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The Role of Information Reduction in Skill Acquisition

HILDE HAIDER

Institut für Kognitionsforschung, Universität der Bundeswehr, Hamburg, Germany

AND

PETER A. FRENSCH

University of Missouri at Columbia

Theories of skill acquisition assume that the effects of practice on task performance are due to either qualitative changes in the task structure, an increased efficiency of performing individual task components, an increased efficiency of performing sequences of task components, or some combination of these mechanisms. We propose an extension to the existing theories by arguing that for many tasks, practice affects which information is processed. More specifically, we argue that people learn, over the course of practice, to separate task-relevant from task-redundant information, and to limit their processing to relevant aspects of the task. In three experiments, subjects verified alphabetic strings, such as M [4] R S T. Strings were correct if they followed the alphabet when the number of letters, given by the digit in parentheses, was skipped. Strings were constructed such that errors occurred only within the initial ''letter–digit–letter'' triplet. Analyses of subjects' RTs for strings of varying lengths demonstrated that: (a) subjects were able to distinguish relevant from redundant task information, and to limit their processing to the relevant information, (b) the ability to reduce the amount of information that is processed takes time and develops gradually over the course of practice, and (c) the mechanism underlying this ability appears to be largely stimulus-independent in the sense that structural components of a task are ignored, rather than specific task information. The findings and their implications for general theories of skill acquisition are discussed.

It is well known that practice affects task performance. Before much practice, performance is slow and effortful for many tasks. With practice, perfor-

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Address correspondence and reprint requests to Hilde Haider, Institut für Kognitionsforschung, Universität der Bundeswehr Hamburg, Postfach 700822, D-22008 Hamburg, Germany, or Peter A. Frensch who is now at the Max-Planck-Institute for Human Development and Education, Lentzeallee 94, D-14195 Berlin, Germany. E-mail: p_HA27@unibw-hamburg.de (H.H.), frensch-@mpib-berlin.mpg.de (PAF).

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mance becomes faster and less effortful. For example, mentally adding several 2-digit numbers, a rather cumbersome, error-prone, and time-consuming process initially, is eventually, after practice, performed rather quickly, smoothly, and without error (Siegler, 1988). Existing theories of skill acquisition generally assume that the effects of practice on task performance are due to either (a) qualitative changes in the effective task structure [i.e., strategy changes (Cheng, 1985; Logan, 1988, 1990, 1992)], (b) an increased efficiency of performing *individual* task components (e.g., the concept of strengthening in Anderson's [1982, 1987, 1992] ACT* theory), (c) an increased efficiency of performing *sequences* of task components [e.g., Anderson's (1982, 1987) composition mechanism, Newell & Rosenbloom's (1981) chunking mechanism], or (d) some combination of these mechanisms (e.g., Anderson, 1987).

In the present article, our focus is on qualitative changes in the task structure that occur as a result of task practice. Theories of skill acquisition that take this perspective generally assume that the way in which a task is performed changes as a result of task practice. Cheng (1985), for example, argues that improvements in task performance can be due to ''a restructuring of the task components so that they are coordinated, integrated, or reorganized into new perceptual, cognitive, or motor units.'' (p. 414). Similarly, Logan (1988) maintains that many tasks that are initially performed by following a mental algorithm (e.g., counting to solve simple addition problems), are eventually solved by single-step direct-access retrieval of the solution from long-term memory. Logan assumes that each time a person performs a task, a new memory trace (i.e., instance) is laid down in long-term memory. When the task is performed again, all traces associated with the task are activated. The solution of the task can then be described as a race between the algorithm that is known to solve the task and the retrieval of an adequate memory trace. With increasing practice, the likelihood that an appropriate trace can be retrieved before the algorithm finishes its run increases (Logan, 1988). In essence, Logan's (1988, 1992) model (see also Siegler & Jenkins, 1989) represents an elaborated, yet special, case of Cheng's more general viewpoint.

Anderson (1983, 1987) has even described a computational mechanism that is capable of altering the way a task is processed if given sufficient practice. Anderson (see also Fitts, 1964) assumes that knowledge about how to perform a task is initially stored in a declarative knowledge base and is transformed into procedural knowledge relatively early on in practice (Charness & Campbell, 1988; Frensch, 1991, 1994). With continuing practice, the resulting procedures are strengthened such that they can be located more quickly in long-term memory, and are composed. Composition collapses a pair of consecutively performed procedures into a larger new procedure that contains fewer steps and can thus be performed more quickly than the original procedures. Essentially, composition changes the way a task is performed by eliminating some of the subgoals that are initially necessary to perform the task. Moreover, Anderson (1987) discusses two additional mechanisms, generalization and discrimination, that are capable of further modifying the way in which a task is processed.

One important, albeit frequently implicit, assumption that underlies the view that practice effects are due to qualitative changes in the task structure (e.g., Anderson, 1987; Cheng, 1985), is that the task information that is processed does not change with practice. That is to say that people attend to, encode, and process the same task information regardless of their level of task proficiency. What changes is *how* the information is processed but not *which* information is processed. For example, the problem of finding the sum of 5 threes can be solved by applying either 4 addition operations or 1 multiplication operation (Cheng, 1985). Early on in skill acquisition, people are more likely to solve the problem by using the addition operations, while later in skill acquisition, they are more likely to solve the same problem by using a single multiplication operation (Siegler, 1988). In both cases, however, the same information is processed, namely ''3 + 3 + 3 + 3 + 3.''

A second example may help to show the inadequacy of the assumption that the same information is processed at all levels of proficiency. Compare a novice and an expert chessplayer. The novice, before making a move, is likely to scan the entire board of chess pieces and to consider the impact of a potential move on every single chess piece that is in the possession of the opponent. An expert chess player, in contrast, ''knows'' which pieces to pay attention to and which pieces to ignore (e.g., Frensch & Sternberg, 1991; Holding, 1985). In essence, the expert player has learned to distinguish important from unimportant information and processes only the important information.

We therefore propose an extension to the present models of skill acquisition by arguing that for many, although certainly not all tasks, practice affects *which* information is processed. More specifically, we argue that people learn, over the course of practice, to separate task-relevant from task-redundant information and to limit their processing to relevant aspects of the task. Thus, the information processed early in skill acquisition may be qualitatively different from the information processed late in skill acquisition. One implication of this position is that changes in RT patterns across practice (i.e., power law of practice) may at least partially reflect systematic reductions in the amount of information that is processed, rather than changes in the efficiency with which task components can be performed.

The general idea that task-irrelevant information can be ignored is by no means new, although the idea is typically stated in terms of its opposite. That is, it is assumed that task-relevant information is learned rather than that irrelevant information is ignored. For instance, in a classic article, J. J. and E. Gibson (1955; see also Gibson, 1979) made the distinction between differentiation and enrichment theories of perceptual learning. Enrichment theories argue for a distinction between sensation and perception, and assume that subjects acquire associations among mental representations of stimuli as learning accrues. This view is not unlike the views championed by current theories of skill acquisition (e.g., Anderson, 1987; Logan, 1988, 1992). Differentiation theories, like the Gibson's own theory, in contrast, assume that subjects acquire knowledge about those features that distinguish perceptual categories. More specifically, for Gibson and Gibson, learning is equivalent to a differentiation of perception such that the observer becomes increasingly sensitive to relevant environmental cues. With practice, the observer learns to isolate invariants, patterns, and transformations within the stimulus array. In other words, the Gibsons argue that with practice perception becomes increasingly discriminative. What does not change with practice are the percepts or memory images. Thus, in essence, the Gibsons claim that subjects learn which information is relevant to distinguishing perceptual categories.

Similar ideas have been expressed by Trabasso and Bower (1968), Neisser (1976), Shiffrin and Schneider (1977), and others. Trabasso and Bower proposed that concept learning proceeds in two stages. In the first stage, subjects learn to attend to the relevant attributes, and in the second stage, they learn to associate relevant attributes with responses. In essence, subjects first learn what to attend to and then how to interpret it. Neisser (1976) argued that skill acquisition is largely the schooling of attention, which involves learning what to ignore as well as learning what to select. Neisser assumed that anticipatory schemata guide perception, and that these schemata can be used to direct attention to relevant aspects of a task. In their model of automatization, Shiffrin and Schneider (1977) argued similarly that, at least in consistent mapping conditions, subjects learn to ignore distractors and attend to relevant targets.

Recent empirical findings in the areas of expertise and concept development are consistent with these general views. For example, analyses of expert–novice differences suggest that experts use different information than novices when they solve a problem. On the basis of a survey study, Shanteau (1992) concluded that ''where experts differ from novices is in what information is used, not how much'' (p. 81). Indeed, experts' problem solutions appear to be frequently based on different aspects of the situation than novices' solutions (Christensen, Murry, Holland, Reynolds, Landay, & Moore, 1981; Hoffman, Slovic, & Rorer, 1968; Myles-Worsley, Johnston & Simons, 1988). For example, Myles-Worsley *et al.* reported that radiological experts bias their perception toward specific classes of information, and differ from novices in their memory for abnormal X-ray films, although they do not differ from novices in their memory for normal X-ray films. Shapiro and Raymond (1989) analyzed the eye movements of experts and novices in the video game Space Fortress, and found that experts and novices differed in how long they attended to redundant information in the stimulus material. When the authors trained novices on the basis of experts' eye movements, they found that the trained subjects were reliably faster in performing a new task than untrained subjects. Thus, one hallmark of expertise appears to be the availability of an internalized evaluation function that helps to distinguish relevant aspects of a task from redundant aspects.

In research on concept development, Regehr and Brooks (1993) have also found that subjects appear to form concepts on the basis of relevant individual stimulus dimensions and to ignore redundant dimensions. Similarly, Lassaline and Logan (1993) reported that parts of the environmental information, such as color, appear to be ignored when they are not relevant for subjects' numerosity judgments. Logan and Etherton (1994) have recently argued that attention might be the crucial variable that mediates between environment and memory. These authors found that information that was attended to, affected a subsequent target search, whereas information that was not attended to, did not.

In summary, the argument that people can learn to distinguish between relevant and redundant task information is not a new one, but one that has been made by various authors in various areas of research. The existing empirical evidence is generally consistent with this argument. Thus far, however, the idea has not been directly applied to the domain of skill acquisition nor has it been tested directly in that domain. We argue therefore that successful skill acquisition involves, in part, a shift from processing all task information to processing only those aspects that are task-relevant.

The experiments reported below serve two main purposes. First, they were conducted in order to empirically demonstrate that the reduction of information is an inherent component of skill acquisition. That is, subjects learn with practice to distinguish relevant from redundant task information and to limit task processing to the task-relevant information. The second purpose of the experiments was to begin exploring the properties of the learning mechanism(s) that underlies the ability to reduce information. Specifically, we asked whether the mechanism needs to be triggered by the environment (through task instructions) or operates in the absence of instructions, and whether it is stimulus-specific or general.

OVERVIEW OF EXPERIMENTS

The main goals of Experiments 1 and 2 were to test if subjects can ignore task-redundant information when they are directly instructed that a task contains redundant information (Experiment 1), and when they are *not* told that some of the task information is redundant (Experiment 2). In Experiment 3, we examined the stimulus-specificity of the effect. That is, we tested whether the effect was specific to the materials practiced or would transfer to new materials that subjects had never encountered before.

The main experimental task we used was one in which subjects were asked to verify alphabetic strings that were presented on a microcomputer screen. The strings consisted of an initial letter that was followed by a digit in brackets, and a varying number of additional letters (0–4). Examples of alphabetic strings are ''D [4] I,'' ''E [4] K L M,'' ''D [4] J K,'' and ''E [4] J K L.'' The digit in brackets was to be interpreted as the number of letters that was to be skipped when the alphabet was recited (only the digit 4 was used). Strings were correct when the sequence of letters followed the alphabet and were incorrect when the sequence did not follow the alphabet. Thus, ''D [4] I'' would be a correct string because D is followed by I when the 4 intervening letters E, F, G, and H are skipped. ''D [4] J,'' in contrast, would be an incorrect string because the letter that follows D, when 4 letters are skipped, is I, not J. The incorrect strings were constructed by replacing the correct letter with its successor in the alphabet. Thus, ''D [4] J'' was a possible incorrect string whereas ''D [4] H'' was not.

The length of the strings varied from a total of three symbols (i.e., letter–digit–letter) to a total of seven symbols (i.e., letter–digit–letter–letter–letter–letter–letter). Subjects in some of the experimental conditions in Experiments 1–3 were told that errors could occur anywhere in the string, whereas subjects in other conditions were told that errors could occur only in the initial ''letter–digit–letter'' triplet of the string or were not told specifically where errors could occur. In the strings that were actually presented to subjects, errors occurred only within the initial ''letter–digit–letter'' triplet (except for one experimental condition in Experiment 1). Consequently, the additional letters (i.e., string positions 4–7) were always correct, and were thus redundant for successfully performing the task.

This setup allowed us to infer whether or not subjects ignored the task-redundant information from the verification times for correct strings of varying lengths. If subjects are not able to ignore the redundant letters, then their verification times should vary systematically with string length such that verification times are longer for longer strings. If subjects are, however, able to ignore the redundant information, then a string length effect should not be observed. Of course, the effect of information reduction, that is, of ignoring the redundant information, was expected to gradually develop with practice. Thus, by comparing the string length effects across levels of practice, we expected to find a gradual transition from processing the redundant information to ignoring this information.

EXPERIMENT 1

The primary purpose of Experiment 1 was to test if subjects are able to ignore task-redundant information when explicitly told to do so. This test was accomplished in two different ways: (a) by a between-subjects manipulation that compared subjects who were told to ignore the redundant string components with subjects who were shown only the relevant string components, and (b) by a within-subjects manipulation where verification times to strings of varying lengths were compared.

There were four different between-subjects conditions: In the Not-Informed condition, subjects were not told that some of the letters within the strings were redundant. Instead, these subjects were asked to evaluate the entire strings and to press one key for strings that followed the alphabet and to press another key for strings that did not follow the alphabet. In the Informed condition, subjects were instructed that errors could occur only within the initial ‘‘letter–digit–letter’’ triplet. In the Informed + Ignore condition, subjects were also instructed that errors could only occur within the initial triplet, but were told, in addition, that they should try to ignore the redundant letters of the strings (i.e., all letters that followed the initial ‘‘letter–digit–letter’’ triplet). Finally, in the Control condition, subjects evaluated strings that contained only the initial ‘‘letter–digit–letter’’ triplets. Thus, if subjects in the Informed and Informed + Ignore conditions indeed learn to process only the initial part of the alphabetic strings, then they should, after some practice, have responded just as quickly as subjects in the Control condition (Neisser & Becklen, 1975; but see Lambert, Spencer, & Mohindra, 1987; Müller & Rabbitt, 1989; Shiffrin & Schneider, 1977, for some evidence suggesting that subjects can be influenced by redundant information even when they try to actively ignore this information).

In addition, the length of the strings that were verified varied between 3 and 7 in all but the Control condition. This within-subjects manipulation allowed for an additional test of our information reduction hypothesis in three of the four experimental conditions. We expected that verification times for correct strings would vary systematically with string length in the Not-Informed condition but not in the remaining conditions where subjects were informed that redundancies existed (i.e., Informed and Informed + Ignore conditions).

Method

Subjects

Subjects were 27 female and 38 male undergraduate students at the University of Missouri at Columbia who received course credit in introductory psychology for participating in the experiment. The subjects ranged in age from 18 to 33 years ($M = 19.6$, $SD = 1.95$). Because of technical problems, data from four subjects were lost and could not be entered into the data analysis.

Materials

Stimulus and apparatus. A total of 50 correct and 55 incorrect alphabetic strings was used. Each of the correct strings consisted of an initial ‘‘letter–digit–letter’’ triplet (e.g., D [4] I) that began with the letter D, E, F, G, H, I, J, K, L, or M. Each of these 10 correct triplets was followed by 0, 1, 2, 3, and 4 additional letters such that the entire string followed the alphabet. The digit in brackets was always equal to 4. There were, thus, $5 \times 10 = 50$ correct letter strings that varied in length from 3 to 7. The construction of 50 of the 55 incorrect letter strings followed the construction of the correct letter strings, except that the letter to the right of the digit always occupied the position in the alphabet that followed the correct one (e.g., D [4] J instead of D [4] I). The construction of the 5 remaining incorrect strings varied across experimental condition and is described in the Procedure section below.

Strings were presented at the center of a 9-in. (22.9 cm) diagonal video screen controlled by a Macintosh SE microcomputer. The letters were approximately 0.3 cm $\times$ 0.3 cm in size. Consecutive letters appeared approximately 0.2 cm apart on the screen. Subjects responded by pressing either the ''z'' or the ''3'' key on the second row from the bottom on an extended Macintosh keyboard. Half of the subjects were instructed to use the ''3'' to indicate that a string was correct and the ''z'' key to indicate that the string was incorrect; for the other half, the key assignment was reversed.

Procedure

Subjects were randomly assigned to one of the four experimental conditions, and were tested in groups of up to 10 people in a large, moderately lit room that contained 10 Macintosh SE microcomputers. Each subject was seated in front of a microcomputer. The experimental session began with computerized instructions. Subjects were told that their task was to verify alphabetic strings. They were informed about what constituted a correct string and were shown examples of correct and incorrect strings. Then, a short training session followed in which subjects evaluated 10 training strings. If they made more than three errors on the training strings, the instructions and training strings were repeated.

When they had successfully completed the training session, subjects in the Control condition began evaluating the experimental strings. Subjects in the remaining experimental conditions received additional instructions before they began working on the experimental strings. In the Not-Informed condition, subjects were told to pay attention to the entire string because errors could occur anywhere in the string. Subjects in both the Informed and Informed + Ignore conditions were told, in contrast, that errors could occur only in the initial ''letter-digit-letter'' triplet of the strings. Subjects in the Informed + Ignore condition were instructed, in addition, to actively ignore all letters to the right of the initial triplet.

To ensure that subjects in the Not-Informed condition would indeed evaluate the entire string, subjects in this group received 5 strings per practice block that contained errors in the latter string part (position of error was randomized). In order to equalize the number of strings across experimental conditions, subjects in the remaining three groups received 5 additional strings in which the letter immediately following the digit was incorrect. These 5 incorrect strings differed from the remaining incorrect strings in the three conditions in that the presented letter preceded, in the alphabet, the letter that would have been correct (e.g., I instead of J).

Overall thus, 50 correct and 55 incorrect strings were each presented once during each practice block in all experimental conditions except the Control condition. Because string length was not varied in the Control condition, subjects here received the 10 correct initial ''letter-digit-letter'' triplets 5 times in each practice block. In addition, they received each of the 10 corresponding incorrect triplets 5 times per block; the remaining 5 incorrect triplets were shown only once in every block, and were constructed as described in the previous paragraph. In all experimental conditions, the experimental session consisted of 8 practice blocks.

In all, subjects thus verified 400 correct and 440 incorrect alphabetic strings that were divided into 8 practice blocks of 105 strings each. The order in which strings were presented was randomly determined for each subject in each practice block.

Each trial began with the presentation of a fixation point at the center of the screen for 500 ms. The disappearance of the fixation point was followed by the presentation of an alphabetic string that remained on the screen until a response was made. When the subjects responded incorrectly, an error prompt with a 600 ms duration appeared on the screen and a 440 Hz-tone was sounded. Then, the screen went blank for 1000 ms, and the next fixation point appeared. When subjects responded correctly, the response was followed immediately by the next fixation point. After every trial block, subjects were given feedback about how well they were doing in terms of their error rate and their mean response time during the preceding trial block, and were allowed to take a short break. The entire experiment lasted between 60 and 90 min, depending on experimental condition.

Design

The two main dependent variables were the median response time per trial block, and the mean error rate per trial block. There were three independent variables: (a) experimental condition

(Control vs Not-Informed vs Informed vs Informed + Ignore; between-subjects), (b) block of practice (1 through 8; within-subjects), and (c) string length (3 through 7; within-subjects).

Results

For each subject, mean error rates were computed for each trial block. Data from subjects who made more than 10% errors in each of the 8 trial blocks were discarded ($N = 2$). This resulted in 15 remaining subjects in the Control condition, 15 subjects in the Not-Informed condition, 15 subjects in the Informed condition, and 14 subjects in the Informed + Ignore condition. For all subjects, median response times (RTs) for correct responses to correct and incorrect strings were computed for each practice block and each string length. Because 5 of the 105 strings that were presented in each trial block differed across experimental conditions, these 5 strings were excluded from all analyses.

There was no speed–accuracy trade-off. The correlation between response time and mean percent error rate was $r(472) = -.06$. The mean error rate per block ranged from 3.44% to 9.43%.

Our discussion of the results is divided into three sections. First, we compare the mean overall verification times for the four experimental conditions. Then, we consider the effect of string length on subjects' verification times for correct strings. And finally, we discuss the string length effect for incorrect strings.

Overall Results

Figures 1a (for correct strings) and 1b (for incorrect strings) display the mean correct response times (averaged over all string lengths) in the four experimental conditions over the 8 trial blocks.[1]

As can be seen, RTs declined rather substantially with practice in all experimental conditions and for both correct and incorrect strings. Two results depicted in the figures seem especially noteworthy. First, the RTs for subjects in the Not-Informed condition declined much less than those for subjects in the remaining conditions. And second, a comparison of Figs. 1a and 1b shows that the RTs for correct alphabetic strings were always faster than the RTs for incorrect strings, except in the Not-Informed condition where the pattern was reversed.

A 4 (condition) $\times$ 8 (practice block) $\times$ 2 (string type) mixed-design analysis of variance (ANOVA) on the RTs indicated reliable main effects of condition, $F(3,55) = 7.14$, $MSe = 6356354.2$, $p < .001$, practice block, $F(7,385) = 155.58$, $MSe = 674701.5$, $p < .001$, and string type, $F(1,55) = 15.66$, $MSe = 109143.8$, $p < .01$, as well as reliable interactions between condition and

[1] Confidence intervals shown here and in subsequent figures were constructed separately for each experimental group where appropriate (Loftus & Masson, 1994).

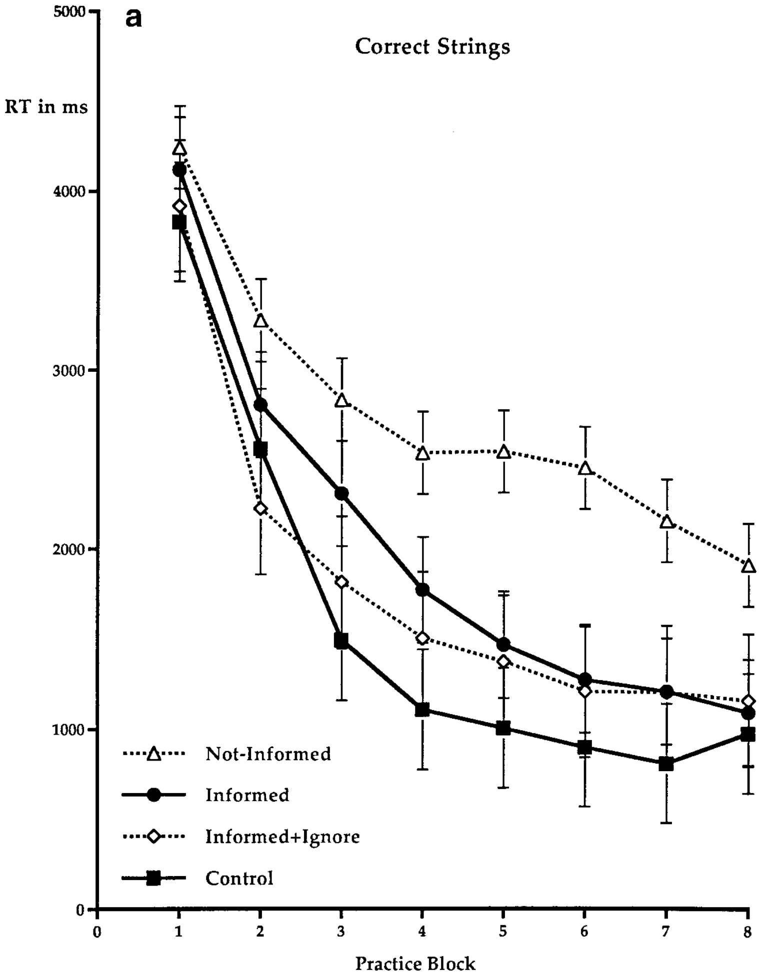

FIG. 1. (a) Mean RTs for correct strings (Experiment 1). Error bars represent 95% within-subject confidence intervals. (b) Mean RTs for incorrect strings (Experiment 1). Error bars represent 95% within-subject confidence intervals.

practice block, $F(21,385) = 1.99$, $MSe = 674701.5$, $p < .01$, and between condition and string type, $F(3,55) = 11.62$, $MSe = 109143.8$, $p < .01$. The interaction between practice block and string type, and the three-way interaction between condition, practice block, and string type were not reliable, both $ps > .05$.

As can be seen by comparing Figs. 1a and 1b, the reliable interaction between condition and string type was due to the fact that the RTs for correct alphabetic strings were faster than the RTs for incorrect strings in all but the

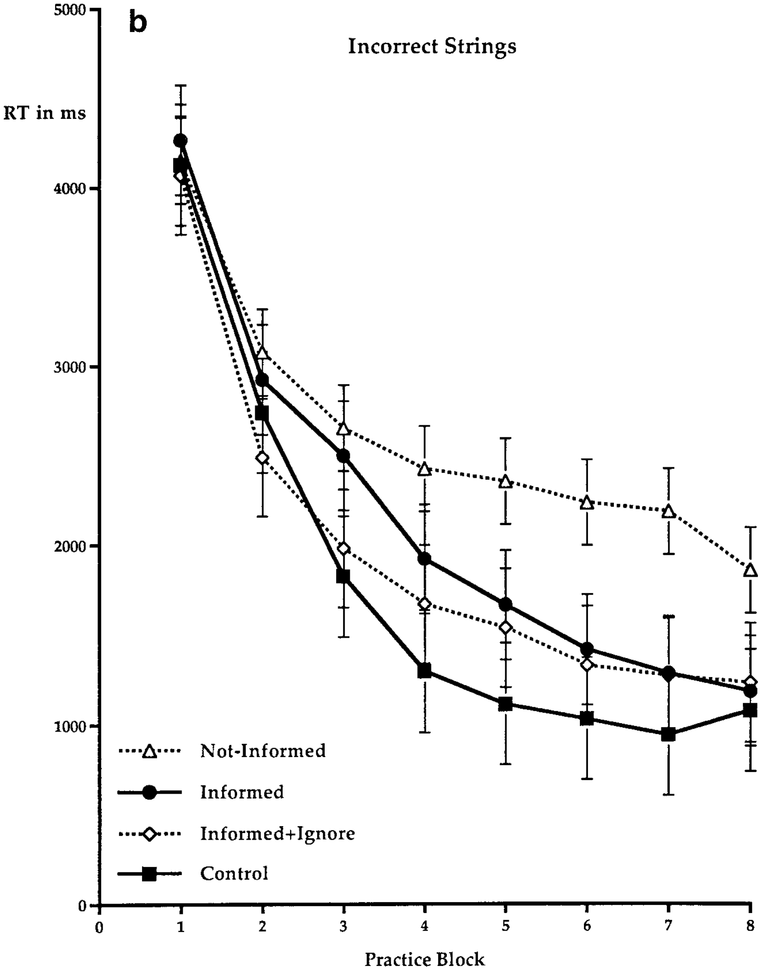

FIG. 1—*Continued*

Not-Informed condition where the opposite pattern held. Similarly, the reliable Condition × Practice Block interaction was obtained because subjects in the Not-Informed condition showed a slower decrease in their RTs across practice blocks than subjects in the remaining conditions. In fact, the Condition × Practice Block interaction was not reliable when the Not-Informed group was dropped from the analysis, $p > .5$.

These results indicate that the experimental instructions were successful. Subjects in the Informed and Informed + Ignore conditions who were told that errors could occur only in the initial triplet of the strings, responded faster than subjects in the Not-informed condition and not reliably slower than subjects in the Control condition who were presented with only the initial triplets of the strings. This indicates that subjects in the Informed and Informed + Ignore conditions were indeed able to primarily process the relevant task information. Subjects in the Not-Informed condition, in contrast, seemed to search the entire string for errors, thus taking more time to respond.

The reason why subjects in the Not-Informed condition processed incorrect alphabetic strings more quickly than correct strings was that correct strings had to be scanned in the entirety whereas processing of the incorrect strings could be terminated as soon as the error had been located in the initial ''letter–digit–letter'' triplet. For subjects in the remaining three conditions, one would not expect to see a difference between the RTs for correct and incorrect sequences because these subjects should always process only the initial string triplet. However, the subjects in these conditions processed correct strings more quickly than incorrect strings. One possible explanation for this particular result (for similar findings see Carlson, Sullivan & Schneider, 1989; Logan & Klapp, 1991) is that subjects might have set different confidence criteria for ''correct'' and ''incorrect'' responses. Specifically, when a string was found to be correct, subjects responded by pressing the ''correct'' key right away. When an error was found, however, at least some subjects might have occasionally double-checked their conclusion before pressing the ''incorrect'' key.

Notice that our interpretation of the findings so far leads to the following predictions regarding the effects of string length for correct and incorrect strings in the Not-Informed, Informed and Informed + Ignore conditions for which string length was manipulated. First, correct alphabetic strings should show a string length effect in the Not-Informed condition where subjects appear to have scanned the entire strings, but not in the two remaining conditions where subjects essentially ignored the redundant information. Second, incorrect strings should show no string length effect in any condition because the errors always occurred in the initial ''letter–digit–letter'' triplets making it thus unnecessary to scan any of the remaining letters in any of the conditions.

Correct Alphabetic Strings

In order to compare the effects of string length directly, the best fitting linear regression lines across the five string lengths were computed separately for each subject and each trial block in the experimental conditions for which string length was manipulated. Figure 2 presents the mean regression slopes for correct alphabetic strings in the Not-Informed, Informed, and Informed + Ignore conditions. Inspection of the figure shows that the slopes differed for the three experimental conditions. Essentially, there was no systematic effect of string length in the Informed and Informed + Ignore conditions where the mean slopes hovered around 0, but there was a string length effect in the Not-Informed condition. Furthermore, the effect decreased slightly with practice in the latter condition.

A 3 (condition) $\times$ 8 (practice block) mixed-design ANOVA on the individ-

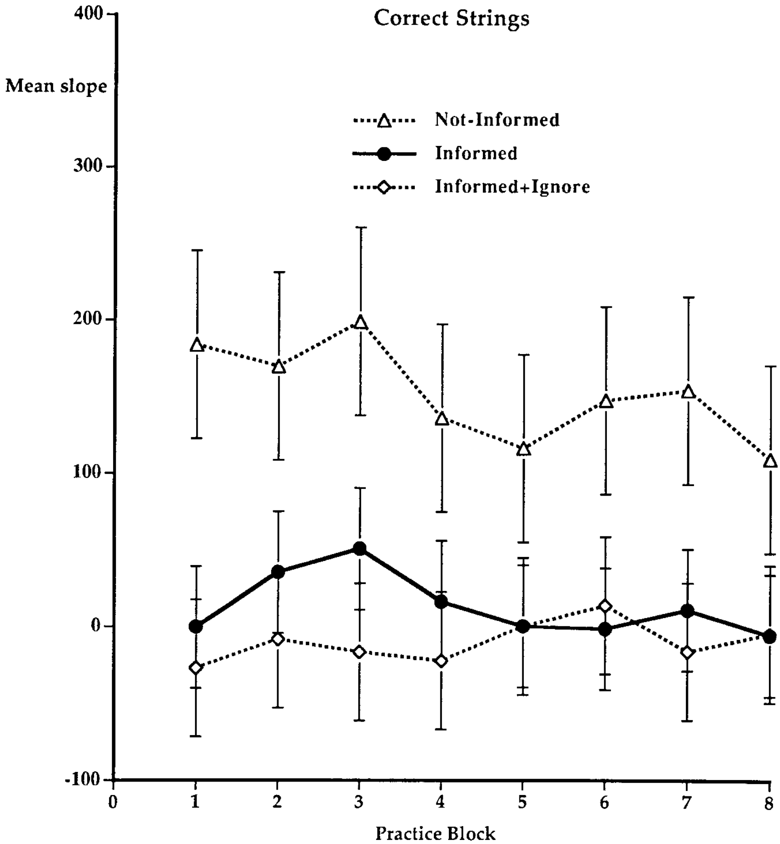

FIG. 2. Means of best fitting regression slopes for correct strings (Experiment 1). Error bars represent 95% within-subject confidence intervals.

ual linear slopes yielded reliable main effects of condition, $F(2,41) = 80.29$, $MSe = 11813.4$, $p < .001$, and practice block, $F(7,287) = 2.80$, $MSe = 9219.4$, $p < .01$, but no reliable interaction between condition and practice block, $F < 1$. These results indicate that the string length effect, as indicated by the magnitude of the best fitting regression slope, differed across experimental condition. The condition main effect was due to the difference between the Not-Informed condition, on the one hand, and the remaining conditions, on the other hand, $F(1,41) = 159.46$, $MSe = 11813.4$, $p < .01$. The latter two conditions did not differ reliably from each other, $F(1,41) = 1.73$, $MSe = 11813.4$, $p > .2$.

Incorrect Alphabetic Strings

Figure 3 contains the corresponding results for the incorrect alphabetic strings. As can be seen, none of the three experimental conditions demon-

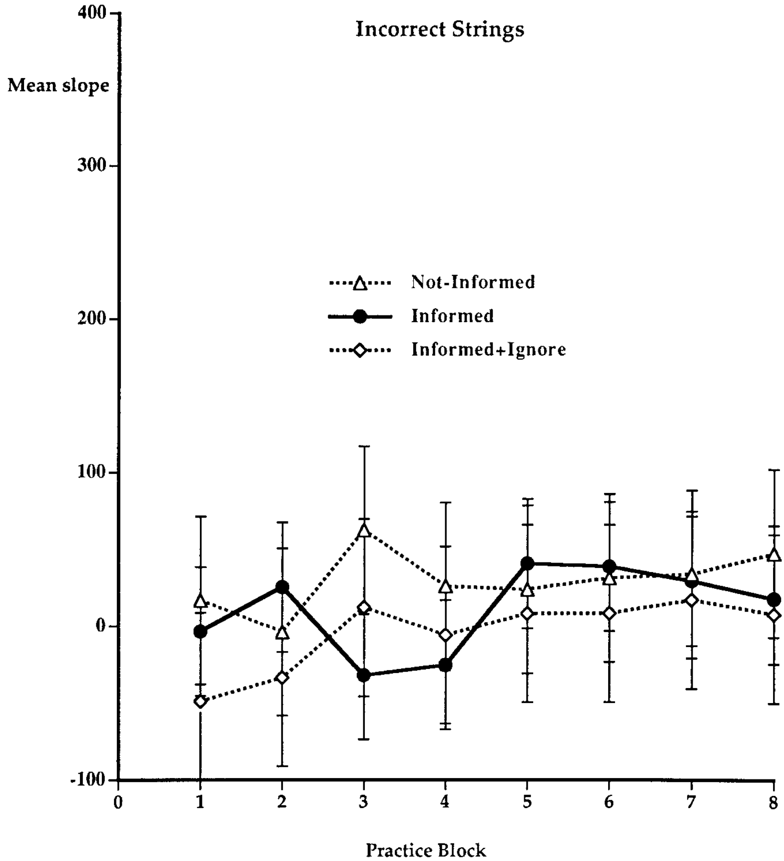

FIG. 3. Means of best fitting regression slopes for incorrect strings (Experiment 1). Error bars represent 95% within-subject confidence intervals.

strated a systematic effect of string length. All mean slopes were around zero, and did not change systematically with practice.

A 3 (condition) $\times$ 8 (practice block) mixed-design ANOVA on the best fitting regression slopes for incorrect strings yielded no reliable effects, all $ps > .08$. These results are consistent with the assumption that the subjects in all three experimental conditions essentially terminated processing of incorrect strings as soon as they discovered the error.

Discussion

The two main goals of Experiment 1 were to (a) examine if subjects could ignore redundant task information when specifically told to do so, and (b) establish an experimental procedure that would allow us to detect this effect.

The experiment accomplished both goals. First, the results suggest that subjects were able to ignore redundant information when told to do so. This claim is supported by the findings that (a) the difference in RTs between the Informed and Informed + Ignore conditions, on the one hand, and the Control condition who received only the initial ''letter–digit–letter'' triplet of each string, on the other hand, was small and not reliable, (b) subjects in the Informed, Informed + Ignore, and Control conditions were faster in responding to correct strings than to incorrect strings, whereas subjects in the Not-Informed condition showed the reversed pattern, and (c) there was no effect of string length for the correct strings in the Informed and Informed + Ignore conditions, but there was an effect of string length in the Not-Informed condition.

Second, the manipulation of string length provided us with the necessary methodological tool to examine whether or not subjects ignored the redundant information. This finding was crucial because it allowed us to test the effect of information reduction in situations where subjects were not told in advance which task information was redundant.

However, the obtained results were not perfect. First, although there was no statistically reliable difference between the Informed, Informed + Ignore, and Control conditions, inspection of Figs. 1a and 1b shows that the mean performance of the Informed and Informed + Ignore conditions was slower than the mean performance of the Control condition, at least during the second half of the trial blocks. We interpret this result as indicating that subjects were not always successful in ignoring the redundant information. Ignoring information may require sustained attention, and with increasing practice, subjects' attention might have slipped.

Second, Figs. 1a and 1b (see also Figs. 2 and 3) also show that early in practice (i.e., blocks 1–4), mean performance was faster for the Informed + Ignore condition than the Informed condition. Although there are various alternative explanations for this finding, we are most comfortable with the argument that subjects in the Informed condition might have needed some time to translate the instruction that the latter string parts were redundant into action, that is, into actively ignoring the redundant information.

In summary, Experiment 1 demonstrated that subjects are able to ignore redundant information when they know in advance that a task contains redundant information. The main goal of Experiment 2 was to go one step further by testing if subjects can also ignore redundant information when they are *not* informed about the redundant information in advance. This test was performed in two ways: First, by manipulating string length, as was done in Experiment 1, and second, by introducing, after variable amounts of practice, errors within the redundant part of the strings. If subjects indeed learn to ignore the redundant part of the string, then (a) the string length effect for correct strings should disappear with practice, and (b) error rates should increase when errors in the redundant part are introduced. Alternatively, if subjects merely learn to scan the redundant part of the strings very quickly but do not ignore the part, then their error rates should not change, although their response times might be expected to increase.

One might argue that, according to this reasoning, the string length effect that we observed in the Not-Informed condition in Experiment 1, should have decreased with practice. Although this was indeed the case (see Fig. 2), the change in slope was very small (difference of 74.5 between Trial Blocks 1 and 8). However, recall that subjects in the Not-Informed condition received five strings in each trial block that contained errors in the latter part of the string. These strings might have prevented subjects from completely ignoring the latter string parts.

EXPERIMENT 2

In Experiment 2, subjects verified the same alphabetic strings that were used in Experiment 1 for a total of either 1, 3, 5, or 7 trial blocks. Subjects were not told that errors could occur only within the ‘‘letter–digit–letter’’ triplet of the string. After the initial practice phase with a varied number of trials, subjects completed, without warning, a test block in which errors occurred in that part of the strings that was redundant up to that point, that is, the part that followed the initial ‘‘letter–digit–letter’’ triplet. If subjects learn to distinguish relevant from redundant task information and to ignore the redundant parts of the alphabetic strings, then we should find that (a) the string length effect for correct strings disappears with increasing practice, (b) subjects’ error rates increase in the test block relative to the preceding practice blocks, and (c) the string length effect for correct strings reappears as soon as subjects notice that they cannot ignore the formerly redundant string part any longer. In contrast, if subjects’ improved performance reflects their improved ability to process the redundant part very quickly, then their error rates should not increase when the test block is presented.

For subjects who received 7 practice blocks, we also manipulated feedback during the testphase. That is, half of the subjects in the 7-Practice-Blocks condition received feedback when they committed an error during the test phase (just as subjects in the remaining conditions did); the other half of the subjects did not receive feedback. The reasoning behind this manipulation was that subjects who received feedback might be alerted to the presence of errors in the part of the string that was redundant up to that point, and that this discovery might minimize the effect we intended. Manipulating feedback thus allowed us to compare subjects who were likely alerted to (7-Practice-Blocks/Feedback) and not alerted to (7-Practice-Blocks/No Feedback) the fact that the location of errors was now distributed across the alphabetic strings.

Method

Subjects

Subjects were 75 female and 32 male undergraduate students at the University of Missouri at Columbia who received course credit in introductory psychology for participating in the experiment. Subjects ranged in age from 18 to 45 years (M = 18.9, SD = 3.95). Because of technical problems, the data from 1 subject were lost.

Materials

Stimulus and apparatus. Subjects verified either 1, 3, 5, or 7 trial blocks using the same alphabetic strings that subjects in the Not-Informed condition in Experiment 1 had evaluated. In each trial block, subjects received 50 correct and 50 incorrect strings. All subjects received one additional test block that contained 50 correct strings, 30 incorrect strings with errors in the initial string triplet, and 20 incorrect strings with errors in the formerly redundant string part. The position of the error in the redundant part was counterbalanced. The presentation of stimuli followed the same format as described for Experiment 1.

Procedure

Subjects were randomly assigned to one of the five experimental conditions, and were tested in groups of up to 10 people. As in Experiment 1, the experimental session began with a computerized instruction and the same training session as used in Experiment 1. The training session was repeated if subjects made more than 30% errors. After the training session, all subjects were told to pay close attention to the entire string because errors could occur anywhere in the string. Subjects then evaluated either 1, 3, 5, or 7 blocks of trials. The additional test block with errors in the redundant string parts followed the last practice block without announcement.

The presentation of strings followed the format described for Experiment 1. During the training and practice phases, subjects received feedback (i.e., error prompt and high-frequency tone) when they made an error as was done in Experiment 1. For the test trials, all subjects, except those in the 7-Practice-Blocks/No Feedback condition, received feedback when they made an error. The entire experiment lasted between 20 and 90 min, depending on experimental condition.

Design

The dependent variables were the median RT per trial block and mean error rate per block. The only between-subjects factor was experimental condition (1-Practice-Block, 3-Practice-Blocks, 5-Practice-Blocks, 7-Practice-Blocks/Feedback, 7-Practice-Blocks/No Feedback). Within-subjects factors were error type (Initial Triplet vs Later String Part) and string length (3 through 7).

Results

First, mean error rates were computed for each subject in each practice block, as was done in Experiment 1. Subjects were excluded from all reported data analyses if their mean error rate was higher than 10% in each practice block (N = 6). This resulted in 24 remaining subjects in the 1-Practice-Block condition, 19 subjects each in the 3-Practice-Blocks condition, the 5-Practice-Blocks condition, and the 7-Practice-Blocks/Feedback condition, and 20 subjects in the 7-Practice-Blocks/No Feedback condition.

Mean error rates per trial block ranged from 4.49% to 6.10% across the five experimental conditions. There was no speed–accuracy trade-off; the correlation between response time and error rate was $r(546) = -.05$. (For the computation of the correlation, strings with errors in the redundant part of the string were excluded.)

Our discussion of the results from Experiment 1 is again divided into three main sections. First, we discuss the effects of string length on subjects' RTs for correct and incorrect strings during the practice phase. Next, we describe

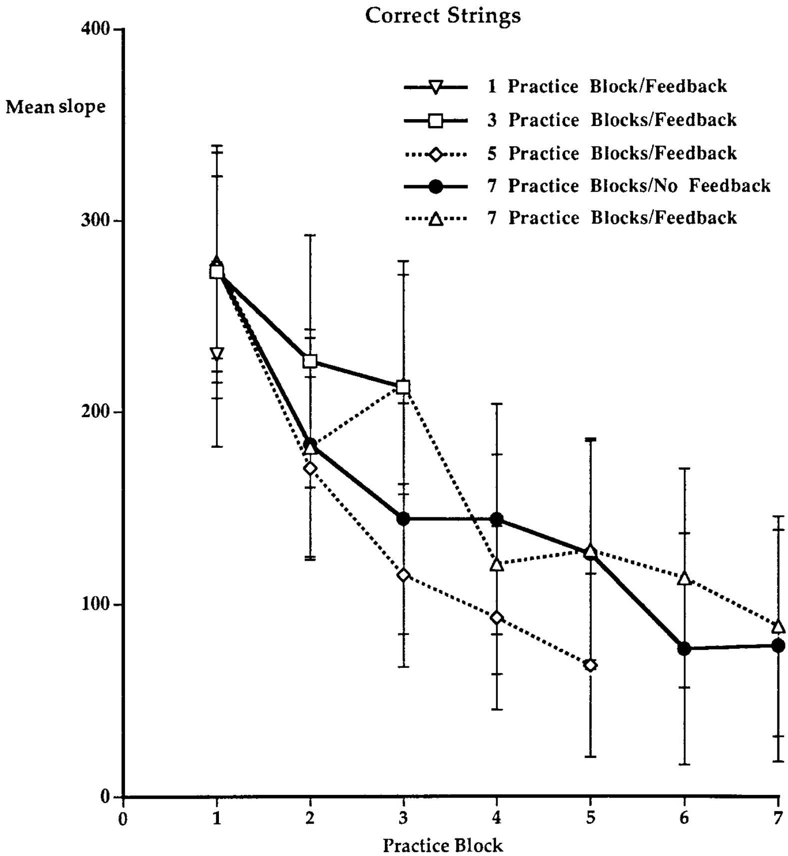

FIG. 4. Means of best fitting regression slopes for correct strings (Experiment 2). Error bars represent 95% within-subject confidence intervals.

the effects of introducing errors in the redundant parts of the strings on subjects' error rates, and, in the third section, on subjects' RTs.

String Length Effect in Practice Trials: Correct Alphabetic Strings

If redundant information can indeed be ignored when subjects are not told in advance which information is redundant, then we should see a string length effect for correct strings that should decrease with practice. We should not see a systematic string length effect for incorrect strings. Figure 4 contains the mean slopes of the best fitting regression lines over the 5 string lengths for correct alphabetic strings in all experimental conditions. Notice that the string length effect, as measured by the magnitude of the slopes, did indeed decrease systematically between the first and last practice blocks, namely by 60.28 in the 3-Practice-Blocks condition, 207.77 in the 5-Practice-Blocks condition, 190.06 in the 7-Practice-Blocks/Feedback condition, and 197.39 in the 7-Practice-Blocks/No Feedback condition.

A 4 (condition: 3-Practice-Blocks vs 5-Practice-Blocks vs 7-Practice-Blocks/Feedback vs 7-Practice-Blocks/No Feedback) $\times$ 2 (practice block: first vs last) ANOVA on the regression slopes from the first and last trial blocks only indicated a reliable main effect of practice block, $F(1,73) = 59.55$, $MSe = 21027.36$, $p < .001$. The main effect of condition was not reliable, $p > .05$, and the interaction between condition and practice blocks just missed significance, $F(3,73) = 2.10$, $MSe = 21027.36$, $p < .11$.

Follow-up one-way ANOVAs with Practice Block as independent variable yielded reliable main effects of practice block in all conditions except the 3-Practice-Blocks condition ($p > .10$), $F(4,72) = 11.94$, $MSe = 10998.86$, $F(6,108) = 7.08$, $MSe = 16159.78$, and $F(6,114) = 6.58$, $MSe = 18785.75$, all ps $< .01$, for the 5-Practice-Blocks, the 7-Practice-Blocks/Feedback, and the 7-Practice-Blocks/No Feedback condition, respectively. These results indicate that the string length effect decreased systematically over trials in the conditions that received more than three practice blocks, and are consistent with the assumption that the redundant part of the strings was increasingly ignored by subjects. The results do not yet rule out faster scanning of redundant information, however.

String Length Effect in Practice Trials: Incorrect Alphabetic Strings

Figure 5 contains the corresponding results for the incorrect alphabetic strings. As can be seen, the mean slopes changed much less over the course of practice than did the slopes for the correct strings depicted in Figure 4.

A 4 (condition: 3-Practice-Blocks vs 5-Practice-Blocks vs 7-Practice-Blocks/Feedback vs 7-Practice-Blocks/No Feedback) $\times$ 2 (practice block: first vs last) ANOVA on the slopes from the first and last trial blocks indicated a reliable main effect of practice block, $\mathrm{F}(1, 73) = 13.14$, $MSe = 15240.45$ $p < .05$. The main effect of condition and the interaction were not reliable, both Fs < 1. Although the main effect of practice block was reliable, the effect was rather small, compared to the effect obtained with correct strings (see Fig. 4).

Effects of Introducing Errors in the Redundant String Parts: Error Rates

Figure 6 displays the mean error rates for incorrect strings in the last practice block and the test block. For the last practice block, error rates are for incorrect strings with errors in the initial ‘‘letter–digit–letter’’ string triplet. For the test block, error rates are for incorrect strings in which the errors occurred in the initial triplet (‘‘Test Block/Triplets’’ in Fig. 6), and incorrect strings in which the errors occurred in one of the letters following the ‘‘letter–digit–letter’’ triplet (‘‘Test Block/Posttriplet Letters’’ in Fig. 6). As can be seen, the mean error rate for incorrect strings in which the error was located in the initial triplet did not change much from the last practice

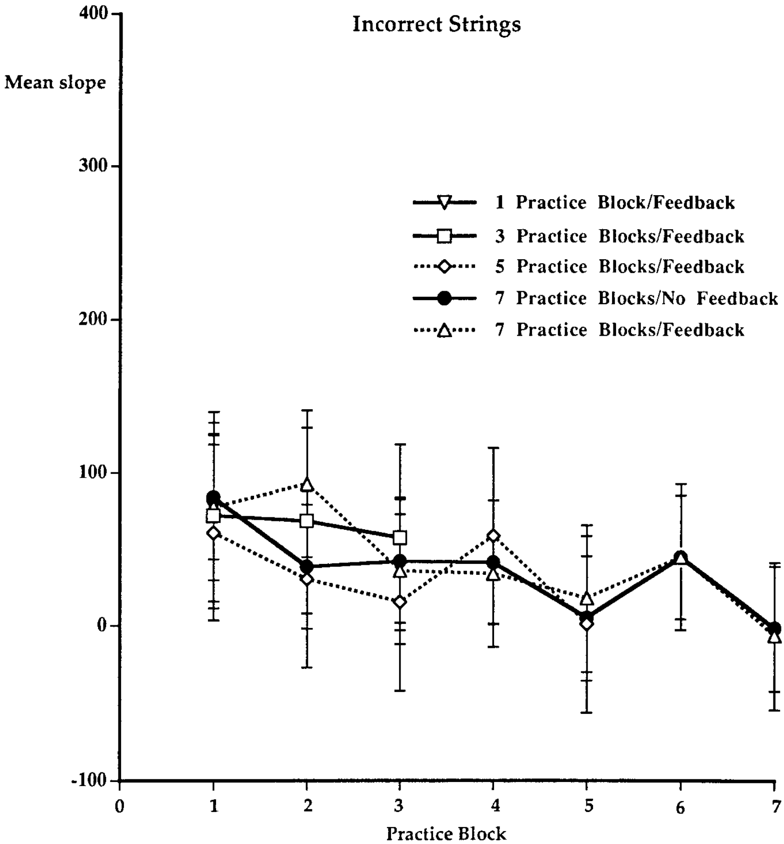

FIG. 5. Means of best fitting regression slopes for incorrect strings (Experiment 2). Error bars represent 95% within-subject confidence intervals.

block to the test block in any of the conditions. However, these two error rates differed markedly from the mean error rate for incorrect strings in which the errors occurred in the formerly redundant string part. In addition, the latter mean error rate increased systematically with the amount of practice that subjects received, and furthermore, was affected by the feedback manipulation in the 7-Practice-Blocks conditions as well. The mean error rate was highest for subjects who practiced for 7 blocks of trials and did not receive feedback when they made errors in the test block.

The error data were entered into a 5 (experimental condition) $\times$ 3 (type of error: error in triplet for last practice block vs. error in triplet for test block vs. error in redundant part for test block) mixed-design ANOVA. This analysis produced reliable main effects of condition, $F(4,94) = 6.59$, $MSe = 385.09$, $p < .001$, and of type of error, $F(2,188) = 82.22$, $MSe = 157.21$, $p < .001$,

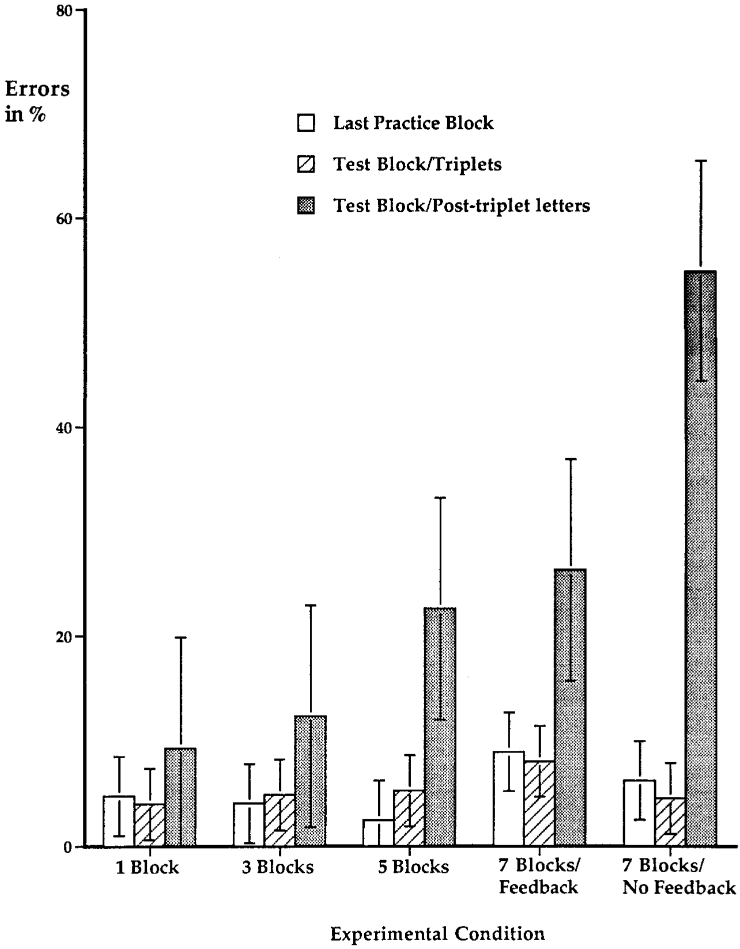

FIG. 6. Mean error rates in last practice block and test block (Experiment 2). Error bars represent 95% between-subject confidence intervals.

as well as a reliable interaction between the two factors, $F(8,188) = 13.58$, $MSe = 157.21$, $p < .001$. As can be seen in Fig. 6, the interaction between condition and type of error was primarily due to the fact that the difference between the mean error rate for incorrect strings with errors in the redundant part, on the one hand, and the mean error rates for the other two error types, on the other hand, increased with practice. Put differently, only the error rate for strings with errors in the redundant part of the strings changed systematically with practice; the remaining two error rates stayed relatively constant across levels of task practice. For the latter, neither the main effect of condition nor the interaction between condition and type of error was reliable, both *p*s

$> .05$. For the former, the main effect of condition was reliable, $F(4,94) = 11.67$, $MSe = 569.6$, $p < .01$.

Also noteworthy is the finding that the feedback manipulation for subjects who received 7 trial blocks of practice had quite impressive effects. The mean error rate for incorrect strings with errors in the redundant parts was roughly twice as high in the condition where subjects did not receive feedback ($M = 55\%$), relative to the condition where subjects did receive feedback ($M = 26.32\%$). This finding is consistent with the argument that subjects learned to ignore redundant information but returned to processing the redundant information when they were told (by way of feedback) that the formerly redundant information had become relevant for solving the task at hand.

Effects of Introducing Errors in the Redundant String Parts: Response Times for Correct Strings

The argument that subjects returned to processing the redundant information once they discovered that the formerly redundant information was now relevant, leads to the prediction that the string length effect for correct strings that, as seen above (c.f. Fig. 4), decreased with practice, should again increase for subjects who noticed that the formerly redundant information had now become relevant. We therefore expected that the string length effect would be larger in the test block than in the last practice block for subjects who noticed that errors occurred in the redundant string part. At a group level, the change in the string length effect should therefore increase with amount of practice. That is, conditions with much practice should show a larger change than conditions with little practice. The exception should be the 7-Practice-Blocks/No Feedback condition. Here, subjects generally did not notice that errors occurred in the redundant string part, as is evidenced by the very high mean error rate in this condition. Therefore, there was little reason to expect that these subjects would change their task processing.

Fig. 7 displays the mean slopes for correct alphabetic strings in the last practice block and the test block. It is apparent that the string length effect indeed increased more for conditions with much task practice than for conditions with little practice. The changes in slope from the last practice block to the test block were 26.20, 51.14, 123.61, 127.73, and 46.5 in the 1-Practice-Block, the 3-Practice-Blocks, the 5-Practice-Blocks, the 7-Practice-Blocks/Feedback, and the 7-Practice-Blocks/No Feedback condition, respectively. These values are generally consistent with the reasoning above.

The data were examined by a 5 (condition) $\times$ 2 (practice block: last practice block vs. test block) mixed-design ANOVA on the individual regression slopes. The analysis indicated reliable main effects of condition, $F(4,94) = 8.49$, $MSe = 17780.3$, $p < .001$, and of practice block, $F(1,94) = 32.21$, $MSe = 12003.1$, $p < .001$, as well as a marginally reliable interaction between condition and practice block, $F(4,94) = 2.29$, $MSe = 12003.1$, $p < .07$.

Separate follow-up ANOVAs for the experimental conditions indicated that

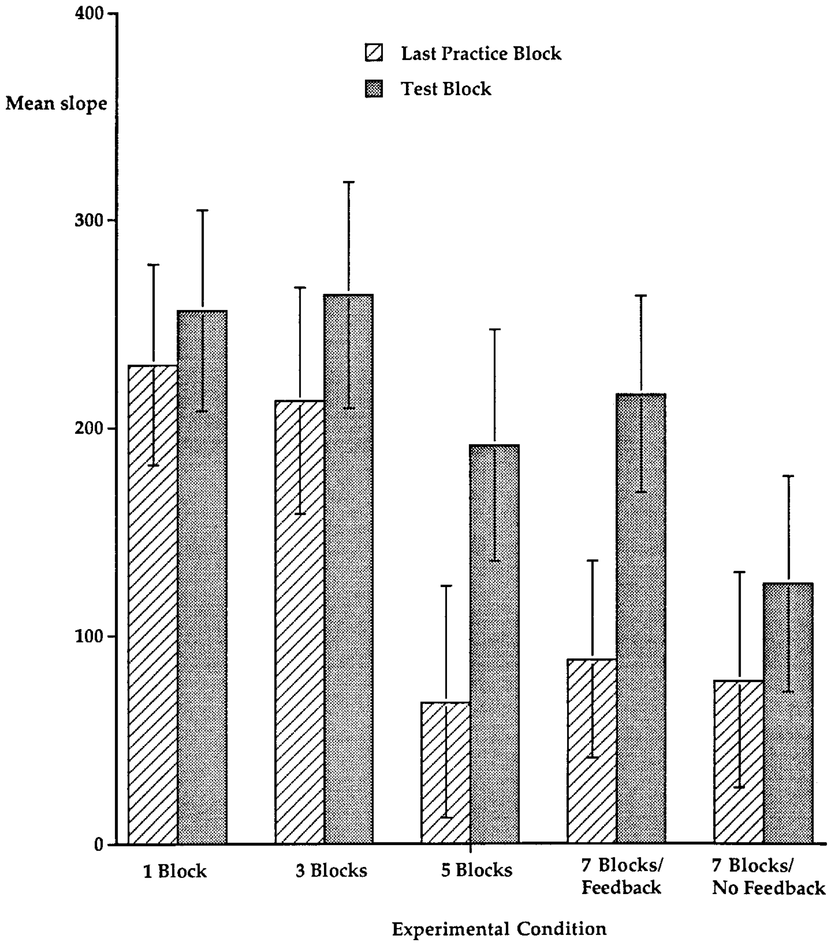

FIG. 7. Means of best fitting regression slopes for correct strings (last practice block and test block; Experiment 2). Error bars represent 95% within-subject confidence intervals.

the slopes changed reliably from the last practice block to the test block in the 5-Practice-Blocks condition, $F(1,18) = 13.45$, $MSe = 13460.98$, $p < .01$, and in the 7-Practice-Blocks/Feedback condition, $F(1,18) = 21.48$, $MSe = 9671.04$, $p < .01$, but not in the 1-Practice-Block, the 3-Practice-Block, and the 7-Practice-Blocks/No Feedback conditions, all ps $> .05$.

Discussion

The results of Experiment 2 demonstrate that redundant information can be ignored even when subjects are not told in advance that the task includes redundant information. This argument is based on three main findings: (a) the string length effect (slope) for correct strings decreased with practice, (b) error rates for incorrect strings with errors in the redundant part were much higher than error rates for incorrect strings with errors in the relevant part, and varied systematically with amount of practice, and (c) the string length effect reappeared for subjects who noticed that the location of errors within the strings had switched to now include the formerly redundant part. Taken together, the results of Experiment 2 support the contention that redundant information can be ignored and that the reduction in the amount of information that is processed may be one reason why subjects get faster with task practice. The alternative explanation, namely that subjects learn to process the redundant information very quickly, is not supported by our findings. If this were the case, subjects should have been able to detect the errors in the redundant part of the strings that were introduced in the test block.

Knowing that the phenomenon exists does not, of course, tell us anything about the mechanisms that accomplish it. Experiment 3 was designed to take a first look at some of the properties of the underlying learning mechanism(s). There are at least two distinct possibilities as to how the mechanism(s) might operate. First, proponents of an instance-based view of skill acquisition (e.g., Logan, 1988) might argue that subjects initially process the strings letter by letter but also store the processed strings as instances in long-term memory. During subsequent trials, a race between algorithm based letter-by-letter processing and the retrieval of stored instances occurs that should increasingly favor the retrieval process over the algorithm. If one adds the assumptions that the matching of retrieved instances and encountered strings is a parallel process and that instances are flagged as belonging to either the correct or incorrect response category, then this model would seem to account for most of the data presented so far. For instance, it would explain why the initial string length effect would disappear with practice, that is, as more instances become available. In essence, the instance-based view holds that redundant information is not ignored. Rather, the appearance of such an effect is due to the increased availability and parallel matching of retrieved instances.

On the other hand, one might also argue that subjects simply stop processing redundant task information altogether. This might be the result of a deliberate decision on the part of the subjects, or it might be due to the operation of an as of yet unspecified automatic learning mechanism that is capable of distinguishing relevant from redundant task information. The purpose of Experiment 3 was to examine whether the apparent reduction in the amount of information processed by our subjects was stimulus-specific or general. That is, we tested whether the effect was specific to the materials processed, or whether the effect was general such that subjects stop processing a structural component of a task regardless of the specific information that is contained in the component.

Experiment 3 was a transfer study in which subjects received, after an initial practice session with typical strings, new strings that contained elements that were different from those used in the original strings. If information reduction is stimulus-specific, then we should observe no transfer between the two types of strings. If, on the other hand, the effect is a general one, then information reduction should be observed with the new elements as well.

EXPERIMENT 3

For the purpose of Experiment 3, the alphabetic strings used in Experiments 1 and 2 were divided into two subgroups of strings, letter set D–H and letter set I–M. Subjects practiced with one letter set for 4 trial blocks and were then transferred to the other letter set for an additional 4 blocks of trials. If the ability to ignore redundant information is stimulus-specific, then there should be no transfer from one letter set to the other. Specifically, one would expect to find that the string length effect that decreases over the initial 4 practice blocks would increase as letter set is changed. If, on the other hand, the mechanism underlying information reduction is a general one, then we should find complete transfer, i.e., no increase in the string length effect as letter sets are changed.

Method

Subjects

Subjects were 33 female and 21 male undergraduate students at the University of Missouri at Columbia. Subjects ranged in age from 18 to 30 years (M = 18.7, SD = 1.85). All subjects received course credit in introductory psychology for participating in the experiment. Because of technical problems, data from 3 subjects were lost.

Materials

Stimulus and apparatus. The correct and incorrect alphabetic strings were constructed in the same manner as described for Experiments 1 and 2. The strings were divided into 2 subsets, strings beginning with the letters D, E, F, G, or H, and strings beginning with letters I, J, K, L, or M. String length varied again from 3 to 7. For each letter set, there were thus 25 correct and 25 incorrect alphabetic strings. Subjects evaluated each of the resulting 50 strings twice in each trial block. Thus, each of the 50 strings was repeated 8 times over the four initial trial blocks.

Procedure

Subjects were randomly assigned to one of the two letter sets. Instructions and the initial training session followed the format described for Experiment 1. As in the previous experiments, there were 8 blocks of trials. Blocks 1 through 4 were performed with one letter set; blocks 5 through 8 were performed with the other letter set. Half of the subjects began with letter set D–H; the other half began with letter set I–M. Subjects were not alerted to the change in letter set. The presentation of the strings and the feedback followed the format described for Experiments 1 and 2. The entire experiment lasted between 60 and 90 min.

Design

Dependent variables were again the median RT per trial block and the mean error rate per trial block. Independent variables were letter set (D–H vs I–M; within-subjects), practice block (1 through 4; within-subjects), and string length (1 through 5; within-subjects).

Results

As in the previous experiments, we first computed the mean error rate for each subject in each practice block. In Experiment 3, no subject recorded error rates that were higher than 10% in each trial block; thus, data from all subjects were entered in the data analysis. Again, there was no speed–accuracy trade-off. The error rates ranged from 4.49% to 6.10%; the correlation between error rates and latencies was $r(408) = .02$.

Overall Results

Figure 8 shows the mean RTs over the eight practice blocks, separately for the group that began with letter set D–H (solid line) and the group that began with letter set I–M (dashed line). As can be seen, RTs declined substantially with practice in both groups, and there was a visible effect of letter change. That is, RTs increased by about 350 ms in both groups from the last practice block with the initial letter set (Block 4) to the first block with the changed letter set (Block 5).

A 2 (initial letter set) $\times$ 8 (practice block) ANOVA on the RTs yielded a reliable main effect of practice block, $F(7,343) = 139.09$, $MSe = 373335.4$, $p < .01$, and a reliable interaction between initial letter set and practice block, $F(7,343) = 4.40$, $MSe = 373335.4$, $p < .01$. The reliable interaction appears to be due to subjects' faster responding to letter set D–H than to letter set I–M, and likely reflects a familiarity difference between the two letter sets.

An additional analysis, confined to the comparison of the RTs from Blocks 4 and 5 only, indicated a reliable main effect of block, $F(1,49) = 23.09$, $MSe = 141198.6$, $p < .01$, but no reliable main effect of initial letter set, and no reliable interaction, both ps $> .05$.

These analyses, in conjunction with Fig. 8, indicate that the introduction of a new letter set did have an effect. Subjects clearly needed extra time to adjust to the change in elements. However, of more importance is the question of whether this was an effect that generally slowed task processing, or a specific effect that made it impossible for subjects to rely on stored instances any longer and forced them to return to their initial letter-by-letter processing of the entire strings. Below, we therefore present analyses of the string length effect. As in the previous experiments, we expected to find a gradual decrease of the string length effect over the four initial practice blocks. If the decline in the string length effect is due to retrieval of an increasing number of instances, then we expect the string length effect to return to its Block-1 level when transfer strings are introduced. If, however, the declining string length effect is due to subjects' ignoring the redundant information, then the change in letter set should not lead to a reappearance of the string length effect.

Correct Alphabetic Strings

Figure 9 displays the means of the best fitting regression slopes for correct strings of length 3 to 7 over the eight practice blocks. Because the two letter

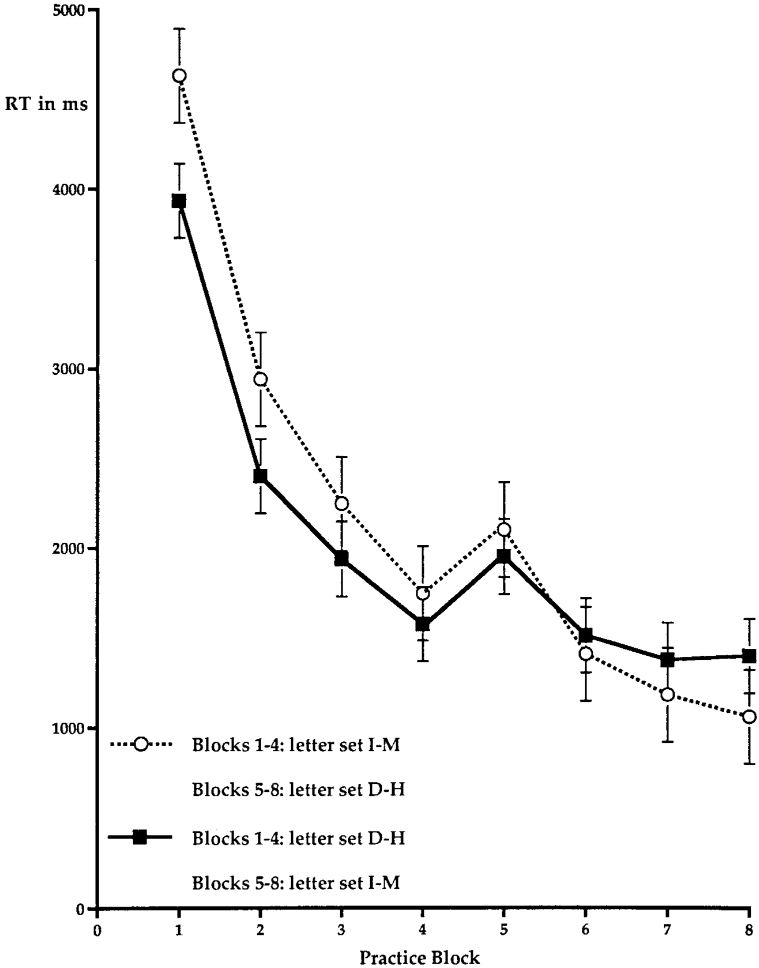

FIG. 8. Mean RTs for two letter sets, D–H and I–M (Experiment 3). Error bars represent 95% within-subject confidence intervals.

set conditions showed very similar effects, we pooled the data from the two groups. As can be seen in the figure, the mean slope declined over the initial four practice blocks and changed only very slightly when the new letter set was introduced. More specifically, the slopes were 284.14 in Block 1, 65.73 in Block 4 and 69.50 in Block 5. Thus, although the introduction of the new letter set did affect overall performance, its effect on the string length effect was very small.

A repeated-measures ANOVA on the individual regression slopes from the initial practice phase (i.e., practice blocks 1–4) yielded a reliable main

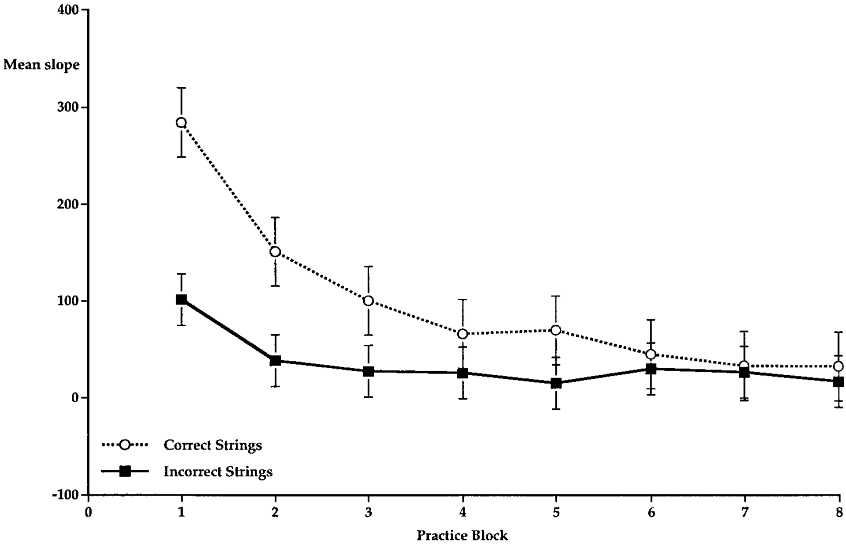

FIG. 9. Means of best fitting regression slopes for correct and incorrect strings (Experiment 3). Error bars represent 95% within-subject confidence intervals.

effect of practice block, $F(3,150) = 31.18$, $MSe = 16756.54$, $p < .01$, confirming the impressions conveyed by Fig. 9. More importantly, the comparison of the mean slopes from practice blocks 4 and 5 only did not yield a reliable difference, $F < 1$. This result suggests that the ability to ignore redundant task information is relatively independent of the specific stimulus material practiced.

Incorrect Alphabetic Strings

Figure 9 also displays the corresponding mean slopes for incorrect letter strings. The figure shows (a) little change in the string length effect with practice, a finding consistent with the results from Experiments 1 and 2, and (b) little change in the string length effect when the new letter set was introduced. Rather, the slope remained relatively flat (101.51 in Block 1, 25.45 in Block 4, and 14.99 in Block 5).

A repeated-measures ANOVA on the slopes from the initial practice phase yielded a reliable main effect of practice block, $F(3,150) = 7.01$, $MSe = 9446.94$, $p < .01$, that was, however, much smaller than the one for correct strings and was due to the change from Block 1 to Block 2. More importantly again, a comparison of the slopes from practice blocks 4 and 5 only did not yield a reliable difference, $F < 1$.

Discussion

The results of Experiment 3 strongly suggest that the mechanism by which information is reduced, or put differently, by which redundant information is ignored, is relatively independent of the stimulus material practiced. The string length effect, as measured by the best fitting linear slopes across the 5 string lengths was only slightly affected by the letter set change (compare Fig. 9). On the other hand, however, there was a reliable increase in overall RTs when letter set was changed (compare Fig. 8). Although this overall increase might well have been due to surprise or confusion on the part of subjects, it may also be indicative of a stimulus-specific component of the mechanism underlying information reduction. Overall, thus, we may cautiously conclude that the mechanism underlying information reduction is largely stimulus independent, and may be activated through deliberate, intentional actions on the part of the subject, or, alternatively, might perhaps be based on some form of implicit procedural learning.

GENERAL DISCUSSION

The main results from the experiments described in this article are threefold: First, subjects appear to be able to distinguish relevant from redundant task information, and to limit their processing to the relevant information regardless of whether they are informed about the task containing redundant information or not. Second, the ability to reduce the amount of information that is processed takes time, and develops gradually over the course of practice. And third, the mechanism underlying this ability is largely stimulus-independent in the sense that structural components of a task are ignored rather than specific task materials. Taken together, the findings are consistent with the argument that one reason why practice leads to an increase in the speed with which a task is processed may be people's ability to limit their processing to the task-relevant information.

As we discussed in the introduction to this article, these findings are also consistent with results reported in the expert–novice literature (e.g., Christensen *et al.,* 1981; Myles-Worsley *et al.,* 1988; Shanteau, 1992), and in the literature on concept development and categorization (e.g., Regehr & Brooks, 1993). In the expert-novice literature, it has been demonstrated that experts identify different aspects of a problem as important than do novices. Several researchers have hypothesized that experts might have acquired an internal evaluation function that evaluates incoming information in terms of their relevance for obtaining domain-specific goals (e.g., Holding, 1985; Hunt, 1991; Larkin, 1983; Larkin, McDermott, Simon, & Simon, 1980). Furthermore, Gaeth and Shanteau (1984) and Shapiro and Raymond (1989) showed that when novices are instructed to focus on the same aspects of a task that experts seem to concentrate on, then the novices' performances improve. Shapiro and Raymond, for instance, were able to show that training novices in the use of a set of oculomotor behaviors that had been found with experts, improved novices' performance on the video game Space Fortress. In this study, novices were trained to (1) minimize eye movements by eliminating repetitive saccades to stimuli previously processed and/or previously found to be redundant, and were encouraged to (2) use peripheral vision thereby eliminating saccades to objects able to be analyzed sufficiently with the peripheral visual field (Shapiro & Raymond, 1989, p. 236).

People's ability to distinguish between relevant and redundant aspects may be even more obvious in concept development, where subjects must learn to select or to abstract some dimensions of the stimulus while ignoring others (e.g., Regehr & Brooks, 1993). Nosofsky (1984, 1986), for example, has shown that dimensions that are more useful for the distinction among various related concepts receive greater attention than dimensions that are redundant for correct classification.

The results of the experiments reported here suggest that the amount of information that is processed may indeed change over practice. One may ask, of course, to what extent the phenomenon of information reduction might be accommodated by recent models of skill acquisition, such as Gordon Logan's (1988) Instance Theory and John Anderson's ACT* (1983) or ACT-R (1993), or whether the phenomenon requires the introduction of additional mechanisms. Some comments on the relation between the findings presented here and existing models of skill acquisition are therefore in order.

Information Reduction and Instance-Based Learning

As mentioned earlier, proponents of an instance-based view of skill acquisition might argue that information reduction is really an epiphenomenon because the effects demonstrated here might be due to the increased availability of stored instances. This view would indeed appear to be consistent with most of the findings reported for Experiments 1 and 2. However, it would not be consistent with the results of Experiment 3, nor would it be consistent with the finding of an increased error rate when errors were placed in the redundant parts of the string (Experiment 2). Matching of earlier stored instances should not be possible any longer when the newly presented strings differ from those presented earlier as was the case for the test block of Experiment 2 and the transfer phase of Experiment 3. Even a somewhat less rigorous matching assumption that would allow for the matching of similar, rather than identical, stored traces with newly encountered strings could not account for the findings of Experiment 3.

Logan (1988) has argued that learning is a side-effect of attention, that is, that instances stored in memory contain only those aspects of a task situation that are attended (obligatory encoding assumption). Data published recently by Lassaline and Logan (1993) and Logan and Etherton (1994) are generally consistent with this argument. Thus, stored instances do not appear to contain all the information that is contained in the environment, but rather only a subset of the available information. One could argue, therefore, that subjects in our experiments stored only parts of the strings, say, the initial triplets, as instances in memory, because this part received most of the attention. If one assumes, further, that, with practice, subjects increasingly rely on the retrieved instances of the triplets, then one may indeed have a possible explanation for the findings of Experiments 1 and 2. However, this explanation would still not apply to the data from Experiment 3 because the string triplets presented in Blocks 1–4 were different from those presented in Blocks 5–8, and yet, no increase in the string length effect was observed.

An instance-based view of information reduction has additional problems. For instance, which criteria are applied by subjects to decide when and how to split environmental information into different pieces, that is, to attend to some information and not to other information? Also, it is not clear whether the process of reducing information is necessarily under subjects' conscious control or is a process that could conceivably operate outside of subjects' awareness. In essence, thus, any instance-based view of information reduction would appear to require the assumption of additional mechanism(s) responsible for distinguishing relevant from redundant task information, and for ensuring that relevant and redundant information is processed differently.

Information Reduction and Process-Based Learning

In process-based accounts of skill acquisition, (e.g., Anderson, 1987, 1992), it is typically assumed that practice affects task performance by speeding up (a) the access to information stored in long-term memory, (b) the processing of task information, or (c) both. According to Anderson's ACT* model, for example, knowledge about how to perform a task is initially stored in a declarative knowledge base. This knowledge is, relatively early in practice, transferred to a procedural knowledge base. With even more practice, the resulting procedures are strengthened such that they can be accessed more quickly in long-term memory, and are composed. Composition is a process by which consecutively performed procedures are merged into a larger new procedure that contains fewer steps and can thus be performed more quickly than the original procedures.

Although some process-based theories of skill acquisition allow for the possibility that the processing of a task changes with practice, the modifications in task processing that these models can account for are typically limited. For instance, composition essentially eliminates subgoals that need to be met initially, but does not change the qualitative nature of task processing. Other proposed mechanisms, such as chunking (Newell & Rosenbloom, 1981), generalization, or discrimination (Anderson, 1983) might go a little further, but none of the mechanisms proposed thus far seems capable of changing task processing such that redundant information is completely ignored. Thus, process-based models, just as instance-based models, appear to require additional mechanisms to account for the data presented here.

In summary, none of the existing theories of skill acquisition seems capable of entirely accounting, at present, for the practice effects described in this article. The findings presented here demonstrate that people are capable, given sufficient practice, of distinguishing relevant from redundant task information, and of limiting processing to the relevant information. At the present time, we assume that the mechanisms by which relevant and redundant information are distinguished and task processing is limited, are under conscious and deliberate control. In theory at least, this assumption ought to empirically testable because it predicts discontinuities in the decrease of latencies during skill acquisition, as have been found by Logan and Klapp (1991), for instance, for alphabetic arithmetic tasks with addend 5 (see also Haider & Kluwe, 1994). Mechanisms that operate outside of conscious awareness may simply not have the power to produce the effects described here.

Finally, a word of caution. Task information can only be safely ignored when it is redundant. Otherwise, information reduction leads necessarily to incorrect task solutions. Clearly, many tasks in our daily lives, such as pure mental arithmetic tasks (Frensch & Geary, 1993), for instance, do not contain redundant information. Thus, any effects of practice that we observe with these tasks cannot be due to information reduction. We suspect, however, that the majority of skills that people acquire contain redundant information, albeit to varying degrees. As a case in point consider air traffic controllers and pilots who, in order to reasonably cope with an abundance of incoming information, are forced to reduce the information to a manageable amount. Indeed, faulty information reduction might be one of the factors contributing to accidents. For this reason alone, further experimental studies on information reduction seem warranted.

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