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Nothing Fails Like Success:

The Search for an Intelligent Paradigm for Studying Intelligence

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The possibility is considered that research on intelligence is entering or is about to enter a time of crisis comparable to that experienced during the decline of the psychometric paradigm as the primary means for studying in- telligence. First, it is suggested that the decline of the psychometric paradigm as the primary means for studying intelligence was due in part to the failure of users of the paradigm to meet in a highly successful manner the challenges that confronted their research. Next, it is shown how the		

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The Search for an Intelligent Paradigm for Studying Intelligence

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Abstract

↘ The possibility is considered that research on intelligence is entering or is about to enter a time of crisis comparable to that experienced during the decline of the psychometric paradigm as the primary means for studying intelligence. First, it is suggested that the decline of the psychometric paradigm as the primary means for studying intelligence was due in part to the failure of users of the paradigm to meet in a highly successful way four challenges that confronted their research. Next, it is shown how, on the surface, users of the information-processing paradigms currently in favor seem successfully to have met these challenges. Then, it is shown that, at a deeper level, the level of success is not as great as it is at a surface level. Finally, conclusions are drawn in response to the challenges that still seem to be facing psychologists studying intelligence.

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Nothing Fails Like Success:

The Search for an Intelligent Paradigm for Studying Intelligence

I believe the time has come at least to consider the possibility that research on intelligence is entering or is about to enter a time of crisis and soul-searching comparable to that experienced during the nineteen fifties and sixties, when researchers experienced certain dissatisfactions with the psychometric paradigm as the sole or primary means for studying intelligence, but were not quite sure of what should replace or supplement it. My contention is that at one level--the level that meets the eye upon a superficial examination of the present condition of intelligence research--current paradigms for studying intelligence have successfully faced the problems that factor analysis seemed to face in a less than wholly successful way, but that at another level--the level that meets the eye upon a deeper examination of the present condition of intelligence research--current paradigms are not facing these problems altogether successfully.

My exposition is divided into three parts. In the first, I state what I believe to have been four of the major challenges that the psychometric paradigm for studying intelligence, in general, and the factor-analytic approach, in particular, failed fully to meet. In the second part, I first show why, at a superficial level, at least, current approaches based upon the information-processing analysis of intelligent behavior are seeming to meet these challenges; I then show why, at a deeper level, I believe all of these challenges have yet to be confronted head on. In the third and final part, I discuss what might be done to meet these challenges. Because of the seemingly negative tone of many earlier parts of the article, I wish to emphasize here that these conclusions will be optimistic--that although all known methods have their limitations, intelligent use of a variety of methods can

result in major advances in our knowledge concerning the nature of intelligence.

The Four Challenges and Factor Analysis

Factor analysis has been and continues to be a highly useful tool for studying intellectual functioning: Nothing I will say is able or intended to refute this assertion. But factor analysis, like any other method of data analysis, is unable to go it alone. I believe that in the case of factor analysis, there are four reasons why supplementary methods of analysis are particularly important in the study of intelligence.

First, as Humphreys pointed out almost two decades ago, factor analysis is a "useful tool in hypothesis formation rather than hypothesis testing" (Humphreys, 1962, p. 475). Factor analysis is useful in hypothesis formation because one can go into it with few or even no ideas about the structure underlying a set of variables, and come out of it with at least some idea of what this latent structure looks like. I believe that nonconfirmatory factor analysis is not useful in hypothesis testing, however, because the inferential machinery supporting it is so weak.¹ There have been, of course, prominent investigators who have taken and still would take issue with this point of view. Burt (despite his apparent proclivity toward "assisting" his data, a competent factor analyst nevertheless) argued that factor analysis should be regarded "not as a source of hypotheses, but merely as a method of comparing, confirming or refuting alternative hypotheses initially suggested by nonstatistical arguments or evidence" (Burt, 1970, p. 17). More recently, Carroll (in press) has argued that "the machinery of factor analysis need not be dependent on any hypotheses adopted in advance of the analysis; actually it affords a way of testing those hypotheses," a way that Carroll believes to be "appropriate and sufficiently objective." But I cannot think of a single plausible psychological hypothesis whose validity has been conclusively tested and established (or disestablished) through the use of

(nonconfirmatory) factorial methods, despite the fact that these methods have been around for three-quarters of a century. Consider, for example, the very basic question of whether there is a general factor in intelligence. Almost eighty years after the first presentation of Spearman's (1904) two-factor theory, has anyone answered through factorial means the question of whether or not a general factor exists? The contents of a set of 16 commentaries on an article I recently wrote (Sternberg, in press-c) make it clear that no one has: Investigators disagree as much now as they did at the turn of the century as to whether to interpret factorial evidence as supporting or refuting the existence of a general factor.

Because of the weakness of its inferential machinery, factor analysis has, in a sense, failed because it has been too successful in supporting, or at least in failing to disconfirm, too many alternative models of intelligence. Horn (1967; Horn & Knapp, 1973) has suggested that Guilford's (1967) theory, in particular, is exceedingly difficult to disconfirm because of the way in which procrustean rotation is used. I have gone further in suggesting that none of the factorial theories are disconfirmable, because in major respects, all of them are correct (Sternberg, 1980a)! They highlight different aspects of intelligence, all of which can be mapped into information-processing terms. On the positive side, therefore, the theories have provided a richly variegated account of human intelligence; on the negative side, however, I do not see how intelligent use of factorial methods can fail to yield a legitimate theory. But science in general, and psychology in particular, progress at least in part by tentatively accepting certain accounts of phenomena, and by tentatively rejecting others: factor analysis has left us little, if anything, to reject. What plausible

theories of intelligence have been disconfirmed by factor analysis? My contention is that not only are the major alternative factorial theories mathematically tenable; they are psychologically tenable as well. The method might therefore be viewed as having failed, in practice, because it has been too successful!

Second, factor analysis has never seemed to be a technique of choice if one's goal is to identify the processes that constitute intelligent behavior. Factor analysis has dealt primarily with products rather than with processes. Even in recent work using confirmatory methods (e.g., Frederiksen, 1980), identification of processes has been through standard information-processing techniques, such as the subtraction method and the additive-factor method. Confirmatory analysis, e.g., analysis of covariance structures (Jöreskog, 1970), has been used to isolate common sources of individual differences in execution of these processes (which could include as well as common processes, common representations of information, common input modalities, common psychological units of analysis, etc.). Perhaps potentially, factor analysis might have told us or still might tell us more about information processing than it has. The past evidence, however, suggests that it is not a useful technique for separating process from other sources of individual differences, such as content. In Thurstone's (1938) theory, for example, the distinction between process and content is not clear. The one theory in which the distinction is very clear is Guilford's, but I suspect this reflects more Guilford's creative conceptualization of intelligence than the results of factor analyses performed on Guilford's or others' data (see the papers by Horn cited earlier). The fact of the matter is that at least until now, factor analysis has not been at its best in elucidating the processes constituting intelligent performance.

Third, by the end of the sixties, it seemed as though nonconfirmatory analysis had told us pretty much what it was going to tell us about the nature of in-

telligence. This view is not a negative assessment of the cumulative contribution of factor analysis; rather, it represents a belief that after three-quarters of a century, nonconfirmatory versions of the technique, at least as they have been used in the past for the analysis of IQ-test items, have pretty much been milked dry. Carroll (1980) has suggested that

factor analysis is not at all as 'indeterminate' as it is often depicted to be, and as it was in fact depicted by Sternberg in his book (1977). With well-designed studies, the principles of simple structure can pretty well dictate the final solution. Parsimony is the essential principle underlying the idea of simple structure; it says that one wants to account for a given variable with the minimal number of factors--often with only one factor.

I do not agree with Carroll that the issue of a preferred rotation is dictated by anything, other than the mathematical constraints of rotation, which allow an infinity of valid rotations rather than a single one. I also consider parsimony to be only one of a number of criteria one should use in assessing the value of a factorial theory, or any other kind of theory, for that matter. (See Sternberg, 1977, Chapter 5, for a discussion of some other criteria--completeness, specificity, generality, plausibility--that matter as well.) And certainly, the issue of a preferred rotation (like, it seems, many other issues pertaining to factor analysis) remains a matter of debate. Contemporary factor theorists other than Carroll (e.g., Cattell, 1971; Guilford, 1967; Horn, 1968; Snow, 1978; Vernon, 1971; to name a few) continue to use solutions other than simple-structure ones in their theorizing about intelligence. But if the issue of a "correct" rotation had been decided, then I would be even more convinced that tradi-

tional factorial techniques have not recently been telling us a great deal new about the nature of intelligence: I don't believe we've learned a great deal more about the simple-structure nature of intelligence than we knew from Thurstone's early investigations of it (e.g., Thurstone, 1938)! I am not stating a belief that simple-structure solutions do not provide us with useful information about intelligence. To the contrary, I believe that these solutions, and others as well, provide us with a great deal of useful information (see Sternberg, 1980a, in press-c). But I do not see that with the coming of the nineteen eighties, they are likely to provide us with much new information, unless they are applied to new materials or otherwise applied in new ways.

Fourth and finally, I believe that by the end of the nineteen sixties, factor-analytically derived theories were being perceived as less informative than might have been hoped with regard to their implications for instruction, in particular, instruction in intelligent information processing. Presumably, these theories were less helpful in this regard than one might have hoped because they did not make clear just what it is that should be trained. Obviously, one could train subjects in their performance on the items that compose the factors. But although the theories may delimit the class of items to be trained, they do not indicate how to train performance on these items. Instead of decomposing the items into smaller, more concrete and potentially trainable units, they relate the items to factors that are larger, more abstract, and probably less trainable than the items. It is easier to see, for example, how one might train analogy performance than it is to see how one might train performance on a factor of "reasoning" or of "general intelligence." One possible exception to this generalization (and there may well be others) is Guilford's (1967)

theory, where the factors seem to specify fairly elementary processes, contents, and products. Here, oddly enough, the problem might not be in the paucity of implications for training, but in their plethora: There are 120 abilities postulated in the model.

The Four Challenges and Information-processing Analysis

An Optimistic View

At first inspection, contemporary information-processing approaches to intelligence seem to be doing admirably in meeting squarely the challenges factor analysis met less than adequately. Indeed, contemporary approaches to the study of intelligence were formulated at least in part to mitigate these and other apparent "inadequacies" of factor analysis used in the absence of converging methods of analysis.

First, the methods of data analysis used in information-processing investigations of intelligence are highly useful in hypothesis testing (although probably less useful in hypothesis formation). Although the major statistical methods used in such investigations--analysis of variance and regression--are based upon the same general linear model upon which factor analysis is based, inferential statistics for the former two methods are much more highly developed than they are for factor analysis. One could, of course, argue over the potentials for hypothesis testing of factor analysis as opposed to analysis of variance or multiple regression. But to a large extent, the "proof of the pudding is in the tasting," and even a perfunctory review of the two literatures will reveal a much greater emphasis upon, and more successful use of, hypothesis testing in the information-processing literature than in the factor-analytic one. In one kind of information-processing analysis, computer simulation, the use of inferential statistics is often at a minimum. But even here, strict hypothesis testing is possible, albeit hypothesis testing of a different kind: The simulator

can test whether his or her program is a sufficient account of behavior simply by seeing whether the program (a) runs and (b) produces the desired pattern of responses. And these, after all, are the tests that the simulator is interested in demonstrating that his or her program can pass. Reviews and examples of the kinds of inferential tests that can be performed in the analysis of human-subject data collected via the information-processing approach can be found in Sternberg (1977, 1978, 1980b).

Second, information-processing techniques such as the subtraction method and the additive-factor method (see Pachella, 1974, for a readable description of these two methods) are highly useful in identifying processes that contribute to intelligent performance. Indeed, these techniques were formulated primarily with this goal in mind. It is often possible not only to identify these processes, but to identify as well the latencies or difficulties of the processes, the strategies into which the processes combine, the representations upon which the processes act, and the consistency over time with which the processes are used. Examples of such analyses can be found in the work of Hunt (Hunt, Frost, & Lunneborg, 1973; Hunt, Lunneborg, & Lewis, 1975), Pellegrino and Glaser (1980), R. Sternberg (1977, 1980b), S. Sternberg (1969), and many others.

Third, I do not see any indications that we have exhausted the potential of the information-processing paradigm to yield new insights into the nature of intelligence, nor do I see indications from the literature that others see this as an impending problem either. To the contrary, it has only been during the past decade that information-processing analyses of intelligent behavior have made a serious and concerted start (with a few earlier exceptions; see, e.g., Gagné, 1967), and all indications seem to be that it will be quite a while before the paradigm is exhausted in its

ability to yield new and interesting findings. Obviously, there is no "acid test" of what constitutes a still-productive paradigm. But the sheer volume of work being done in a large and growing number of different laboratories would seem to attest to the productivity of the paradigm in generating what many investigators apparently believe to be worthwhile research. In contrast, by the late nineteen sixties, the proportion of researchers still doing factorial analyses of intelligence had dwindled to a low level, after a prolonged period of slow but steady decline. Some of the interesting theoretical pursuits currently in progress include attempts to account for intelligent information processing in a variety of tasks via a relatively small number of information-processing components (e.g., Carroll, 1976; Hunt, 1978; Jensen, 1979; Pellegrino & Glaser, 1980; Sternberg, 1979); attempts to build computer models that can perform a wide variety of tasks intelligently (e.g., Anderson, 1976; Schank & Abelson, 1977; Simon & Lea, 1974); and attempts to account for mental retardation in information-processing terms (e.g., Butterfield & Belmont, 1977; Campione & Brown, 1978).

Fourth and finally, information-processing accounts of intelligent performance seem to provide implications for and be conducive to training because they decompose tasks into component processes that seem to be (at least in some cases) of a level of complexity that is compatible with instructional attempts. Training of intelligent performance has leaned heavily and directly upon the information-processing theories from which the training has been derived, and has been done successfully in a number of domains, including learning (e.g., Belmont & Butterfield, 1971; Campione & Brown, 1978), reasoning (e.g., Feuerstein, 1979; Holzman, Glaser, & Pellegrino, 1976; Sternberg & Weil, 1980), number skills (e.g., Resnick & Ford, in press), and problem solving (e.g., Siegler, 1978).

To conclude this section, on one view, information-processing analyses have been highly successful in meeting challenges that factorial methods have met with only partial success. But there is at least one other point of view.

A Pessimistic View

There is another side to the information-processing story, one that may lead to a more pessimistic assessment of the current state of the information-processing paradigm for studying intelligence. The pessimistic view is not inconsistent with the optimistic one: It is possible to be both optimistic and pessimistic simultaneously, depending upon the level of analysis one conducts. Consider once again the "four challenges."

First, although specific information-processing theories are usually disconfirmable (see, e.g., Sternberg, 1977, 1980b), the various subparadigms that generate these theories are not disconfirmable; moreover, they have generally been posed in ways that make it very hard to assess whether they are succeeding or failing. Consider some of the major information-processing subparadigms currently being used, and why I am afraid they can be "assured" of continued success.²

One subparadigm is what Pellegrino and Glaser (1979) have referred to as the cognitive-correlates approach. In this paradigm, introduced by Hunt, Frost, and Lunneborg (1973), parameters from rather simple information-processing tasks of the kind used in the cognitive psychologist's laboratory, e.g., the S. Sternberg (1969) memory-scanning task and the Posner-Mitchell (1967) letter-comparison task, are correlated with scores from standardized psychometric tests of mental ability. Hunt et al. computed these correlations, and found them generally to be at the level of what Mischel (1968) has called "personality coefficients": most of

the correlations are at the .30 level. How does one interpret correlations of this order of magnitude? Mischel (1968), noting the fact that almost anything in the personality literature correlated .30 with anything else, took a rather dim view of the progress that had been made toward understanding the nature of personality. He pointed to the literature on intellectual abilities as a bright spot, however, because correlations in that literature were generally higher. But Hunt and his colleagues did not share Mischel's chagrin, perhaps because researchers in the field of intelligence have not had to confront as directly as have researchers in the field of personality what seems to be a basic fact--that within a given domain such as personality or ability measurement, most concurrent test scores correlate at about the .30 level with each other. Hunt and his colleagues were sanguine, and it is worthwhile to quote them at length:

The general psychologist will have noted that while we have shown significant results, we have not shown strong correlations. Indeed, our initial studies can be criticized on the grounds that we have shown a large number of moderate correlations, rather than having elucidated any one relationship in greater detail. Our data, then, may be more suggestive than conclusive. One of our original points, however, was that low positive correlations are of interest. Although it seems reasonable on a priori grounds to expect that the study of individual differences in cognition would locate the same critical underlying variables of human performance that would be revealed by an experimental approach to cognition, this, in fact, has not happened. Psychometrics and cognitive psychology appear to be currently sharing information about man, but by no means do they duplicate each other. At least, we see no other way to account for the pattern of our results. (pp. 114-115)

Levels of correlations between cognitive tasks of the kind studied by Hunt and his colleagues and psychometric tests were at similar levels in subsequent studies (see, e.g., Hunt, Lunneborg, & Lewis, 1975), although Hunt (1978) showed that with much wider ranges of ability levels (e.g., studies including retarded individuals), it is possible to boost these correlations, as would be expected from the increase in individual-difference variation caused by the expansion of range. The question one must pose regarding the "personality coefficients" Hunt has obtained in the majority of his studies is whether there is any set of plausible results that would differ from those Hunt obtained. On the one hand, it seems unlikely that the relatively low-level processing required by simple tasks such as comparing the physical appearance or names of letters would yield high correlations with relatively high-level tasks like complex algebra word problems. Indeed, a long-standing literature comparing the approaches of Galton (low-level tasks) and Binet (high-level tasks) prepared us for the extreme unlikelihood of such an event. On the other hand, it seems unlikely that the cognitive tasks and psychometric tests would be wholly uncorrelated or only trivially correlated. Complex problems, such as algebra word problems or verbal analogies must draw at least to some extent on the perceptual and memory processes that are salient in the lower-level tasks. Although these processes are of much less importance in the higher-level tasks, they cannot be bypassed altogether, and to the extent that they are sources of individual differences, they will result in at least some correlation between the cognitive tasks and the psychometric tests.

To conclude our discussion of the cognitive-correlates approach, it is not clear what set of plausible outcomes would be inconsistent with the

utility of the cognitive-correlates approach, at least given Hunt, Frost, and Lunneborg's view of what constitutes success of the approach: Correlations that can be interpreted as "moderate" include a very wide range of values, and moreover, most cognitive measures are correlated at a "moderate" level with each other. These moderate correlations tell us nothing about directions of causal relationships, although one might interpret Hunt et al. (1975) as inferring from such correlations that speed of access to codes in working memory is to some extent causative of individual differences in verbal ability. Equally consistent with these data would be the possibility that high verbal ability leads to quick access, or that both high verbal ability and quick access are dependent upon some third variable.

Suppose high rather than moderate correlations are obtained in a cognitive-correlates study. What can one then conclude? Unless there is a strong theory underlying the newly discovered relationship, probably not much. I have seen numerous cases in which the high correlations represent nothing more than the fact that the predictors and criterion or criteria are very highly similar to each other, sometimes even on their face.

A second subparadigm is what Pellegrino and Glaser (1979) have called the cognitive-components approach. In this approach, investigators analyze complex tasks such as those actually found on intelligence tests, seeking to decompose performance on these tasks into elementary constituents of some kind, usually component processes. In this approach, the goal is to account for as much of the variation between stimulus types as possible, usually using either response time, response choice, or error rate as a dependent variable. Numerous tests of model fit can be performed (see Sternberg, 1978), so that one does not run into the problem of trying to decide what "moderate" level of fit to be happy with. Ideally, one wants as high a degree of model fit as is possible, given the reliability of the data.

In this subparadigm, disconfirmation of specific models of information processing is both possible and likely: Very few, if any, models are "true" in the sense that they specify veridically just what subjects do. Moreover, it is possible to reject alternative models, and to select the best one on a tentative basis. Although it may not be a "true" model, one can accept it as the best of the available models until a better model is found. But the experimental and quantitative rigor of the method hide what I believe to be cause for at least some concern. The approach in itself is not much more disconfirmable than is the cognitive-correlates approach: It, too, cannot fail if used to full advantage.

The cognitive-components approach requires more prior conceptualization and quantitative sophistication for its use than does the cognitive-correlates approach. The investigator must go in with a prior information-processing model that he or she has quantified or simulated on a computer, and can thus test for its validity. But if the investigator is able to do these things, then past experience indicates that there is almost always some componential model that will provide a very good fit to the data. Indeed, there is usually a linear model that will do so, since the predictions of linear models usually accord well with the predictions of nonlinear ones. Thus, given sufficient cleverness on the part of the investigator in manipulating independent variables that are sources of solution difficulty, and in formulating a model for how these sources combine to yield the total difficulty of the item type being studied, past experience suggests that the investigator is highly likely to succeed in modeling task performance. Of course, these are big "givens," and there may be many tasks that do not seem susceptible to this kind of modeling. But there seem to be enough tasks that are susceptible to this kind of modeling to keep investigators busy for quite a while. With enough parameters, of course, success

can be guaranteed, but previous modeling attempts in the literature suggest that most researchers can do quite well even with a fairly modest number of parameters. Even if the model cannot be rejected relative to the true model, one has no guarantee that the preferred model is the true model, since alternative models may all predict a given set of data with little or no departure of observed from predicted values (see, e.g., Carpenter & Just, 1975; Clark & Chase, 1972).

A third subparadigm is what might be called a training approach. In this approach, one starts

with a detailed task analysis of a cognitive endeavor of particular interest to the theorist. If the analysis is thorough enough, it should be possible to instruct an immature learner (or a computer) to perform well on that task. If the instruction (or program) does not result in an appropriate type or level of performance, one likely cause is that the theory is not specified in sufficient detail. Ideally, the way in which performance deviates from optimality will provide more specific hints about the ways in which the theory needs elaboration.... Finally, the point where training ceases to be effective is of central importance to the development of a theory of intelligence. The underlying assumption is that as the difficulty of instructing some important component increases, i.e., the component begins to appear impervious to training, we would argue that the centrality of that component to intelligence also increases. (Campione, Brown, & Ferrara, in press)

This approach, which has been used by Campione and Brown (1970), Belmont and Butterfield (1971), and others, can be helpful in telling us what aspects of cognitive functioning are trainable with reasonable amounts of effort, and what kinds of functioning resist training. It can also be helpful sheerly at a practical level in improving people's cognitive

performance. But as an approach to helping us find out the nature of intelligence, I am concerned that it, like the other approaches, cannot fail: Failure in training does not really help us much in disconfirming a theory of intelligence. Unfortunately, even success in training is at best ambiguous in its interpretation.

Suppose, first, that one is unable to train subjects in a given sample of a population either to improve their proficiency in executing a postulated component of a theory of intelligence, or that one is unable to train them to use the component at all. What can one conclude? There seem to be at least four highly plausible alternative interpretations of this outcome. The first is that the component is simply not a component of intelligence--that one cannot train it because it is not a natural part of a functioning cognitive system. A second interpretation is that the component is an aspect of intelligence, but that it is what Campione, Brown, and Ferrara refer to as "impervious to training." Not all intelligent acts need be accessible or even available to consciousness. For example, part of intelligent functioning is the generation of words to represent one's thoughts, although it is difficult to imagine how one could train a skill like this that is so inaccessible to consciousness. A third interpretation is that the component is an aspect of intelligence and is in fact trainable, but that the training methods used are inadequate. One can never know for sure that a failure to train subjects successfully is nothing more than a reflection of one's failure to devise training procedures that work. A fourth interpretation is that the component is an aspect of intelligence and that it is trainable, but not in the population being used in a given study. For example, Campione, Brown,

Belmont, Butterfield, and others have done most of their training work in populations of mentally retarded subjects. Could one reasonably conclude that failure to train a component process in such a population is an indication that the component is not an aspect of intelligence? One might equally well argue that its untrainability in such a population shows that the component is indeed an aspect of intelligence. To summarize, the interpretation of a failure in training is quite equivocal.

Suppose instead that one is able to train subjects in a given sample of a population either to improve their execution of a component or to execute the component at all. What, then, can one conclude? Unfortunately, not much. First, consider one interpretation of a component's being "impervious to training." In the passage from Campione, Brown, and Ferrara cited earlier, it is stated that "as the difficulty of instructing some important component increases,...the centrality of that component to intelligence also increases." But this statement implies that the more successful one is in training a component of intelligence, the less successful one is in demonstrating the centrality of that component to one's theory of intelligence, and vice versa. In other words, to succeed (in training) is to fail (in demonstrating theoretical importance of the component). If there was ever a prototypical example of nothing failing like success, certainly this is it! Second, sometimes subjects can be taught to perform tasks in ways that they almost never would perform them spontaneously; other times, they cannot be trained to perform the tasks in ways that they would have no trouble in using spontaneously. In linear syllogistic reasoning, for example, one can rather easily train subjects to use an algorithmic model that they would almost never use spontaneously; but it is extremely difficult to train them to use the mixture strategy that most subjects use

spontaneously (Sternberg & Weil, 1980). Would one want to claim that success in training the algorithmic model but not the mixed model indicates that the mixed model is not as good a source of information about the nature of intelligence, or vice versa? Certainly not, because in training strategies for analogical reasoning, the opposite pattern holds: It is quite easy to train the strategy subjects use spontaneously, but quite difficult to train the strategies subjects do not normally use (Sternberg & Ketron, Note 1; see also Sternberg, Ketron, & Powell, in press). One can only conclude that the trainability of a set of components (a strategy) bears no clear relation to the nature of intelligence. Third, even if one does succeed in training a component or strategy, one has no guarantee that the component or strategy represents part of what one should call intelligent performance. The fact that a component can (or cannot) be trained does not in itself indicate whether that component is a part of intelligence as opposed to, for example, lower-level perception. The identification of the component as one aspect of intelligence must come from elsewhere (prior theory, correlations with other measures, or whatever), which brings one back to the question that one started with, namely, how does one identify the components that constitute aspects of intelligent performance?

I have reviewed three of the major information-processing subparadigms for understanding the nature of intelligence, and have concluded that in at least one respect, they face the same difficulty as factor analysis. The difficulty is better disguised, but may be viewed as all the more pernicious because of its unobviousness. The problem is that in at least one sense, each subparadigm cannot but succeed in meeting the goals its users and even many of its critics have set for it, and hence, in at least one sense, it cannot fail. I will claim next that information-

processing approaches share other problems of factor analysis, although, again, in an unobvious way. Consider now the "second" challenge.

Second, there can be no doubt that information-processing approaches have fulfilled their promise of identifying component processes in intelligent performance (see, e.g., Hunt, 1978; Mulholland, Pellegrino, & Glaser, 1980; Sternberg, 1977, 1980c). But certain problems lurk beneath the surface: Our success no longer seems as "successful" as we had once hoped it would seem.

One goal of information-processing analysis was to identify a unit of analysis that would in some sense be basic--a unit, for example, in terms of which individual differences in factor scores could be understood (Carroll, 1976; Sternberg, 1977). The hope was that the component process would serve as such a unit, and ultimately provide a "common currency" for the exchange of views regarding the basic nature of intelligence. But it has become increasingly clear that we really have no way of determining what constitutes a basic unit, nor are we, I suspect, even clear as to just what we mean by a basic unit. For example, we have no way at the present time of knowing whether the factor or the component is the more basic unit. People seem to take all possible positions. Carroll (1980) now seems to claim that the factor is the more basic unit, and that it is responsible for individual differences in components; I previously claimed that the component was the more basic unit, and that it was responsible for generating individual differences in factors (Sternberg, 1977). I now believe that the question is not a meaningful one in our present state of knowledge (Sternberg, 1980a). We are no better able to say which unit is more basic than we are able to say which came first, the chicken or the egg. Factor scores can be regressed on component scores; component scores can be regressed on factor scores. What experimental or mathematical operation would enable us to claim with

any confidence that one unit is more basic than the other?

A second goal of information-processing analysis was to tell us how subjects solve complex problems requiring intelligent performance. At one level, information-processing analysis has told us that. We can say, at least to some order of approximation, that solution of analogies requires the execution of operations such as encoding, inference, application, and the like, or that solution of linear syllogisms requires execution of operations such as premise reading, processing of marked adjectives, combination of terms into a visualized spatial array, and the like (see Sternberg, 1977, 1980b). But as Pellegrino and Lyon (1979), among others, have pointed out, the components identified in my and others' "componential" analyses are black boxes. Some of the information we would be most interested in would come from our figuring out what mental events occur during encoding, inference, premise reading, and so on. In other words, how does a person do these things? One approach to this problem is to decompose information-processing performance into smaller components or subcomponents than the ones we have used. Such an approach at least reduces the magnitude of the problem. To date, we have no evidence that it is capable of eliminating the problem altogether.

A third goal of information-processing analysis was to tell us what intelligent performance consists of. But we need to ask ourselves whether the processes we are identifying are ones that we can confidently identify as sources of individual differences in interesting kinds of intelligent performance that occur in the real world, such as making a career choice,

performing well in one's career, deciding how to schedule one's time to maximize one's operating efficiency, and so on. I have serious doubts that the kinds of processes being identified in cognitive-correlate analysis are on the right track. Speed of naming two letters as "same" or "different" does seem to me quite removed from ordinary conceptions of intelligent performance or its antecedents. I have more confidence that the kinds of processes being identified in cognitive-component analysis are on the right track. For example, I am prepared to believe that "inference" is an integral part of intelligent functioning in the real world. I am much less ready to believe, however, that "inference" of the kind used in solving analogy test items is the same as, or closely related to, say, inference in seeing relations between two important historical events, or between two economic indicators. We have what I perceive to be a "levels of processing" problem. There is a large gap between the levels of inference used in laboratory or psychometric tasks and the levels used in more consequential kinds of reasoning performance. And the difference in levels may be of a qualitative as well as of a quantitative nature. Although there have been reasonably successful attempts to show that the parameters named in the same way across different laboratory information-processing tasks are correspondent (Chiang & Atkinson, 1976; Sternberg & Gardner, in press); there have been no attempts to relate these parameters to performance in real-world performance.

I have claimed that although information-processing analysis has identified processes of task performance, this identification has not proven to be the panacea many people hoped it would be. Many of the questions we would like answered about component processes still remain. And other problems remain for the information-processing approach as well.

Third, on the face of things, the information-processing paradigm would certainly not seem to have been exhausted. But we must ask ourselves exactly what it means for a paradigm to be exhausted. Is it, for example, conventional factor analysis that has been (in the opinion of some, at least) exhausted in the factorial paradigm, or is it the types of uses to which we have put conventional factor analysis? I suspect that the latter is the case. We pretty much ran out of tasks to factor analyze or that we cared to factor analyze, and it wasn't clear where to go from where we were with factor analysis. The critical question concerns not so much the technique itself as the use to which the technique is to be put to continue to yield interesting new information about the nature of intelligence.

We have not yet run out of tasks upon which to conduct information-processing analyses. We have yet to see (convincing) process models of anagram performance, remote associates performance, counting of cubes from a three-dimensional surface represented in two dimensions, etc. The question we must ask, though, is that of what is to be gained from isolating component processes from still more tasks like the ones we have analyzed? A number of studies have been done in which component process scores from cognitive tasks have been correlated with scores from psychometric ability tests (e.g., Hunt et al., 1973, 1975; Sternberg, 1977, 1980_b). As noted earlier, the correlations between scores on these tasks and scores on psychometric ability tests have been less than impressive. But these psychometric tests have only been proxies for the criteria we really do or at least should care about--namely, performances in real-world tasks. Although cognitive process scores have not, to my knowledge,

been correlated with performance such as school grades, supervisory or peer ratings, income, or whatever "real-world" criteria one would like, I am inclined to believe that almost inevitably, these correlations would be lower than those that have been obtained with psychometric tests. Such reduced correlations would stem not only from the reduced reliabilities of the criterion measures, I believe, but also from the reduced complexity of the cognitive components relative to the composite psychometric tests that have until now served as the predictors of real-world performance. Past experience in research on intelligence has shown to almost everyone's satisfaction that higher predictive validities for complex outcomes are almost always associated with greater complexities in predictors. Indeed, increasing the reliability of a predictor by simplifying its structure often results in a decrease rather than an increase in predictive validity. We must therefore ask ourselves whether still more information-processing analyses of the kinds of tasks we have studied are likely to turn things around. I suspect they are not. We need new kinds of tasks.

Fourth, consider again the issue of training component information processes. There can be little doubt that some training of cognitive processes is both possible and feasible (see, e.g., Borkowski & Cavanaugh, 1979; Feuerstein, 1979; Holzman, Glaser, & Pellegrino, 1976). Although evidence supporting the durability and generalizability of such training is still meager, we have cause to be at least modestly optimistic regarding the feasibility of training some cognitive skills (Brown & Campione, in press). I believe it important that these training efforts continue, because in terms of theoretically-based programs, training of cognitive skills is pretty much our best option now, even if at times these cognitive skills are more narrow than ideally we'd like. Eventually, though, I think

it important that we supplement these training programs with training in real-world problem solving and decision making of the kinds needed for important events in one's life. Ultimately, the real-world behaviors rather than their proxies are what we are interested in, and we can at least hope that any training effects we can get through the proxies will be strengthened by direct training of the real-world behaviors that we hope to affect.

Conclusions

I have argued that the present state of research on intelligence could be conceived of as on the borderline of a crisis period: Conventional factor-analytic research on intelligence has been less than successful in meeting four challenges that confront intelligence research; although on the surface, information-processing research has been quite a bit more successful in meeting these challenges, at a deeper level, these approaches, too, have been less successful than one might wish. What conclusions can be drawn from the review? I believe there are at least three reasonable ones.

First, we should be wary of a trend in intelligence research to reject old approaches (or new competitive ones) to studying intelligence in favor of our own preferred ones. In the initial burst of enthusiasm that accompanies the success of a new methodology, there is an understandable tendency to view the new methodology as a panacea for the problems of old or new competitive methodologies. There is also a tendency to attempt to sell a new method not only on its virtues, but on the alleged limitations of its competitors. Thus, we have been treated to disquisitions on why cognitive-components analysis is to be preferred over factor analysis (Sternberg, 1977) or cognitive-correlates analysis

(Pellegrino & Glaser, 1979); on why analysis of covariance structures (Frederiksen, in press) or latent trait analysis (Whitely, 1980) is to be preferred over cognitive-components analysis; or on why, if one's goal is to isolate "latent abilities," traditional factor analysis is to be preferred over these and other alternatives (Carroll, in press; Guilford, in press). I am inclined to believe, however, that all of the methods now available have overlapping strengths and weaknesses. The best strategy to follow is to attempt to show in what respects different methodologies lead to the same conclusions in some respects and different conclusions in others regarding the nature of intelligence. I also believe that various methods in fact show striking convergences in the generalizations to which they lead us about the nature of intelligence (Sternberg, 1980a, in press-b, in press-c), a belief I shall discuss further shortly.

Second, I think we need to think more about the criteria we wish to use in evaluating the relative successes of various approaches to studying intelligence. One could draw the conclusion from this review--wrongly, I believe--that none of the methods are very useful because they are flawed in so many respects. A more valid conclusion, I believe, would be that probably all of the methods that have been used can lead to important insights or to dull ones. Advocates of one approach can often turn around the arguments being leveled against their approach by advocates of another approach to apply to that other approach. The value of a contribution seems to lie in how creatively and insightfully a given method is used by an investigator, rather than in the method itself. What seems to matter most is not what method is used, but how it is used. Attempts to argue for or against methods, in the abstract, seem not to be terribly fruitful.

A better use of time might be in arguing about ways in which one or more methods might be put to more productive use by psychologists interested in using the method, in effect, to think of method-investigator interactions. If no better use can be found for a method at a given time, it should by all means be put into cold storage until, perhaps at some later time, a better use is thought of for it.

Third, we need to remember the oft-repeated admonition that the validity of a theory can be adequately tested only through the use of converging operations (Garner, Hake, & Eriksen, 1956). Any one method for studying a psychological phenomenon is incomplete in some respects, and inadequate in others. But if the same phenomenon appears almost without regard to the method that is used to uncover or analyze it, then one's confidence in the validity of the phenomenon is increased. Although each of the methods I have reviewed has inadequacies, the use of a combination of methods (including, of course, ones not reviewed here) can provide a powerful demonstration of a phenomenon of interest.

Finally, I think we need to consider more carefully our choice of tasks in studying intelligent behavior. A common theme running throughout this article has been that in studying sometimes remote proxies for interesting real-world behaviors, there has been some loss in the real-world significance of the outcomes. I would not take the position of Cole (Note 2) that the results of these studies have been misleading. Rather, they have been incomplete. I would like to see our laboratory studies of intelligence supplemented with (but by no means replaced by) studies of real-world behaviors or simulations of such behaviors. The behaviors studied should be consequential ones, such as choosing a college, a career, or a mate, or deciding whether or not to pursue major surgery. Such work has already been undertaken by several investigators. Frederiksen (1952, 1966; Frederiksen, Saunders, & Wand, 1952) has been a pioneer in this respect in his

direction-setting studies of the in-basket technique and in his investigations of creative hypothesis formation and evaluation in scientific thinking (Frederiksen & Evans, 1974). Hayes-Roth and Hayes-Roth (1979) have proposed a theory of real-world kinds of planning, such as the order in which one plans to carry out a sequence of daily errands, and Goldin and Hayes-Roth (Note 3) have found systematic individual differences in the nature of the planning process. Our own recent research has begun to take a more practical bent as well. Rick Wagner and I are investigating the kinds of practical skills and knowledge of value systems people in everyday life and in professions such as law and psychology need to get ahead in their respective pursuits, e.g., how people decide what activities are worth doing in limited amounts of time, and how they budget their time according to the value they place on each activity. Craig Smith and I are investigating the construct of social intelligence, following in the footsteps of Archer (1980) and Rosenthal, Hall, DiMatteo, Rogers, and Archer (1979), among others, in examining how people decode implicit communication, such as the nonverbal cues people emit in expressing approval or disapproval. We are interested in isolating components of social intelligence, if they exist, and in relating them to each other and to components of cognitive intelligence.

Laboratory research has been, and I believe, will continue to be, useful in isolating various aspects of intelligent performance. A recent and particularly promising development has been in the investigation of knowledge representations in people of various levels of expertise who are engaged in solving complex problems such as those found in physics (Larkin, McDermott, Simon, & Simon, 1980; Chi, Feltovich, & Glaser, Note 4) and geometry (Greeno, 1976). My research and that of others (e.g., Hogaboam & Pellegrino, 1978) leads me to believe that a particularly promising route to pursue in the study of laboratory tasks will be that of what I have called nonentrenched, i.e., novel, tasks (Sterndberg, in press).

Performance on such tasks seems to be substantially more highly correlated with performance on psychometric tests than performance on more standard laboratory tasks is correlated with performance on the tests. I agree with Cole (Note 3) and Neisser (1976), however, that we need to pay more attention to macroscopic aspects of information processing that are sometimes overlooked in laboratory task analysis. At the present time, our knowledge of high-level performance in real-world tasks is meager. But if our goal in research on intelligence is to understand intelligence as successful adaptation to and purposive action in one's real-world environment, knowledge about such relations would seem to be essential.

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Footnotes

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¹By nonconfirmatory methods, I mean those methods of factor analysis that do not use maximum-likelihood estimation procedures to test the validity of a prior structural model. Confirmatory methods have come into widespread use only during the past decade or so (see, e.g., Jöreskog, 1970). These methods, I believe, can be highly useful in hypothesis testing.

²The distinguishability of alternative subparadigms is often hazy, especially since it is possible to combine subparadigms within a single study. Hence, some of the criticisms directed at use of one particular paradigm may apply to another in certain instances, whereas others of the criticisms directed at that paradigm may not apply at all in certain instances.

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No.	Name	Published Reference
1	<u>Intelligence Research at the Interface between Differential and Cognitive Psychology.</u> January, 1978.	Sternberg, R. J. Intelligence research at the interface between differential and cognitive psychology. <u>Intelligence</u> , 1978, 2, 195-222.
2	<u>Isolating the Components of Intelligence.</u> January, 1978.	Sternberg, R. J. Isolating the component of intelligence. <u>Intelligence</u> , 1978, 2, 117-128.
3	<u>Deductive Reasoning.</u> January, 1978.	Sternberg, R. J., Guyote, M. J., & Turner, M. E. Deductive reasoning. In R. E. Snow, P.-A. Federico, & W. Montague (Eds.), <u>Ability, learning and instruction: Cognitive process analysis</u> (Vol. 1). Hillsdale, N.J.: Erlbaum, 1980.
4	<u>Toward a Unified Componential Theory of Human Reasoning.</u> April, 1978.	Sternberg, R. J. Toward a unified componential theory of human intelligence: I. Fluid ability. In M. Friedman, J. Das, & N. O'Connor (Eds.), <u>Intelligence and learning</u> . New York: Plenum, 1980.
5	<u>A Transitive-Chain Theory of Syllogistic Reasoning.</u> April, 1978.	UNPUBLISHED TO DATE
6	<u>Components of Syllogistic Reasoning.</u> April, 1978.	Sternberg, R. J., & Turner, M. E. Components of syllogistic reasoning. <u>Acta Psychologica</u> , in press.
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