis did not alter the significance of the treatment effects; dropping the treatment variables from the ANCOVA and rerunning the analysis did not alter the significance of the covariates. Ruling out potential differences in results for nontraditional students, including the interaction of age and experience as an additional covariate, explained no incremental variance in any of the four training outcomes.

Regression analysis was conducted to test the mediating role (Hypothesis 4) of knowledge structure similarity between the SMR intervention and each training outcome. The following three conditions together indicate mediation (Baron & Kenny, 1986): (a) A significant relationship exists between the independent variable and the hypothesized mediator, (b) a significant relationship exists between the independent variable and the dependent variable, and (c) in the presence of a significant relationship between the mediator and the dependent variable, the previously significant relationship between the independent variable and the dependent variable is no longer significant or the strength of the relationship is significantly decreased. These three tests were repeated for each training outcome.

Regression results supported all three of Baron and Kenny’s (1986) criteria for the mediational effect specified in Hypothesis 4 for all four training outcomes (Table 7). First, SMR had a significant effect on knowledge structure similarity. Second, SMR had a significant effect on each of the four training outcomes. Third, when SMR and knowledge structure similarity were both entered as independent variables, the previously significant effect of SMR on each learning outcome became nonsignificant, whereas the effects of knowledge structure similarity remained significant for

Table 5  
Experiment 2 Means and Standard Deviations of Training Outcome Variables by Experimental Condition

Experimental condition	Declarative knowledge		Task performance		KSS	Affective reaction
	Immediate	Delayed	Immediate	Delayed		
Control ( $n = 47$ )						
<i>M</i>	8.49	7.53	6.22	5.18	.13	6.97
<i>SD</i>	2.27	2.48	3.17	3.17	.06	2.08
SMR ( $n = 48$ )						
<i>M</i>	9.85	8.87	7.60	6.88	.19	7.07
<i>SD</i>	2.29	2.45	2.28	2.91	.07	2.06

Note.  $N = 95$ . KSS = knowledge structure similarity; SMR = symbolic mental rehearsal.

Table 6  
Experiment 2 Analysis of Covariance

Source of variation	SS		df	MS		F	
	Immediate	Delayed		Immediate	Delayed	Immediate	Delayed
Declarative knowledge							
Covariate							
Age	0.00	0.17	1	0.00	0.17	0.00	0.03
Gender	0.68	5.54	1	0.68	5.54	0.13	0.90
Computer experience	6.65	1.33	1	6.65	1.33	1.27	0.22
Spreadsheet experience	4.29	6.85	1	4.29	6.85	0.82	1.11
Symbolic mental rehearsal	38.78	37.42	1	38.78	37.42	7.38**	6.06*
Error	467.74	550.04	89	5.26	6.18		
Total	527.96	609.79	94				
Task performance							
Covariate							
Age	0.00	6.62	1	0.00	6.62	0.00	0.73
Gender	3.81	0.84	1	3.81	0.84	0.52	0.09
Computer experience	34.64	12.58	1	34.64	12.58	4.68*	1.39
Spreadsheet experience	0.12	22.13	1	0.12	22.13	0.02	2.44
Symbolic mental rehearsal	39.78	55.36	1	39.78	55.36	5.38*	6.10*
Error	658.51	807.60	89	7.40	9.07		
Total	751.66	928.12	94				
Knowledge structure similarity							
Covariate							
Age	0.00		1	0.00		0.74	
Gender	0.01		1	0.01		3.15	
Computer experience	0.02		1	0.02		3.94*	
Spreadsheet experience	0.01		1	0.01		2.17	
Symbolic mental rehearsal	0.07		1	0.07		15.43***	
Error	0.39		89	0.00			
Total	0.53		94				

\*  $p < .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

all four training outcomes. The results provide consistent empirical evidence that knowledge structure similarity mediates the effect of SMR on training outcomes supporting Hypothesis 4 (Figure 1). To rule out the possibility that mechanically combining the knowledge structures of two experts to form a referent structure may produce a composite expert structure that does not truly represent any individual expert, we additionally ran the full analysis separately using Expert 1 and Expert 2 as referent structures, confirming the same pattern of full statistical mediation for both experts on all four training outcomes as reported in Table 7. Ruling out two rival explanations, there was no significant effect of SMR on either attention,  $t(93) = 1.50$ , *ns*, or motivation,  $t(93) = -0.02$ , *ns*.

### General Discussion

The current research found that SMR increased the effectiveness of modeling-based computer skill training and did so by altering trainees' knowledge structures. Experiment 1 found that SMR significantly increased declarative knowledge but not task performance. After correcting a shortcoming of the task performance measure, Experiment 2 found that SMR significantly increased both declarative knowledge and task performance, each measured both immediately posttraining and 10 days later. Effect sizes for

Experiment 2 all exceeded .50 (Cohen's *d*), indicating that the gains are of both practical and statistical significance. Experiment 2 went further to directly test the underlying theoretical mechanisms purported to link SMR to training outcomes. As hypothesized, results indicate that the effects of SMR on training outcomes can be attributed to the mediating role of how similar each trainee's knowledge structure is to that of a composite domain expert.

Various features of the current research mitigate the risk that the findings are spurious. Both experiments used completely random assignment to treatment groups and counterbalancing to control for potential confounding effects of trainer, time of day, training room, and day of week. By using representative professional trainers (both trainers in Experiment 1 and one of two trainers in Experiment 2) who were blind to the hypotheses, we not only ruled out demand characteristics and experimenter expectancies as threats to validity but also confirmed the practicality of real-world implementation of the SMR training intervention. Trainers followed detailed scripts using stopwatches to standardize delivery of the training protocols. Care was taken to isolate the training interventions from other possible differences across treatments. For example, Experiment 1 included short and long versions of the control condition to rule out the possibility that effects were due to the

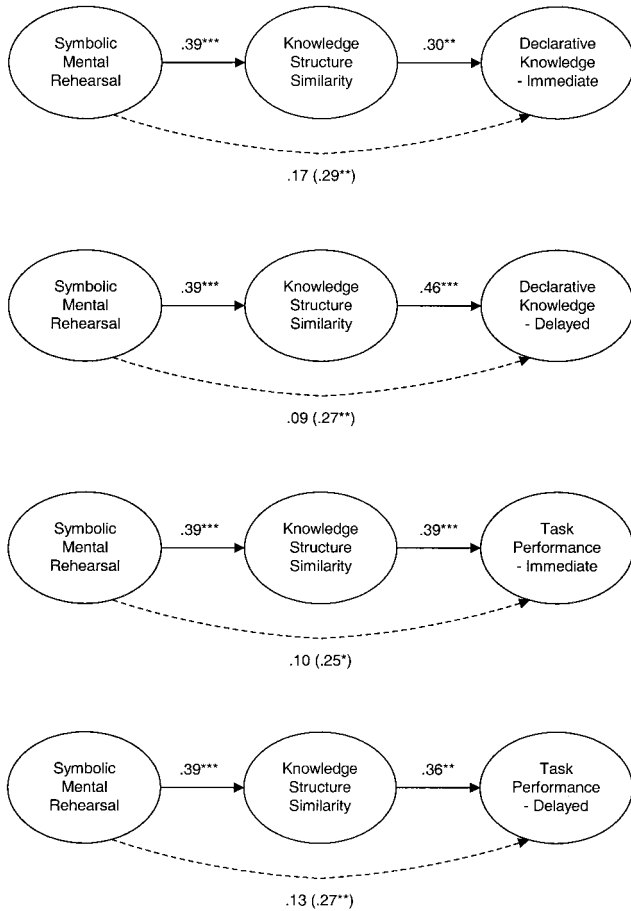


Figure 1. Knowledge structure similarity fully mediates effects of symbolic mental rehearsal on declarative knowledge and task performance (Experiment 2). Values are standardized regression coefficients. Values in parentheses represent the direct effects of symbolic mental rehearsal prior to controlling for its indirect effects through knowledge structure similarity. Dashed lines indicate that significant direct effects of symbolic mental rehearsal became nonsignificant after controlling for knowledge structure similarity. \*\*  $p < .01$ . \*\*\*  $p < .001$ .

extra training time needed to implement training manipulations per se, rather than how that time was specifically used. Manipulation checks confirmed that training interventions were delivered as intended. Experiment 2 included self-report measures to rule out the alternative explanations that SMR improves training through effects on attention or motivation. Overall, many precautions were taken to ensure the validity of the findings. The robustness of the current findings is underscored by the fact that the test of full mediation of SMR through knowledge structures was confirmed across 12 separate mediational tests involving four different training outcome measures (immediate and delayed declarative knowledge and immediate and delayed task performance) each examined using three different referent expert knowledge structures (Expert 1, Expert 2, and a mechanical aggregate of both).

From a practical standpoint, this research shows that SMR is an effective intervention that should be added to modeling-based computer skill training to further improve its effectiveness. The

present research responds to calls for more research to improve behavior modeling and better understand its effectiveness in various practical conditions (Baldwin, 1992; Tannenbaum & Yukl, 1992; Werner, O’Leary-Kelly, Baldwin, & Wexley, 1994). The instructions needed to administer the symbolic coding and cognitive rehearsal activities to trainees, and the observations needed to capture process traces to confirm the quality and quantity of trainee engagement in these activities, can potentially be programmed into technology-mediated learning environments including CD-ROM or Internet technology. Technology-mediated training delivery promises further improvements on the implementation of SMR, for example, by further controlling the quality of verbal summaries provided by trainees during symbolic coding and by providing timely feedback and remediation to material on insufficiently mastered concepts. Future research is needed to investigate the effectiveness of various design configurations for such technology-mediated delivery of SMR, which would accelerate its accessibility, affordability, and diffusion among the numerous trainees who so urgently need these skills in a just-in-time manner. Given that most occupations increasingly require computer skills and most organizations provide their employees with computer skill training, the present findings offer a substantial contribution to future training practice and workplace productivity.

What implications do our findings have beyond the domain of computer skills? Knowledge structures (or mental models) are attracting increased attention from researchers across such diverse learning and performance domains as negotiation (Bazerman, Curhan, Moore, & Valley, 2000), team collaboration (Mathieu et al., 2000), tactical military decision making (Kraiger et al., 1995), and comprehension of human biological systems (Chi et al., 1994). We suspect that the role of knowledge structures in mediating the effect of SMR on training outcomes may well generalize across multiple skill domains. Our stance that the role of knowledge structures as a learning mechanism is not highly domain specific is thematically aligned with some recent theorizing in cognitive

Table 7  
Experiment 2 Regression Tests for Mediating Role of Knowledge Structure Similarity

Step and dependent variable	R <sup>2</sup>	Independent variable	B	SE B	β
Step 1. KSS	.14***	SMR	0.006	0.01	.39***
Step 2. DK: immediate	.07**	SMR	1.37	0.47	.29**
		DK: delayed	1.34	0.51	.27**
		TP: immediate	1.38	0.57	.25*
		TP: delayed	1.69	0.62	.27**
Step 3. DK: immediate	.14***	SMR	0.82	0.49	.17
		KSS	9.52	3.28	.30**
		DK: delayed	0.45	0.50	.09
		KSS	15.54	3.34	.46***
		TP: immediate	0.53	0.57	.10
		KSS	14.77	3.85	.39***
		TP: delayed	0.83	0.64	.13
		KSS	15.02	4.23	.36**

Note. KSS = knowledge structure similarity; SMR = symbolic mental rehearsal; DK = declarative knowledge; TP = task performance. Steps 1–3 refer to the three conditions for testing mediation outlined by Baron and Kenny (1986). \*  $p < .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

psychology on the domain generality of skill acquisition mechanisms spanning a range of cognitive and perceptual-motor domains (e.g., Markman & Gentner, 2001; Rosenbaum, Carlson, & Gilmore, 2001). It will be important to examine the generalizability of the present findings to other skill domains and to establish key boundary conditions. Furthermore, despite our statistical support for full mediation, it is important to acknowledge that additional mechanisms beyond knowledge structures may play important mediating roles between observational learning and skill acquisition (Bandura, 1986, 1997) and that those roles could vary across contexts. Nevertheless, we regard the present research as part of the quest for relatively domain-general learning and skill acquisition processes.

### Conclusion

Insufficient computer skills are a key reason why organizational investments in information technology so often fail to deliver desired productivity gains. Improvements in computer skill training therefore represent a key driver of ongoing productivity improvement. The current research seeks to advance the state of the art of computer skill training, starting with behavior modeling as the currently established benchmark of best practice. We introduced a simple, practical, and effective training intervention that improved on behavior modeling: SMR. We demonstrated that SMR improves declarative knowledge and task performance in a representative training situation. Moreover, we went further to examine why SMR has this positive effect on training outcomes. By showing that changes to relevant knowledge structures are a key mediational process by which SMR produces training improvements, we both bolster the credibility of the findings and open up possibilities that SMR may generalize to other skill sets having a cognitive component.

Instructional designers and training professionals should immediately incorporate SMR into their training protocols to reap its benefits. If practitioners are not already using behavior modeling, they should incorporate this first, because of its effectiveness demonstrated by many prior studies. When added to modeling-based training, SMR is not only effective but also inexpensive and time efficient, requiring only 15 min to administer within a 2-hr training workshop. It is important that administering SMR does not require any highly specialized abilities on the part of trainers. This is evidenced by the fact that in our studies representative professional trainers were hired to perform SMR training using instructional scripts and successfully did so with minimal orientation to the technique.

In summary, the contribution of this article is that we (a) identified a practically useful training technique from supervisory training that had not previously been exploited for advancing modeling-based computer skill training, (b) developed a theoretical rationale as to why the technique should generalize to computer skill training, (c) demonstrated that the training technique does actually improve computer skill training in a representative situation, (d) ruled out many rival explanations for why the technique was found to be effective, (e) confirmed the underlying cognitive mechanism theorized to be responsible for the effect of the training technique, and (f) made a compelling case that practitioners should add the technique to their repertoire of training interventions. These discoveries about the effectiveness of symbolic mental

rehearsal for computer skill training therefore have important and actionable implications for both researchers and practitioners.

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