CHAPTER TEN

Experimental Research on Complex Problem Solving

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INTRODUCTION

In the European tradition, experimental research on Complex Problem Solving (henceforth CPS) is relatively new; indeed, it was not the preferred mode of studying CPS when this research domain was introduced during the mid-1970s. This statement may sound somewhat surprising given that one of the most cited early German studies on CPS, the LOHHAUSEN study (Dörner, Kreuzig, Reither, & Stäudel, 1983) in which subjects were asked to perform the duties of the mayor of a small simulated city, was an experimental study in which a treatment factor (i.e., training schedule) was effectively manipulated. That this study was indeed an experimental study has often been overlooked because the experimental results were by far not as impressive as the low correlations between test intelligence and CPS scores that were reported by the authors (see, e.g., Dörner, 1980).

The experimental treatment in the Dörner et al. study consisted of two different types of training, (a) training where global information about strategic procedures was available to subjects, and (b), training where more concrete hints on strategic and tactical issues were given. Performance in the two training groups was compared to performance in a control group. The results demonstrated that although subjects in the treatment conditions judged the training sessions as helpful, the three groups did not differ on the dependent variables that captured the quality of system's control.

Soon after the new research domain CPS had been established, a theoretical discussion concerning the experimental approach to studying CPS began that has not ended to this date. Dörner (1989), on the one hand, pointed out that classical experimental methods, and especially tools like analysis of variance, are not useful when one wishes to understand the complex behavior of people operating on complex systems. Funke (1984), in contrast, has argued that experimental research within CPS is not a contradiction in terms.

In the following, I am concerned with how the experimental method can be, and has been, fruitfully employed to help us understand CPS. As previously stated (Frensch & Funke, this volume), CPS, at least in the European tradition, deals with problem solving of tasks that are novel, dynamic, complex, and intransparent. This discussion, therefore, is limited to experimental research utilizing tasks, mostly computerized, that meet these criteria. I do not discuss static tasks, for example.

In the first section, I summarize and discuss the pros and cons of the experimental approach to studying CPS. In the second section, I present a taxonomic scheme that categorizes most of the experimental work that has been performed to date, and discuss some experimental studies in detail to illustrate what has been achieved so far. In the final section, I draw conclusion regarding why, when, and how to conduct experimental studies in CPS research.

PROS AND CONS OF EXPERIMENTAL RESEARCH

In the first section, I describe a critique of the analytical approach that has been formulated recently, discuss some alternatives to ANOVA techniques, and describe the main features of the experimental approach as it has been used to conduct CPS research. I start with some provocative and critical remarks.

A Critique of the Analytical Approach

In a rather amusing paper, Dörner (1989) has illustrated his critique of the analytical approach to studying CPS (i.e., the experimental analysis of complex behavior) by using an example of strange green turtles that have invaded earth from outer space and have been found by human scientists. The scientists, of course, want to understand how the turtles behave. Unbeknownst to the scientists, the turtles' behavior can be described by a finite state automaton and is rather simple: they drive through pipes with little space shuttles and polish the tubes wherever they find dust. Sometimes the turtles appear to sleep; at other times, they behave restlessly as if they were searching for something special. They also react to light differentially: a red light makes them stop, a green one lets them go, etc. The researchers propose to analyze the turtles' behavior experimentally in a *turtle box* in order to find contingencies between the turtles' behavior and the degree of dust or type of light they encounter. Analysis of variance reveals that 15% of the variance in the turtles' behavior is due to the type of light the turtles encounter. If one uses the previous behavior of the turtles as additional predictor, one gains an additional 11% in variance explained.

Dörner's (1989) main argument is that the program behind the turtles' behavior, driven by a finite state automaton, cannot be detected by any experimental analysis that is based on the general linear model. Instead of aggregating and averaging over situations, one needs to very precisely describe the individual turtles' behavior, based on long periods of observation, if one wants to understand "what makes the turtles tick." Indeed, Dörner reports that it was one of the researchers' children who developed a good model of the turtles' behavior based on her long observation during playing with the turtles.

Alternatives to ANOVA Techniques

Dörner's provocative paper argues against the use of the experimental method in cases where the subjects under study show interaction phenomena. In these cases, only controlled single case studies in combination with computer simulations of the cognitive processes can reveal what is going on—according to Dörner. But is this really true?

Dörner's story, I argue, misrepresents experimental psychology and is a good example for how ANOVA techniques can be misunderstood. ANOVA is a tool for data analysis; it is not, nor was it ever intended to be, a research method. If one uses experimental designs in one's research, one need not rely on analysis of variance for data analysis. Confirmatory LISREL analysis, for example, can be a very powerful tool for testing causal assumptions. As far as I know, Müller (1993, in press) has been the first to use LISREL methods in the analysis of causal models involving CPS variables. Müller was interested in predicting subjects' control performance from their knowledge about the system. His study was designed according to the principles of latent state-trait theory (see Steyer, Gräser, & Widaman, in press). That is, at two different points in time, subjects' knowledge about the system and the quality of their control performance was measured on two independent, but formally identical systems. This design allows to distinguish state from trait influences. Figure 10.1 shows the causal model that fit Müller's (1993) data well.

The model depicted in Figure 10.1 shows a latent trait variable *I* that represents subjects' ability to identify the relations among the system variables. This ability directly influences the state variables for identification performance at time 1 (I_1) and time 2 (I_2); both, in turn, have direct and strong effects on the state variables for control performances at the corresponding time points, C_1 and C_2, which are moderately correlated.

My main point here is that the misuse of a specific data analysis technique cannot and should not be used as an argument against the use of experimental methods in general. A similar point has recently been made by Riefer and Batchelder (1988) who argue for the use of multinomial processing models of cognitive processes instead of classical ANOVA techniques in human memory research. They do not, and neither do I, argue against the use of the experimental method in general, however.

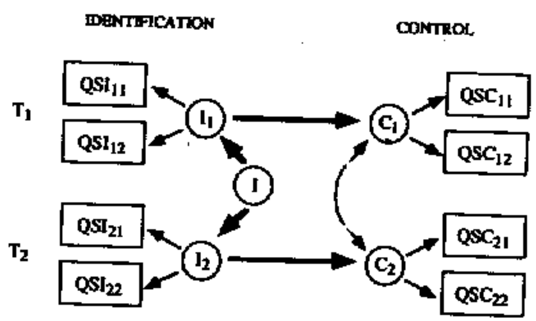

FIG. 10.1. Simplified LISREL model capturing the relation between system identification (manifest variable "Quality of System Identification," *QSI*) and system control (manifest variable "Quality of System Control," *QSC*). The indices stand for time of measurement (T_1 or T_2) and type of system. The latent variables "Identification" (*I*) and "Control" (*C*) also have time indices (adapted from Müller, 1993, p. 102).

Features of the Experimental Approach

Which approaches to examining CPS might a researcher choose? In addition to the experimental manipulation of variables, the researcher might decide to adopt a single-case analysis. The latter approach has as its goal the exact reconstruction of an individual solution approach to a given problem (see Kluwe, this volume). Or the researcher might attempt to construct an artificial system that reproduces subjects' behavior as accurately as possible (i.e., computer simulation of cognitive processes; see Dörner & Wearing, this volume). Although all of these represent reasonable approaches to studying CPS, I am concerned with the first possibility only, that is the experimental manipulation of variables that are deemed important for our understanding of CPS. Next, I shall discuss some pros and cons of the experimental approach.

Separation of Independent and Dependent Variables. One of the fundamental aspects of experimental research is the separation of independent (IV) and dependent variables (DV). IVs are variables that are experimentally manipulated; DVs are response variables that indicate the effects of the experimental manipulation. In principle, and when some assumptions (e.g., randomized allocation of subjects to treatments) are met, this setup allows for a causal interpretation such that the observed effects have been caused by the manipulation of the IVs. The separation of cause and effect in combination with a model that captures the relation between IVs and DVs constitutes the basis for causal explanations which is a high goal in any natural as well as social sciences.

However, some argue that the separation of cause and effect, or IVs and DVs, ignores the fact that for some systems such a differentiation is highly artificial. If one looks at a simple predator-prey-system in ecology, for instance, then it would be a mistake to claim that one of two species is the cause of the other's survival. In reality, both species depend highly on each other due to feedback loops in the system. As Brehmer and Dörner (1993, p. 178) put it, "Thus, in experiments with microworlds we have to give up the traditional focus on stimulus-response laws in favor of more cybernetic conceptions." However, even feedback loops can be described in terms of causes and effects and can thus be examined experimentally. There is therefore no convincing reason to assume that the study of complex systems cannot be approached experimentally.

Control of Manipulation. Closely related to the separation of cause and effect is the question of experimental control. For an experiment to be considered solid, one needs a strong degree of control over the treatment conditions. One of the problems with the control of the manipulation in

CPS research, however, is that typically only the starting values can be controlled in any given system. Once a subject has entered the first response, the subject moves through the microworld on an individual path. Due to the fact that, at least in complex microworlds, there exists a huge number of potential interventions for each simulation cycle, it is virtually impossible that two subjects follow exactly the same pathway through the system. Therefore, the *stimulus* cannot be controlled by the experimenter—the experimenter merely sets the stage for a subject who then follows an idiosyncratic path. Brehmer, Leplat, and Rasmussen (1991, p. 379) point out that "the traditional psychological idea of a strict causation from stimuli to responses must be abandoned, for in these experiments, stimuli are produced by the subjects."

Although I agree that experimental control, at least in a narrow sense, cannot be maintained in complex microworlds, I believe that Brehmer et al.'s conclusion is unwarranted. The loss of control does not invalidate the experimental approach. It does, however, require a more careful analysis of between-subjects effects. Because there exist different individual pathways through a system, one needs to make certain, for example, that the dependent variables are comparable across levels of the IV. One way of achieving this would be to partial the proportion of variance within a dependent variable that is due to the Eigendynamik of the system (see the following); this can be done, for instance, by running the simulation system without any intervention. Another way to achieve comparability across levels of the IV would be to *reset* a system more often and to give subjects a new chance with the same, or comparable, start values. Both of these procedures make the measurement of subjects' performance more reliable.

Replication. One of the criteria for solid experimental work is that an observed phenomenon can be replicated. This requirement distinguishes artifacts and epiphenomena from real, *true* phenomena. In psychological research, replicability is affected by the reliability of measurement. If one is interested in determining a person's IQ score, for instance, and if it is assumed that IQ is a stable trait that does not change much over time, then measures obtained at different points in time should not differ by much. The reliability of measures is thus a necessary requirement in experimental CPS research.

However, if one asks subjects twice to play the role of the major of the small city, for instance, one cannot expect the second performance to be equal to the initial performance. After all, subjects learn about the system during their interaction. Thus, it should not come as a surprise that studies on the reliability and stability of CPS measures all too often yield low scores (e.g., Hasselmann, 1993; Schoppek, 1991; Strohschneider, 1986; Süß, Kersting, & Oberauer, 1991, 1993; but see Müller, 1993; Putz-Osterloh, 1991), leading some to even argue that situation specificity is a characteristic feature

of CPS (see Schaub, 1990), or worse, that unstable data is a reason for not working experimentally but, for example, to perform single-case analyses in order to gain some insight into what is going on.

From the viewpoint of an experimental psychologist, of course, it does not make sense to obtain reliability scores if there is no stable phenomenon. If one is interested in finding replicable phenomena, then one has to conduct better experiments. Data snooping, I believe, is, at best, useful for generating hypotheses. Hypotheses, however, can be generated by other means also.

Objectivity. One of the advantages often claimed by proponents of the experimental method is the objectivity of the method. This means that different people all watching the same event should all come to the same conclusion about what has happened. In the case of experimental CPS research, this implies the use of measures that objectively capture a subject's knowledge and behavior. Notice that the existence of an experimental treatment effect does not necessarily imply that the subject is aware of the effect—what is important is that the treatment has an effect on the dependent variables.

Of course, one might argue, as some do, for a strong reliance on subjects' opinions about, and perceptions of, say, a microworld's demands (e.g., Putz-Osterloh & Bott, 1990; Schaub, 1993; Strohschneider, 1990). If subjects do not perceive any difference between two selected systems with respect to their cognitive demands, then the systems should be categorized as of equal difficulty, regardless of whether or not objective measures show significant differences (see the discussion between Funke, 1991, and Strohschneider, 1991a). The main point here is that subjective evaluations are assumed to be more important than any criterion variable.

I believe strongly that any reliance on the self-reports of subjects is mistaken. There are simply too many processes going on during CPS that, although they might never reach subjects' awareness, nevertheless affect CPS performance.

Summary

There are at least two different opinions concerning the adequacy of experimental methods for studying CPS (cf. Eyferth, Schömann, & Widowski, 1986). Some argue that such complex phenomena cannot be analyzed with classical experimental techniques but require different techniques, such as, for instance, cognitive modeling (e.g., Brehmer & Dörner, 1993; Dörner, 1989, 1992; Dörner & Wearing, this volume; Schaub, 1993; Strohschneider, 1991a). Others argue for the use of experimental techniques because of their central role in scientific progress (e.g., Funke, 1991, 1993; Hussy, 1985; Kluwe, this volume; Müller, 1993; Strauß, 1993). It appears that the controversy may, at least in part, be due to a misconception of what experimental methods really are. Experimental techniques are not only useful in testing a set of static assumptions; they can also be used to test dynamic process models.

A TAXONOMY OF VARIABLES THAT AFFECT COMPLEX PROBLEM SOLVING

Before describing examples of experimental studies, I will first introduce a taxonomic scheme in order to structure the research on CPS that has been performed. According to the taxonomy, three different factors affect CPS performance, namely person, situation, and system variables. The taxonomy was first presented in Funke (1986; later refinements in Funke, 1990) as an elaboration of Hussy's (1985) two-factor model. Hussy proposed a differentiation between person variables and problem variables. Funke's three-factor proposal was subsequently criticized by Strohschneider (1991a) and Strauß (1993). According to Strauß, the main controversy is one between a more operationally oriented position (the experimenter's view) as represented by Funke (1990), and a more subjective point of view (the subject's view) as represented by Strohschneider (1991a). Strauß himself argues for a two-fold taxonomy consisting of person factors and problem factors. I present next the original taxonomy from Funke (1990) with its three classes. The three factors are introduced first.

Person Factors

Person factors comprise competencies that a subject introduces into the CPS situation and competencies that a subject acquires during interaction with the situation. For example, subjects working with a certain simulation system may be experts in the simulated domain or may be novices (see, e.g., Reither, 1981). Also, subjects may learn more or less about the dynamics of a simulated scenario during their exploration and control of the system (see, e.g., Heineken, Arnold, Kopp, & Soltysiak, 1992).

Situation Factors

Situation factors include different experimental contexts in which a simulation system can be embedded. Situational context factors are independent from the used scenario. For example, subjects may be instructed to either passively observe a system or to actively make interventions (e.g., Funke & Müller, 1988), or subjects may be presented with a diagram describing the system's relations or not (e.g., Putz-Osterloh, 1981).

System Factors

System factors represent specific attributes of the used system, that are either formal or content related. For example, the same simulation system of an epidemic disease may be presented to subjects as simulating a harmless flu or as simulating a dangerous smallpox propagation (see Hesse, 1982). Independent of this semantic embedding, the task may vary on the situation factor as being transparent with respect to its interrelations, for example, or not.

Summary

Taxonomies are useful for structuring research domains. Traditionally, taxonomies in the area of problem solving have differentiated between well-defined and ill-defined problems in terms of their givens and goals (e.g., Reitman, 1965). In CPS, three main factors can be distinguished where each factor can be manipulated independently of all others. These three factors are the person, the given situation, and the system, or task, at hand.

EXPERIMENTAL RESEARCH ON COMPLEX PROBLEM SOLVING

In the following, I discuss some experimental results for each of the above mentioned influence factors. The section is intended to demonstrate the merits of experimental research on CPS. Therefore, I discuss only a specific selection of studies and do not give a complete overview.

Studies on Person Factors

Studies exploring the effect of person factors on CPS tend to focus on test intelligence as one dominant and important person variable. In addition, comparisons between experts and novices and analyses on clinical groups and on strategic preferences belong into this category. Other person variables that have been explored theoretically as well as experimentally but are not discussed here because of space limitations, include self-reflection (e.g., Putz-Osterloh, 1985; Reither, 1981), value orientation (e.g., Reither, 1985), emotions (e.g., Dörner, Reither, & Städel, 1983; Hesse, Spies, & Lüer, 1983; Städel, 1987) and language (e.g., Roth, 1985; Roth, Meyer, & Lampe, 1991).

Test Intelligence. Strohschneider (1990, 1991b) has compared the predictive value of test intelligence for CPS performance under two different experimental conditions. All subjects operated an abstract system called VEKTOR first, and were then confronted with the semantically rich peace-corps worker simulation system, MORO. The *Berlin Intelligence Structure* test (BIS; see Jäger, 1982, 1984, for a detailed description) was used to assess subjects' intelligence. The BIS differentiates between two factors, a content-oriented component representing knowledge in three different modalities, and a process-oriented component representing four operative abilities. For the MORO system (Strohschneider, 1991b, Exp. 2), *all* of the seven subtest scales correlated significantly with a general measure of control performance; the same was found for the VEKTOR system with respect to six out of the seven subscales. Strohschneider concluded that test intelligence was indeed a significant predictor of CPS performance. Comparing the two systems, Strohschneider found that performance on the two was not correlated significantly. This indicates that a single CPS trait may not be responsible for performance under all conditions.

Süß, Oberauer, and Kersting (1993; Süß, Kersting, & Oberauer, 1993) also assessed the value of test intelligence for predicting CPS performance measures. In their study, the authors used the well-known system TAILORSHOP under intransparency conditions. All subjects were also asked to complete the intelligence test BIS. Using traditional measures of performance for the TAILORSHOP (total assets at the end of simulation and the number of simulated months with a revenue), the authors found no significant correlations with any of the BIS scales. Based on the assumption that the (unknown and therefore perhaps low) reliability of the CPS measures could have caused this zero-effect, the authors tried other ways of operationalizing the TAILORSHOP performance. In a task analysis, the authors discovered that two subgoals were used by clever problem solvers that, unfortunately, conflicted with each other: shirt sales and profit margin. Due to system inherent characteristics, all subjects had a negative profit margin; lucky problem solvers, however, decreased this negative value and at the same time increased shirt sales. Because revenues are the product of sales and profit margin per shirt, clever problem solvers increased their losses despite the fact that they were using efficient strategies. When Süß, Oberauer, and Kersting (1993) constructed a new dependent measure, namely the *sum* of the increases in shirt sales and in profit margin, then the new measure of CPS quality correlated significantly with the BIS scale "capacity of information processing."

Taken together, the two studies demonstrate that intellectual abilities have predictive value for CPS results when certain conditions are met: (1) instead of using a global IQ measure, one needs to separate different components of intelligence of which the "capacity of information processing" appears to be the most promising predictor; and, (2) CPS quality has to be measured reliably—a condition which is rarely met. For more details on the role of intelligence in CPS, see Hörmann and Thomas (1989), Hussy (1989, 1991), Jäger (1984, 1991) and Beckmann and Guthke (this volume). A recent review on this topic is given by Kluwe et al. (1991).

Expert-Novice Comparisons. Reither (1981) was the first to validate the third-world scenario DAGU by comparing the control performances of experienced technical advisers, who had about ten years of practice in third-world countries (i.e., experts), and of postgraduate students who had just begun their first mission as development aid volunteers (i.e., novices). Both experts and novices worked on the system DAGU in groups of three. The main result of Reither's study was that experts showed a broader range of actions and a greater willingness to make decisions from the start, but also that experts used only standard strategies and were not able to adopt to changing task conditions. Reither calls this behavior the "blindness of the specialists" (see also Frensch & Sternberg, 1989). Despite these strategic differences between novices and experts, *both* groups performed terribly on the system, however. In both groups, the number of inhabitants of the simulated country had decreased dramatically after 20 simulated years, due to starvation. This finding leads to the (as of yet unanswered) question if either the experts did not acquire any usable knowledge during their ten years of practice, or if the simulated system did not capture reality in a valid manner.

Schaub and Strohschneider (1992) examined if managers and students act differently when dealing with the MORO scenario. In MORO, subjects have to take the role of a peace-corps worker in Africa. The authors reported that the managers' problem solving behavior was characterized by a more intensive exploration of the scenario and by a more cautious and adaptive way of adjusting to the demands of the task. On average, the managers achieved better results than the students. One potentially confounding factor in this comparative study was the age of the subjects, however. The managers were 25 years older than the students, on average, and, thus, had more life experience. Similar studies comparing students of economy with professors of that discipline on their performances on the TAILORSHOP scenario have been conducted by Putz-Osterloh (1987; see also Putz-Osterloh & Lemme, 1987). These studies are presented in more detail by Buchner (this volume).

Repeated exposure to a problem, of course, also produces a certain degree of expertise and should therefore lead to better problem performance and representation. This could indeed be demonstrated in a number of studies where subjects had to work on the same scenario for more than one simulation period (e.g., Dörner & Pfeifer, 1992; Funke, 1985; Heineken et al., 1992; Schmuck, 1992). In general, it seems fair to argue that knowledge is an important predictor of control performance (Funke, 1985; Putz-Osterloh, Bott, & Houben, 1988), although dissociations between the two variables in CPS situations have also been reported (see Berry & Broadbent, 1984; Broadbent, FitzGerald, & Broadbent, 1986; Hayes & Broadbent, 1988; for critical remarks see Haider, 1992, 1993; Sanderson, 1989). These dissociations are covered in more detail by Berry and Broadbent (this volume).

Clinical Groups. In a prospective longitudinal study, Fritz and Funke (1988; see also Fritz & Funke, 1990) compared the quality of CPS in pupils who had *minimal cerebral dysfunctions* (MCD) and in matched controls (CON). Working with the dynamic system OEKOSYSTEM, all subjects were asked to first explore and then to control the system. In terms of the quality of subjects' knowledge acquisition (as revealed by causal diagrams of the assumed structural relations), the authors found that the MCD group did not acquire significantly less knowledge than did the CON group. However, the strategies used differed markedly for the two groups. Subjects in the CON group used single variation interventions which tend to reveal the causal relations among variables three times more often than did subjects in the MCD group. With respect to the quality of system control, there was no significant group difference, although 20% of the subjects in the CON group reached the required goal state at least once, whereas almost none of the MCD subjects did.

Strategies. Schmuck (1992) used a self-constructed instrument to assess the degree to which subjects spontaneously exert executive control and compared subjects with high and low efficiency of executive control on their performances in the FIRE scenario. Subjects classified as highly efficient showed better performance from the start but also needed more time and made more interventions. These subjects also showed a greater variability in behavior than did subjects classified as low in efficiency (see also Krems, this volume). Schmuck argued that these strategic differences explain the low stability scores found in many CPS studies (a similar argument has been made by Ringelband, Misiak, & Kluwe, 1990). The explanation relies strongly on the assumption, however, that Schmuck's instrument allows a reliable differentiation between people who differ in efficiency, a fact that has yet to be demonstrated.

Vollmeyer and Holyoak (1993) have recently analyzed the strategies subjects use when exploring, controlling, and predicting an unknown complex dynamic system called BIOLOGY LAB. Subjects were categorized according to their exploration behavior as either (1) using a scientific strategy, (2) using systematic variations of a strategy, or (3) using unsystematic variations of a strategy. As expected, strategies (1) and (2) led to a better representation of the system and to a better prediction of system states than did strategy (3). Surprisingly however, no group differences were found for subjects' control performance. The authors interpret their result as indicating that different types of knowledge are necessary for the three different tasks (see also Reichert & Dörner, 1988, on the use of simple heuristics).

Putz-Osterloh (1993) also strongly recommends strategy analyses for the explanation of individual differences. Using the DYNAMIS microworld, she found significant improvements in structural system knowledge for subjects using efficient strategies for intervention, a finding that stands in direct contrast to the findings reported by Vollmeyer and Holyoak (1993).

Studies on Situation Factors

In studies exploring the role of situation factors on CPS, several variables have been experimentally manipulated, including the type of task, the effects of noise-induced stress, individual versus group problem solving, the transparency of system variables, and the type of the system presentation.

Type of Task. Funke and Müller (1988) conducted an experiment with the SINUS system in which the subjects' task was to first explore an unknown dynamic system for a given number of trials through either passive observation of another person interacting with the system or active intervention. Later, all subjects were asked to control the system such that given goal states were reached. The dependent variables in this study were the quality of knowledge acquisition and the quality of control performance. Results showed that active intervention led to better control performance but reduced the amount of verbalizable knowledge. Surprisingly, the observers, who were poor in control performance, constructed better causal diagrams showing the system variables; thus, they appeared to have acquired knowledge about the variables and their interrelations but not about how to control the system.

Berry (1991) performed a similar study using the SUGAR PRODUCTION and the PERSONAL INTERACTION tasks. In her Experiment 1, subjects had to first watch an experimenter interacting with the system and then to control the system by themselves. It seemed as if subjects did not learn anything through pure observation, neither on the control scores nor on the post-task questionnaires. In a second experiment, Berry found that learning by observation was however possible when the task was changed from a task with non-salient relations to a task with salient relations among the system variables. The effect of this modification was apparent on both diagnostic measures, on the quality of control performance as well as on the system knowledge as measured by a questionnaire (for more details, see Berry & Broadbent, this volume).

Stress. Dörner and Pfeifer (1992) tested the effects of noise-induced stress on CPS. Despite the fact that stress is certainly a person variable this study is subsumed under situation factors because the experimental conditions manipulated situational aspects. The authors used a version of the FIRE scenario developed by Brehmer and his associates (Brehmer & Allard, 1991; see also Brehmer, this volume). The subjects' task was to manage five different fires either under conditions of a stressful white noise or under quiet conditions. Time pressure was present in both conditions. At a global level, there was no difference between the two conditions with respect to their success and failure rates. A more fine grained analysis—looking at subjects' tactical decisions—revealed, however, that although stress did not affect the number of errors made, it did affect which types of errors were made (for a classification of CPS errors see, e.g., Dörner, 1991). For example, an incorrect dosage of fire fighting interventions and a more reactive type of behavior was characteristic of the stressed subjects.

Individual versus Group CPS. Köller, Dauenheimer, and Strauß (1993) compared group CPS to individual CPS. In a first session, all subjects worked individually on the scenario FUEL OIL DISTRIBUTION. Performance on the system was then used to classify subjects as either good or poor problem solvers. Then, in a second session, subjects worked on a very similar scenario called TEXTILESHOP either individually or in a dyad consisting of either two poor or two good problem solvers. It turned out that the individual problem solvers' performances were worse than the performances achieved by the dyads. For the latter, it did not seem to matter whether they were composed of good or poor problem solvers.

Leutner (1988) worked with pupils who had to deal with a derivative of the TAILORSHOP either as individuals or in groups of three persons. In distinction to the previously reported work it turned out here that knowledge acquisition was significantly higher for individuals than for groups but with respect to control performance there was no difference (for more details see Leutner, 1992).

Badke-Schaub (1993) analyzed problem-solving strategies of individuals and groups dealing with a model for the epidemic of AIDS. Subjects had to propose interventions to prevent the spreadout of the disease. Badke-Schaub found that groups have problems to define a common goal but have advantages in finding problem-relevant informations. Groups also produced more proposals for solutions but found it difficult to select one or more of these proposals.

Transparency. Putz-Osterloh and Lüer (1981; see also Putz-Osterloh, 1981) investigated the effect of transparency on problem solving quality in the scenario TAILORSHOP. One group received the system under conditions of intransparency; here, subjects were told only which interventions were possible but did not receive further information. The second group received a graphical representation of the relations among (almost all of) the system variables. After 15 simulated time cycles (i.e., months), subjects in the transparency condition had achieved better scores on the performance measure. In addition, the correlation between system performance and test intelligence was also moderated by transparency. Only under transparent conditions was

a small but significant rank correlation between the two variables observed; under intransparency conditions, the correlation was zero. Putz-Osterloh and Lüer (1981) argued that the equivalence of the two tasks—the intelligence test and the CPS task—might have been much higher under transparency than under intransparency conditions. In the former case, both tasks shared the attribute that information was given to subjects who had to analyze it. In the latter case, CPS required additional information search procedures that were not necessary to complete the intelligence test. Although it may have been true for the Putz-Osterloh and Lüer study, Funke (1983) has shown empirically that this assumption does not generally hold: The moderating effect of transparency on the IQ-CPS relationship is lost in favor of a main effect of test intelligence if one selects a larger range of IQ values than those shown normally by student subjects.

In a recent study, Putz-Osterloh (1993) again manipulated the transparency of a system by presenting, or not presenting, a structural diagram of the system DYNAMIS. In this study, the experimental group which received the diagram was not superior to a control group without diagram on measures of task performance and strategy selection. But on a follow-up transfer task with a modified system, the experimental group outperformed the control group on both types of indicators. Putz-Osterloh concluded from these results that knowledge acquisition is not necessarily a prerequisite for good control. The strategies that are applied may be more important for predicting the quality of performance.

Information Presentation. Hübner (1987, 1988) performed an experiment in which 20 subjects had to control a simulated GAS ABSORBER. The system state was displayed either in an analog or a numerical format. With respect to the dependent variable *Quality of Control* it turned out that the analog group was significantly better and also needed less time than the group with numeric presentation.

Studies on System Factors

Experimental research manipulating system attributes has concentrated on the effects of the variables Eigendynamik, feedback delay, and semantic embedding.

Eigendynamik. In a series of experiments, Funke (1993) systematically varied several system factors, one of which was the *Eigendynamik* of the system (Exp. 2). Eigendynamik is present when a system changes its state at time *t* due to the values of some variables at time *t*-1 but does so independently of any input by the operator. In the extreme case, Eigendynamik means that a system changes over time despite the fact that no active intervention has occurred. Many natural systems show this property requiring an operator to anticipate the system's inherent changes due to the Eigendynamik (see, e.g., de Keyser, 1990). Funke (1993) has used the SINUS system, an artificial system simulating the growth of living creatures from a distant planet with three exogenous and three endogenous variables, to study the effect of Eigendynamik. There were three different conditions, a control condition with no Eigendynamik, and two conditions with different degrees of Eigendynamik. The results demonstrated that increased Eigendynamik yielded a decrease in the quality of system control although the quality of system identification remained unaffected by the manipulation. This pattern of findings suggests that the two dependent variables may tap different processes that are differentially affected by Eigendynamik.

Feedback Delays. Heineken et al. (1992) tested the effects of feedback delay on CPS by using a simple system called TEMPERATURE in which subjects had to control the temperature of an artificial system for 1.200 simulation cycles. Feedback concerning the quality of the intervention was either immediate or after little or much delay. In addition, half of the subjects were informed in advance which delay condition would be realized. Heineken et al. reported that, (a) the quality of system control decreased with increasing delay, and, (b) a priori information about the delay was not effective. Interestingly enough, even in the much-delay condition, subjects were—after a long period of time—able to control the system. This indicates that although feedback delay may influence the rate of learning, it does not appear to completely block the ability to master a time-delayed systems.

Other studies manipulating feedback delay have been performed by Funke (1985), Matern (1979), and, most notably, Brehmer (1990). Brehmer's research will not be presented here; instead the interested reader is referred to his chapter in this volume.

Semantic Embedding. Hesse (1982) has compared two different semantic embeddings for the same underlying system. EPIDEMIC simulates the spread of a disease in a small community. In one condition, subjects, as the managers of a local health service, were asked to care for people who had the flu. In the second condition, the disease was changed to a life threatening small-pox epidemic. The change in semantics changed subjects' behavior drastically; in the more "dangerous" situation, subjects tended to be, among other things, much more involved, and to take more time for making their decisions.

Another interesting study on the effects of semantic embedding has been reported by Beckmann (1995; see also Beckmann & Guthke, this volume). The author compared two semantic embeddings (CHERRY TREE vs. MACHINE) of the same system structure with respect to subjects' knowledge acquisition and control performances. In this experiment, the semantically rich embedding seemed to prevent problem solvers from using efficient analytic knowledge acquisition strategies.

Problem isomorphs in the sense of Hayes and Simon (1976) have also been used by Berry and Broadbent (1984; see also Berry & Broadbent, this volume), Funke and Hussy (1984), and by Huber (in press; see also Huber, this volume).

Studies on Interaction Effects

The interactions between person, situation, and system factors have been researched less frequently than the individual factors. One selected area concerns the interaction between person and situation variables.

Person and Situation. Rost and Strauß (1993) analyzed the interaction between type of information presentation (numerically vs. graphically) and type of induced mental model (propositional vs. analog) using a simple simulation system called SHOP. Their study demonstrates the usefulness of interaction analysis in CPS research. The authors started with the assumption that the advantages of a certain presentation format (presentation of system information in numeric or in graphical form) would affect performance only if it corresponded to the format in which knowledge about the system was internally represented. The internal representation format was induced in this study in a short training session that either stimulated thinking about the system in terms of propositions (if-then statements) or in terms of a graphical network in which the nodes represented the variables connected by causal links, and the diameter of the nodes indicated the quantitative state of the variables. Rost and Strauß (1993) assumed that a propositional representation of system knowledge would best fit a numerical presentation and that the analog representation would best fit the graphical presentation. The central system variable in their scenario was *Money*. For each of the 25 simulation cycles, the dependent variable *Problem Solving Quality* was set to *+1* if an increase in money had occurred, *−1* in case of a decrease, and *0* in case of no change. The results of this rather interesting experiment are summarized in Figure 10.2.

The figure illustrates a significant disordinal interaction between type of presentation and type of training. The analog training condition showed large differences between the two presentation formats whereas the propositional training differences were much smaller for the two presentation formats. The interaction between person (i.e., representation format) and situation (i.e., presentation format) variables clearly indicates a necessity to go beyond main effects in the experimental analysis of CPS. Other work on interaction research has been done by Leutner (1988, 1992) within his studies

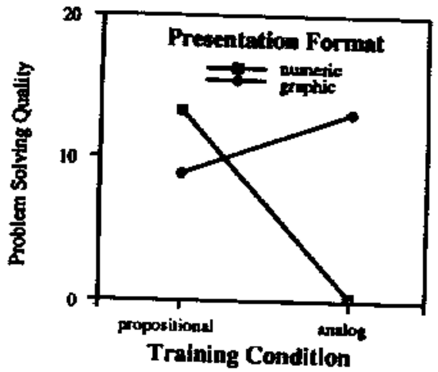

FIG. 10.2. Interaction between type of training and type of presentation format on problem solving quality (adapted from Rost & Strauß, 1993, p. 80).

on aptitude treatment interaction between pupils' ability and type of learning during CPS.

Summary

The short overview over experimental research conducted in the area of CPS illustrates that the different facets of the taxonomic scheme have not received equal attention by researchers. In addition, at least some preliminary conclusions turned out to depend on the selection of the facet under study. If one allows only for a variation in the person facet, one may indeed conclude that there are no situation effects and no effects due to system attributes on CPS. Clearly, studies that simultaneously manipulate at least two of the three mentioned facets are required because only they allow for the analysis of interactions between the different facets. To illustrate the importance of this argument look at the case of transparency: we know very clear about the moderating role of transparency with respect to the relation between CPS performance and test intelligence. Other examples discussed above demonstrate how interesting results can be produced within such multifactor experiments.

CONCLUDING REMARKS

What are the merits, the advantages, and the disadvantages of using the experimental method for exploring CPS? Do the findings presented above really *depend* on the use of the experimental method? Could we have come up with similar conclusions if we had used different techniques? From my very personal point of view, the presented examples demonstrate at least three main points.

First, the experimental method is useful, that is, CPS *can* be fruitfully explored experimentally. The assumption that this research area can, in principle, not be explored through well established analytical tools is simply not justified. The complexity of the research topic is independent of the complexity of the analysis used: complex systems can be studied with simple tools.

Second, the taxonomy presented is a useful one. There is overwhelming evidence for differential effects of person, situation, and system variables on measures of CPS knowledge and performance. One may, of course, discuss whether or not the three facets are independent, semi-independent, or related, but their usefulness for guiding research agendas should not be doubted.

And third, interaction studies are both useful and necessary. It is absolutely essential to conduct more interaction studies because the real story is in the interaction of various variables. One should keep in mind, of course, that interaction analyses are needed only to test interaction hypotheses, and not to test main effect hypotheses.

Are there any problematic aspects of the experimental approach to exploring CPS that must be dealt with? I believe there are, but I also believe that these problems are general problems of the research domain that are not a property of any particular analysis technique or method. I list four of these problems as follows:

The first problem concerns the *measurement of CPS knowledge and performance*. As I stated in the introduction to this chapter, adequate measurement of subjects' knowledge and performance in CPS situations represents a major hurdle that needs to be addressed and resolved before we can make any real progress toward understanding CPS. To this end, Hübner (1989), for instance, has proposed mathematical procedures for the operationalization of certain aspects of task performance. Kolb, Petzing, and Stumpf (1992) propose the use of operations research methods for the same purpose. I personally believe that real progress will not come from these propositions (which, however, may be very useful for certain purposes) but will only come from theoretical advances. Any good theory of CPS must prescribe the dependent variables and must outline how these variables can be measured. Additionally, a theory of the formal system itself may help to select important and reliable indicators of system performance.

The second problem concerns *generalizability and external validity*. Although the artificial systems currently used in our labs are much more complex than they were 20 years ago, we cannot necessarily assume that increased complexity has also led to improved generalizability. Dörner's attempt to bring complexity into the labs of the scholars of thinking and problem solving was successful—but has the situation really changed with respect to our understanding of real-world phenomena? I agree with Hunt

(1991, p. 391) who argues that "Geneticists have a theory that explains how one generalizes from inheritance in the fruit fly to inheritance in human beings. Cognitive psychology does not have a theory to explain how we move from game behaviors to behaviors in other situations."

The third problem concerns the *analysis of problem solving processes*. The experimental method has not been specifically designed for process analyses, although experimental treatments can help in testing assumptions about parameters and their assumed dependence on external factors (e.g., multinomial modeling; see Riefer & Batchelder, 1988). Thus, process models and experiments are not contradictory; they are complementary tools that help us understand CPS.

And finally, the *development of problem solving theories* is in a rather desolate condition. Developing a theory, or multiple theories, is the most difficult job to achieve—and yet at the same time the most necessary prerequisite for additional experimental research. A good theory prescribes and determines experimental research. Theoretical assumptions can be derived from everyday experiences, from cognitive modeling, or from single-case or field studies. Most, if not all, of the assumptions can be tested experimentally—but the experimental method does not in itself prescribe the development of theories.

In my own view, the experimental method will remain the method of choice for studying human CPS simply because no other method is as capable of providing decisive answers to clearly formulated questions. At the same time, however, it remains clear that progress in this difficult research area can be achieved only if different approaches work together to achieve insights into how people deal with complex problems.

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