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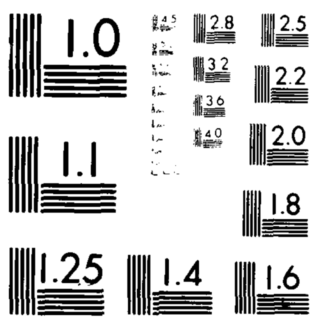

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Claims, Counterclaims, and Components:

A Countercritique of Componential Analysis

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Technical Report No. 25
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Although the particular issues addressed are those raised by the commentators, the present article can be read in the absence of both the original article and the commentaries, since the points raised are (I believe) worthy of response in their own right.

Claims, Counterclaims, and Components:

A Countercritique of Componential Analysis

Robert J. Sternberg

Yale University

A reply to commentaries on "Sketch of a Componential Theory of Human Intelligence," Behavioral and Brain Sciences, in press

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Running head: Claims & Counterclaims

Abstract

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Claims, Counterclaims, and Components:

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Just a few years ago--during the mid seventies--a vigorous debate arose regarding the preferred method for studying the nature of human intelligence. On one side were the "conservative" differential psychologists, claiming that "factor analysis is alive and well" (Royce, 1980). On the other side were the self-proclaimed "progressive" information-processing psychologists, claiming that factor analysis had not lived up to its original promise, and that although differential methods such as factor analysis had a place in the study of intelligence, they had to be combined with the explicit study of human information processing (see, e.g., Hunt, Frost, & Lunneborg. 1973; Sternberg, 1977). Within a brief period of time, the points of debate became progressively less clear, and the demarcations separating differential psychologists, on the one hand, and information-processing psychologists, on the other, became downright fuzzy (Sternberg, in press). Some psychologists, such as Frederiksen (1980) and Whitely (1980), began using information processing analysis in the context of analysis of covariance structures (in the case of Frederiksen) or latent trait analysis (in the case of Whitely), and even what had seemed like a fairly sharp distinction between factorial and componential units of analysis seemed not to hold up under closer examination (Sternberg, in press; see also commentaries of CARROLL, KLINE, and PELLEGRINO). I view the debate as having been largely defused by the recognition of all parties to it that each side shares the concerns of the other, and that if one puts methodological preferences aside, the goals of the two enterprises are quite similar. Even if one does not put methodological preferences aside, there seems to be a growing realization that there is no one preferred method: Differential and information-processing analyses complement each other and should ultimately lead to converging conclusions regarding the nature of intelligence.

As this first debate began to lose its fire, another debate started to heat up. The "progressives" took to quibbling among themselves, disputing, for example, whether the "cognitive correlates" method or the "cognitive components" method was the preferred integration of psychometric and information-processing techniques for studying intelligence (see, e.g., Pellegrino & Glaser, 1979). But this debate also had a brief life span. As MACLEOD has pointed out in his commentary and as I have noticed as well in trying to explain the difference between the two approaches to graduate students, the demarcation between the two approaches is a fuzzy one at best. In practice, many research projects defy classification as belonging in one camp or the other (e.g., MacLeod, Hunt, & Mathews, 1978). These two approaches, like the differential and information-processing approaches that spawned them, seem complementary rather than conflicting.

I entered a period where I was concerned that the new areas of agreement everyone seemed to be finding would diminish the enthusiasm that had been responsible at least in part for the resurgence of interest in intelligence during the past decade. The level of mutual support of colleagues of divergent views seemed to me to have become so high that I began to wonder whether the time had not come for some new generation of investigators to show us we were all wrong. Whether or not this time has come, my worries about becoming a member of a placid "mutual admiration society" ended when I read the commentaries on my article. Apparently, plenty of hot disagreements remain. Indeed, the level of disagreement among the commentators with my point of view seemed to be equaled only by their level of disagreement with each other. I shall organize the remainder of my response around what I see as the main areas of disagreement between my commentators and myself.

The Nature of Intelligence

1. The article does not take sufficient heed of the cultural relativity of human intelligence. According to BERRY, "there is a great need to keep the question of how cognition is structured in different populations an open one." BERRY believes there is no support for the position that the components of human intelligence or the ways in which they are organized are universal across cultures. GUILFORD reads the evidence in a different way, agreeing with me "that the basic intellectual abilities or functions are the same in all cultures, and there is much evidence [for this position] from factor-analytic studies."

The position I have taken--that the components of human intelligence are the same across cultures but that their weights differ--seems to me to be a most reasonable one in light of information-processing analyses of task performance across cultures (e.g., Cole, Gay, Glick, & Sharp, 1971; Cole & Scribner, 1974). A key aspect of intelligence - perhaps the key aspect - is adaptation to one's environment. Certainly, the requirements for adaptation will differ across cultures, and even differ within cultures. Behaviors that lead to successful adaptation in one culture might even lead to unsuccessful adaptation in another culture. But on the face of it, I find it hard to accept that inhabitants of all cultures would not at some time have to, say, (a) infer what is common to two or more entities (a performance component), (b) apply to new situations information that has been previously inferred (another performance component), (c) decide upon the strategy one should use to accomplish a particular task (a metacomponent), or (d) monitor one's progress in reaching a goal in order to determine whether the strategy one has adopted is working successfully (another metacomponent). The frequency or importance of such activities may be variable, but the need for them seems to me to be part of what is distinctively "intelligent" about human beings. The cross-cultural generalizability of components such as these is, of course, testable, and I would hope that it would be tested in the near future. (Marshall Segall has indicated to me plans to do some empirical work of this kind.)

2. I wrongly accept the notion of a "general ability" (*g*). GUILFORD and ROYCE have taken issue with my acceptance of the existence of a general ability; CARROLL has expressed some skepticism regarding the kinds of evidence I have adduced to support the existence of *g*, but has not taken issue with my claim for its existence. Several other commentators, on the other hand, have agreed that a general ability exists, e.g., BUTTERFIELD and LANSDELL. LANSDELL has gone further and argued that GUILFORD'S evidence against the existence of *g*--the high number of near-zero correlations in his test data--is flawed in that the data were collected from "groups homogeneous with regard to education, age and sex...; it would be difficult to find groups more likely to provide some near zero correlations."

I have no bones to pick with CARROLL: As Humphreys (1979) and many others (including CARROLL) have pointed out, evidence supporting the existence of *g* comes from a large variety of sources; some sources are undoubtedly more persuasive to some people than others. I am less sympathetic with GUILFORD'S point of view. In the first place, the powerful demonstrations of Horn (1967) and Horn and Knapp (1974) lead me to be extremely chary in drawing any psychological inferences at all from the procrustean rotations GUILFORD has used to support the structure-of-intellect model. In the second place, if GUILFORD is indeed prepared to start extracting higher-order factors, as he indicates he now is, then I would venture to guess that he will find himself in the same position Thurstone found himself in when he started extracting higher-order factors: Successive factoring of higher-order factors eventually will lead (as it did for Thurstone) to a general factor. Indeed, extraction of higher-order factors is one method CARROLL recommends for extracting a general factor.

ROYCE claims that my arguments for _g_ "won't wash in the factor analytic context for the simple reason that such a claim would require that _every_ measure of intelligence have a significant loading on this factor--and there has been no such empirical demonstration!" I agree there hasn't been. One could spend one's whole life attempting to do factor analyses of all of the intelligence subtests that have been (or might be) proposed. Such a demonstration would clearly take forever, as new tests are being invented every day. But the evidence from factor-analytic studies is, in my opinion, persuasive in arguing for a general factor; and information-processing considerations argue for it as well. Certain performance components of information processing, such as encoding and response, seem almost necessarily to be general across all but the most artificially contrived tasks, and metacomponents such as planning a strategy or choosing a representation for information to be processed also seem almost necessarily to be general in task performance. If these components are indeed general across tasks, then they should generate constant sources of individual differences that result in the appearance of a general factor.

3. *The pattern and importance of interacting components should change with age.* I agree with this point of BORKOWSKI: This view is perfectly consistent with that I present in Item 6 of the section of my article on relations between components and human intelligence: At different ages, different aspects of the interactive model may be more important than others. (See also Sternberg & Powell [in press] for a more detailed expression of this point of view.)

4. *There is an overemphasis on analogical thinking in my approach.* "It is far from representative of the whole of intelligence," according to GUILFORD. I have used analogies (but not only analogies!) as example problems in this and other articles because of their relative simplicity, their appeal to a variety of audiences, and their importance in many theories of intelligence. My collaborators and I have studied a number of other kinds of problems that we believe to be important in intelligent functioning, however, and I believe that even a fairly cursory review of my past and present research will show that analogies occupy no privileged position. (See, for example, our studies of linear syllogistic reasoning [Sternberg, 1980b, 1980d, 1980e]; categorical and conditional syllogistic reasoning [Guyote & Sternberg, 1978;Sternberg, Guyote, & Turner, 1980; Sternberg & Turner, in press]; metaphorical comprehension and appreciation [Sternberg, Tourangeau, & Nigro, 1979; Tourangeau & Sternberg, in press]; causal inference [Schustack & Sternberg, in press]; classificational and serial reasoning [Sternberg & Gardner, 1979]; and verbal comprehension [research by Powell & Sternberg described in Sternberg, 1979b]).

The Componential Approach to Intelligence

5. *The article presents a metatheory or framework for a theory, rather than a theory itself.* Variations on this theme were noted by BRODY, COLLINS, HUNT, KEATING, and ROYCE. I think the point is essentially well-taken. In the past, I have referred to "componential metatheory" (e.g., Sternberg, 1977), and to a large extent, the present article represents an expansion of that metatheory. For the most part, the content of the theory is provided in the papers cited in Item 4 above, as well as in numerous other papers I have published. Again, even a cursory review of the literature will reveal considerable detail regarding the state of the theory (see, especially, Sternberg, 1980f). The article is not entirely metatheoretical, however. As HUNT correctly points out, metatheory is not in itself directly disconfirmable. For example, it is not clear to me what it would mean to disconfirm the component (factor, or any other unit) as a convenient unit of analysis. But many aspects of the presentation are disconfirmable, and hence theoretical rather than metatheoretical. One could show that the particular performance components or metacomponents I identified are not, in fact, isolatable processes, or that the sources of difficulty Janet Powell and I have proposed affect the ease of execution of acquisition, transfer, and retention do not in fact affect acquisition, transfer, or retention. One could show that tasks do not conform to the hierarchical model of component generality I proposed, or that metacomponents can account for virtually all of the stimulus variation in learning or recall experiments (as proposed by BUTTERFIELD), and hence that the postulation of acquisition, transfer, and retention components is unnecessary. The list of disconfirmable empirical claims could go on much longer. To conclude, the article is a mixture of metatheory and theory, a point I would have done well to make in the original article rather than in this one.

6. <u>The proposed componential theory can account for too much data</u>. KEATING suggests that the componential theory is too powerful: "Any theory which can easily adapt itself to S-R bonding or elementary information processing, Spearman's <u>g</u> or Guilford's SOI, Jensen's inherited capacity for learning or Cole's enculturation perspective, is either brilliantly integrative or insufficiently specific. Until convinced otherwise by substantial evidence, I suspect the latter." Naturally, I was hoping KEATING would suspect the former, but you can't win them all. KEATING'S viewpoint is in contrast to those of FREDERIKSEN and HUNT, both of whom who suggest alternative models that they believe can account for data my componential perspective cannot account for.

I agree with KEATING that the sketch as it now stands is in need of what KEATING refers to as "defining details." I say as much in the second paragraph of my target article. I present as the first of two caveats the explicit warning that "the article presents a sketch, not a finished product. Some of the proposals are clear and reasonably well-articulated; others are fuzzy and in need of further articulation. Some of the proposals have solid empirical backing from my own laboratory or the laboratories of others; other proposals have only the most meager empirical backing, or none at all." Particular aspects of the theory--accounts of intelligent information processing in key tasks such as analogies, series completions, linear syllogisms, and the like--have been presented in great detail, but the structure into which these various accounts fit is, as both KEATING and I recognize, in need of further articulation.

I am much less concerned than is KEATING about the ability of the theory to account for seemingly disparate findings. Indeed, I view as a major contribution of the componential perspective its ability to reconcile seemingly conflicting views. I would argue strongly that the alternative psychometric theories are compatible, that these theories are compatible with information-processing accounts of intelligence, and that even stimulus-response accounts of behavior can be integrated into this global perspective if one is willing to go beyond stimuli and responses in accounting for the mental events that intervene between them. Moreover, I am quite convinced that both inheritance and enculturation play important roles in the development of intelligence. Although Jensen's and Cole's emphases are different, certainly a full understanding of intelligence would require some elements from both of their perspectives. Of course, one does not want a theory that can account for absolutely anything, implausible though it may be. But anyone who has actually tried componential modeling of task performance, and who has tried relating models across tasks, will find out that this is the least of their worries. At the more global level, it would be rather easy to disconfirm the hierarchical structure for interrelating tasks that I have proposed (see also Sternberg, 1979a), and as FREDERIKSEN has pointed out, there exist viable alternatives to my functional system for interrelating different kinds of components (e.g., Rumelhart, 1977).

7. I never say what a "subtheory" is. As pointed out by ROYCE, I don't say what a "subtheory" is. As a "subset" is a portion of a complete set, a "subtheory" is a portion of a complete theory. The proposed theory is, as I state in the article, incomplete, and hence I refer to it as a "subtheory." A full theory of intelligence would have to take into account many noncognitive variables as well as cognitive ones that I do not consider.

8. Classification of components by level of generality is a truism of little interest. I agree with HUNT that division of components into general,

class, and specific levels in itself adds little to our knowledge, although I suspect GUILFORD and ROYCE would claim that such a classification is not a truism at all. They seem not likely to accept the existence of general components. PELLEGRINO, unlike HUNT, seemed to find the classification scheme an attractive one, as did I, for the reasons PELLEGRINO seems to recognize: first, that it is useful to know how generalizable various components are across cognitive tasks, since nongeneralizable ones are of little interest; second, that this classification provides one means of tying in the component construct with the factorial one; and third, that the hierarchical scheme I proposed for interrelating tasks can show, at least potentially, how tasks are related to each other in terms of their componential structure. This scheme, as I mentioned above, is disconfirmable, and hence certainly does not represent a tautological organization for information.

The Notion of the "Component"

The Nature of Components

9. It is not clear what a component is, or what limits the number of possible components. KEATING would like to know what, exactly, a component is, whether my sketch presents a preliminary or an exhaustive list of components, and how one can avert the spectre of an ever-growing "laundry list" of components. First, a component is a process. Although one's knowledge base can affect the components one uses in performing a task, and although the components one uses in performing a task can affect one's knowledge base, I think that some distinction between the two is worth maintaining. Second, the list of components I have presented is certainly a preliminary one. I have investigated only a small subset of the tasks that could be reasonably considered as requiring intelligent behavior, and although such tasks show overlaps in the components that tend to be used in their performance, there

are certainly components I have not dealt with (e.g., ones used in spatial visualization tasks) that should be included in a complete theory of intelligence. Finally, the best protection against an ever-growing laundry list of components is the cleverness of evolution in equipping us with a relatively small number of mechanisms to do a relatively large number of things. I have found that the number of components one finds in task performance increases at a decreasing rate with the number of tasks one studies, because components do overlap across tasks (as discussed in more detail later). The task hierarchy described in my article shows how even large increases in the number of tasks studied tends to result in only small increases in numbers of components: The number of general components stays the same; the number of class components increases, but slowly, because of overlaps in such components; and although the number of specific components increases linearly, this number is of little interest, since specific components are of themselves of little interest. Hence, the list of components does not in fact seem to expand endlessly (see Sternberg, 1979a, 1980f).

10. Components are essentially unanalyzed "black boxes." This point, made by COLLINS and HUNT, has also been made in a previous review of my work (Pellegrino & Lyon, 1979), and also responded to in my reply to that review (Sternberg, 1980a).

The level of analysis one chooses to label "elementary" is essentially a matter of theoretical or practical expediency. There is no one "elementary" level. My collaborators and I have found our level of analysis both theoretically useful and practically useful in training reasoning processes (e.g., Sternberg, Ketron, & Powell, in press; Sternberg & Weil, 1980). Moreover, the number of components identified in each task is usually intermediate between a number so small that it gives little insight into the processes a person uses (e.g., one or two) and a number so large that it fragments performance beyond useful bounds (e.g., twenty or thirty). We believe, however, that components can always be further subdivided into subcomponents, which are in turn elementary components at a more fine-grained level of analysis. Similarly, they can be combined into components at a coarser level of analysis. In a sense, the "black box" argument leads to an infinite regress, because (on our view) components can be subdivided indefinitely. I certainly agree, however, that it is important to discover as much as possible about what happens at a psychological level when each component, at any level of analysis, is executed.

11. Labeling a control process as a metacomponent does not get around the problem of identifying the control process (or metacomponent). This point, made by PELLEGRINO, is certainly correct, although I don't see its applicability to my own article. In the article, I identified six metacomponents (and there are certainly others I have not identified), and explained their possible workings in task performance.

12. There are too many kinds of components in the proposed framework. BUTTERFIELD, DETTERMAN, KEATING, and MACLEOD all question the need for so many kinds of components. BUTTERFIELD and MACLEOD query whether it is necessary to postulate separate components of acquisition, retention, and transfer. BUTTERFIELD, for example, believes that the work of these kinds of components can be done by metacomponents. DETTERMAN, in contrast, questions the need for metacomponents, and seemingly suggests that performance components alone could do the job. Disagreeing with all three of these authors, COLLINS and GUILFORD find the typology of components too parsimonious, and suggest that more kinds of components would be needed in a complete account of human

information processing. My own sympathies run more with the latter two authors than with the former three. I suspect that my typology does not do justice to the full complexity of the human information processing system. Nevertheless, we are faced with empirical questions these commentators raise as to what components of information precessing are used in performing various information processing tasks; whether, in particular, metacomponents such as the six I proposed can account for performance, say, in a learning task; and whether performance components could operate in the absence of metacomponents (control processes, or whatever) of some kind. I find this last contention implausible, but debates without evidence will not resolve these questions.

There is a certain kind of question that is not empirically answerable. This question deals with the labels and numbers of labels one wishes to assign as higher-order categories for classifying components. Most of the disagreement among authors actually seems to center around this issue, which I believe to be essentially a matter of taste. One formulates categories that are heuristically useful and explanatorily powerful. I do not claim any kind of priviliged status for my own taxonomy, but I do find it heuristically useful and potentially, at least, explanatorily powerful.

13. The cross-situational identifiability of components has yet to be shown. This point, made by FREDERIKSEN, is similar to one made by Pellegrino and Lyon (1979) in an earlier review of my work. In fact, my collaborators and I have been quite concerned with cross-situational identifiability of parameters, and have attempted to show such identifiability in some of our work. (See Sternberg, 1979a, 1980a, 1980f, for further general discussion of this issue.) For example, Guyote and Sternberg (1978) wanted

to show that the same components are involved in the solution of categorical syllogisms (such as "All A are B. x is an A. Can one conclude that x is a B?") and conditional syllogisms (such as "If A then B. A. Can one conclude, B?") Categorical and conditional syllogisms were constructed and paired so that the same processes, representations, and contents were theorized to be used in each of these two items occurring in each matched pair. Various tests suggested that the theory was plausible. The mean proportion of categorical syllogisms solved correctly was .82; the mean for the conditionals was .83. The mean latency for solving the categorical syllogisms was 13.38 seconds; the mean for the conditionals was 13.51 seconds. The correlation of response choices across pairs to items was .97, whereas the reliability of each set of response choices was .98, only .01 higher than the correlation between response choices. The same model fit performance on both kinds of items. For response choices, this model fit the data for the categorical syllogisms with an R^2 of .97 and an RMSD of .07; for the conditional syllogisms, the corresponding values were .95 and .10. For response times, this model fit the data for the categorical syllogisms with an R^2 of .88 and an RMSD of .25; for the conditional syllogisms, the corresponding fits were .84 and .28. The correlation of parameter estimates across the two-item types was .92 for the response-choice parameters and .99 for the response-time parameters. Sternberg and Gardner (1979) conducted similar kinds of analyses to show the cross-task generalizability of parameters of response choice and response latency in analogies, series completions, and classifications. They, too, showed generality of components across tasks. Hence, I do not believe identifiability across tasks to be the problem FREDERIKSEN seems to think it is.

14. The proposed framework assigns too much importance to the metacomponents. Although I chose to highlight this point of BORKOWSKI, I might just as easily have highlighted the opposite point of BUTTERFIELD, namely, that I don't assign the metacomponents enough importance (since they are alleged by BUTTERFIELD to be capable of subsuming the functions of the acquisition, transfer, and retention components). I do believe, with BUTTERFIELD, that metacomponents (called by whatever name one likes) form the core of an intelligent information-processing system. In most kinds of models of intelligent functioning systems, there is some kind of executive that controls the workings of various kinds of subordinate functions. A very large array of evidence in the domains of human memory, reasoning, and problem solving is consistent with this point of view, and I am prepared to accept it, at least for the time being, and until I am shown that an executive is either unimportant or nonexistent.

15. The label "retention" as applied to "retention components" is inapt, as retention is a state rather than a process. My view is that retention can be either a state or a process. Rehearsal in order to retain information would seem to be an example of a "retention" process. I agree with GUILFORD, however, that there is a potential confusion here, and perhaps the label "retrieval components" would have served better.

16. The importance of training components and the strategies into which they enter should not be overlooked. This point, made by MACLEOD, seems to have been intended as supplementation to rather than criticism of the article. Because I believe it is so important, I am repeating it here. We have in fact done several training studies that were not discussed in the article for lack of space and seeming direct applicability (see Sternberg,

Ketron, & Powell, in press, for a review), and we are presently preparing two large-scale componential training studies to be conducted in Venezuela.

Identification of Components

17. How one identifies a factor is clearly specified; how one identifies a component is not. Variants of this basic criticism are made by BUTTERFIELD, HUNT, and PELLEGRINO. There seem to be two senses of the criticism, neither of which I believe to be justified. The first, seemingly intended by BUTTERFIELD and PELLEGRINO, is that the analytic mechanisms for extracting components are not well specified. It is true that my article did not describe these analytic mechanisms in detail, but then, the article was never intended to be a methodological one. A variety of analytic mechanisms for extracting performance components are described in some detail in a previous methodological article (Sternberg, 1978b), and details of specific procedures for extracting performance components are stated as well in some of my original reports of experimental findings (e.g., Sternberg, 1977, 1980e; Sternberg & Rifkin, 1979; Sternberg & Nigro, 1980). Methods for extracting acquisition, transfer, and retention components are less well worked out, although they are described at a superficial level in Sternberg (1979b). My point here is simply that some of my previous articles provide (sometimes excruciating) details regarding procedures for extracting components. Such details would not have been of interest in the present article.

The second sense of the criticism, seemingly intended by HUNT, is that there are no fixed ways either for identifying the components that are employed in a given task, or for guaranteeing the isolation of these components once they have been identified. In factor analysis, on the other hand, one need not have previously identified the requisite factors in order to isolate them.

First of all, I see nonconfirmatory forms of factor analysis (the kinds to which HUNT apparently refers) as serving quite a different purpose from componential analysis. I believe, with Humphreys (1962), that factor analysis is a "useful tool in hypothesis formation rather than hypothesis testing" (p. 475). Nonconfirmatory factor analysis is not useful for hypothesis testing because the inferential statistical machinery is weak, and because the rotation problem renders unique orientations of factorial solutions an impossibility. Componential analysis, in contrast, is useful as a method of hypothesis testing. The inferential machinery is strong (see Sternberg, 1978b, in press), and there is no rotation problem. Componential analysis can also be very useful in hypothesis reformulation. I remember few instance in which my initial theory of task performance was correct; I needed analysis of subject protocols, residuals, and the like, to enable me to reformulate my theories in a psychologically more plausible way. One must go into componential analysis with some initial theory, of whatever degree of plausibility. Such a theory may have been generated on the basis of logical task analysis, subject protocols, previous theories of the same or other tasks, intuitions, or the like. Once this theory is tested, it is possible (and, I have found, not very difficult) to bootstrap one's way to a better theory, and then to cross-validate this theory on a subsequent sample of subjects and tasks.

Second of all, I find in research that what one gets out of a data-analytic procedure tends to be largely a function of what one puts into it. If one starts without good ideas, one often ends up without good ideas. Componential analysis, unlike factor analysis, cannot be used (and hence misused) by anyone who knows how to get a computer program to run, nor by just anyone who knows how to correlate scores from laboratory tasks with scores from psychometric tests. Use of componential analysis requires psychological insights and the ability to translate these insights into quantitative statements. But maybe these requirements will spare us the chaff that has accompanied the wheat in the literature employing factor analysis, and in the literatures of some contemporary approaches that can be used in the complete absence of any psychological insights.

18. *Componential analysis has been more successful in dealing with component durations than with component difficulties or probabilities of execution*. I would reply to CARROLL'S criticism in two ways. First, we have attempted to measure probabilities of component execution in only two sets of experiments (including six experiments in all), those dealing with our transitive-chain theory of syllogistic reasoning. These were the only experiments in which measurement of probability of component execution seemed relevant, since the transitive-chain theory, unlike other theories my colleagues and I have proposed, is a stochastic one. In these experiments (Guyote & Sternberg, 1978; Sternberg & Turner, in press; see also Sternberg, Guyote, & Turner, 1980), fits of the transitive-chain theory to response-choice data were as high as in any of our experiments in which we have attempted to fit deterministic models to latency data. Values of R^2 between predicted and observed response probabilities, for example, were generally in the .90s. In an experiment modeling response choices in analogical, serial, and classificational reasoning, where a single exponential parameter was fit to the data, values of R^2 were also in the .90s. I am therefore not at all clear as to why CARROLL believes that models incorporating probabilities of response execution have been less successful than latency models, unless he is unaware of these experimental data (which are, however, the only relevant ones with regard to probabilities of component execution). Second, we have attempted to estimate difficulties of component execution in a number of experiments. With one exception (the verbal analogies experiment in Sternberg [1977), model fitting has been quite successful, and values of R^2 between predicted and observed values have been largely a function of the variance across item types in error rates. This functional relationship is not, in itself, surprising. In most of our experiments, error rates have been extremely low (usually less that 5%, and almost always less than 10%), so that there has been hardly any variance to account for in the data! In experiments where there has been more variation in error rates, e.g., a study of the development of verbal analogical reasoning (Sternberg & Nigro, 1980), correlations between model predictions and error rates have been almost as high as correlations between model predictions and latencies. Mulholland, Pellegrino, and Glaser (in press) have used what is essentially a componential modeling procedure to predict error rates in geometric analogy solution, and they, too, had sufficiently high error rates to obtain very high levels of correspondence between predicted and observed values. The general point is that the generally lesser model fits for error rates than latencies seem to reflect the tasks most often chosen for analysis rather than any intrinsic feature of componential modeling. If tasks are studied with nontrivial levels of mistaken responses (and such tasks have been studied!), then levels of model fits for error rates are not much different from levels of model fits for latencies (see also Sternberg, 1980d, for a discussion of why it is of key importance to model error rates as well as latencies.)

19. The probability of the use of a component is a property of the person, not of the task. HUNT believes my characterization of components as having a probability of execution in a particular task is "surely...in error: the probability of use must be a property of the user and not of the thing used." Actually, probability distributions apply both over stimulus types and over persons, despite the fact that probability parameters are usually estimated over persons. If, for example, half of the linear syllogisms in a complete set of stimulus items contain negations in the premises, then the probability of executing operations associated with comprehension of the negations is .5 over stimuli, without regard to what it is over persons. Componential analyses should take into account probability distributions across both persons and tasks.

The Knowledge Base upon which Components Operate

20. The proposed framework understates the importance of the knowledge base upon which components operate. According to BORKOWSKI, "the core of Sternberg's model does not leave room for knowledge states, especially knowledge about metacomponents." It is true, as BORKOWSKI states, that "control processes or components never operate in a vacuum," but I can't imagine why he thought I believed they did. My interpretation of the proposed system is in line with that of BUTTERFIELD, who notes that "Sternberg emphasized the importance of the informational content and context, and of previous learnings when explaining acquisition." Indeed, I felt the need for adding acquisition, retention, and transfer components to my taxonomy of types of components because of my belief that previous statements of my theory (or metatheory, as some prefer) insufficiently emphasized the role of knowledge in intelligent fuctioning. In my article, I give numerous examples of why, as BORKOWSKI puts it, "the 'state' of the knowledge system at any given moment helps determine the level and type of processing of basic components." In discussing, for example, how components interact with one's

knowledge base in the solution of anagrams, I note that "as a given strategy is being executed, new information is being acquired about how to solve anagrams, in general. This information is also fed back to the metacomponents, which may act upon or ignore this information." In discussing, for a second example, why vocabulary is such an excellent predictor of measured intelligence, I note that "lack of knowledge can block successful execution of performance components needed for intelligent functioning.... Thus, vocabulary is not only affected by operations of components, but affects their operations as well. If one grows up in a household that encourages exposure to words..., then one's vocabulary may well be greater, which in turn may lead to superior learning and performance on other kinds of tasks that require vocabulary." For a third example, I note in my discussion of creativity that many previous attempts to understand the nature of creativity may have failed because of overemphasis upon fluid abilities, and underemphasis upon the knowledge base. "Creativity, on the componential view, is due largely to the occurrence of transfer between items of knowledge (facts or ideas) that are not related to each other in an obvious way." In short, the knowledge base of the individual plays an extremely important role in the present formulation, and enters into task performance of every conceivable kind. I agree with BORKOWSKI'S point, but believe his characterization of my position is incorrect. Although the theory does not have knowledge as its object, it does take knowledge into account.

Componential Analysis Compared to Alternative Paradigms

Componential Analysis and Factor Analysis

21. Components and properly rotated factors are essentially the same thing. According to KLINE, "factors when properly rotated emerge which are

equivalent to the processes experimentally defined by cognitive psychology." I strongly agree with KLINE (and CARROLL and PELLEGRINO) that components and factors are complementary (see Sternberg, in press, for an exposition of this point of view); but components are not identical to factors. Components are processes with real-time durations, probabilities of execution, and probabilities of being executed correctly. Factors share none of these characteristics. A factor is a hypothetical source of individual differences identified (in most investigations) through patterns of individual-difference variation. Components are processes identified through patterns of stimulus variation. Whereas factors depend for their identification upon the existence of individual differences, components could be identified in the absence of such differences. The sources of individual differences that can generate factors are endless. They include processes, of course, but also content, form of mental representation, response format, modality of stimulus presentation, and the like. Multiple sources of individual differences may combine into single factors, as when a particular process acts upon a particular representation, and the process-representation pair gives rise to a unitary source of individual differences in a given analysis. The strengths and weaknesses of components and factors are different ones (see Sternberg, in press), and these differences make both useful in the study of intelligence. But whatever the exact nature of the relationship between components and factors, it is not one of identity.

22. *One can characterize components and factors in terms of their relative depth of level of analysis*. Which are more basic, components or factors? When I wrote my 1977 book, I made the mistake of stating or at least strongly implying that components were in some sense more "basic" than factors. I proposed to characterize factors in terms of the components

that underlay them, but I did not believe it made sense to factor analyze components, since the former were more basic, and since one then ended up with the seemingly ugly specter of an infinite regress in which one componentially analyzes factors, factor analyzes components, componentially analyzes the new factors, and so on. COLLINS has characterized components as "at about the same level of analysis as the units in factor-analytic theories of intelligence." And CARROLL seems to be claiming that factors are at a deeper level of analysis than components--indeed, that a factor analysis of components tells one about the "underlying sources of individual differences." He proposes that "component scores...may be entered into a factor analysis, along with scores on appropriate reference ability tests. The selection of tasks and reference ability tests can reflect hypotheses as to the nature of the source traits," which Carroll believes to be represented by factors.

I now believe that the debate as to which is more basic--the factor or the component--is about as fruitful as the debate as to which came first--the chicken or the egg. First of all, it is not even clear what it means, psychologically, for one unit to be "more basic" than another. This notion can have a clear meaning in some contexts. Certain psycholinguists, for example, refer to "deep structures" as more basic than "surface structures" because, on the standard theory, surface structures are translated into deep structures. But I doubt anyone would claim that at a psychological level, components are translated into factors, or factors into components. Second of all, we don't really have any empirical means of determining which unit, the component or the factor, is more basic. It is sometimes possible to perform data manipulations in the absence of a theory--indeed, factor analysis provides a classic case of such a possibility. But one can regress factor scores on component scores or component scores on factor scores, so that the statistical means exists to account for either unit in terms of the other. And, of course, it is possible to factor analyze any set of data for which one can obtain a correlation matrix (such as component scores), although the possibility of doing a factor analysis does not guarantee the psychological meaningfulness of the results, any more than the possibility of doing a componential analysis guarantees the meaningfulness of the results. In his early articles on individual differences, HUNT (Hunt, Frost, & Lunneborg, 1973; Hunt, Lunneborg, & Lewis, 1975) seemed to be comfortable with what I now perceive to be an indeterminacy of the "basic" unit, if, indeed, there is such a unit. I believe that at least for the time being, the rest of us would do well to join him.

23. Factor analysis provides an objective method for testing hypotheses about components because the machinery of factor analysis is not dependent upon prior hypotheses. CARROLL proposes factor analysis as an objective way of testing hypotheses about the factorial composition of component scores. I reject this proposal. First, the inferential statistical machinery of nonconfirmatory factor analysis is so weak that I cannot accept factor analysis as a reasonable way of testing hypotheses about anything (despite its usefulness in generating and exploring the ramifications of hypotheses). Second, it is not clear to me in what meaningful sense factor analysis can be "objective." The solution of a factor analysis can be and usually is radically affected by one's choice of rotation; it may also be affected by the choice of variables to enter, the choice of subjects, the model of analysis (component or common factor), the method used, and so on. In short, numerous decisions affect the final outcome in greater or lesser degree. In my opinion, the intrinsic subjectivity of factor analysis,

combined with the weakness of its inferential machinery, is what led to the stagnation of abilities research in the mid-twentieth century. We should not reject factor analysis because it could not meet the goals some abilities researchers set for it, but neither should be readopt it in still another attempt to meet these goals. Factor analysis can certainly be used in the absence of a well-formulated psychological theory; the lack of a theory is not tantamount to objectivity, however. Moreover, implicit theories are rife in the use of factor analysis, whether or not the investigator is aware of them. Each possible rotation of factorial axes corresponds to a theory, and indeed, the use of factor analysis at all presupposes a theory about the way abilities are constituted. One simply cannot escape the necessity for some kind of theoretically-driven analysis. Given that theory, and concomitant with it, subjectivity, must drive any kind of data analysis (if only by the choice of what analyses one conducts), it is far better to be aware of the theories driving one's analyses, and consciously to make them the best theories of which one is capable.

24. Confirmatory maximum-likelihood factor analyses provide an attractive alternative to componential analysis. FREDERIKSEN suggest that the form of confirmatory analysis he has used, analysis of covariance structures, is an attractive alternative to componential analysis. The main reason he proposes is what he believes to be superior identifiability of parameters. I have discussed this issue earlier. I do not believe identifiability to be a problem in componential analysis. Like FREDERIKSEN, I find confirmatory methods highly attractive, and hence I have no desire to debate the relative merits of these methods versus componential ones. I made the mistake several years ago of attempting to argue for componential methods over nonconfirmatory factorial ones, and have since come to believe, as

FREDERIKSEN does, that "at this juncture, it is important to the development of the science to continue a variety of approaches to the analysis of human abilities." Certainly, confirmatory factor-analytic methods should be among those that we use. If our theories are good, they should be supported by the results of a multiplicity of methods.

Componential Analysis and Information-processing Analysis

25. The article doesn't show how an information-processing approach can help in understanding intelligence. ROYCE believes that the information-processing paradigm has the potential to revolutionize psychology, "but, unfortunately, Sternberg failed to show us how this might occur in the domain of intelligence." He suggests an article of his own as showing how this potentially revolutionary paradigm can be combined with differential psychology to yield potent insights about intelligence (Royce, 1979). I can't say whether or not information-processing psychology has revolutionized the study of intelligence, or whether or not it will do so in the future, but I can point out what I believe to be its major contributions in my own work and in that of numerous others (e.g., BORKOWSKI, BUTTERFIELD, CARROLL, COLLINS, HUNT, MACLEOD, PELLEGRINO, and many others who happened not to be among the commentators on my article).

A first major contribution is in the emphasis upon process in the study of intelligence. Psychometric theorists dating back to Spearman and Thurstone recognized the importance of process in a theory of intelligence, but it was not until the information-processing approach became firmly entrenched in the early nineteen-sixties that information processing in task performance became a central focus of theories of intelligence. Prior to that time, products rather than processes of performance had provided the central focus, and, whatever the potential of factor analysis and other psychometric methods might have been for elucidating process, they just weren't, in practice, telling us much about it. We did not have, for example, powerful ways of discovering even first approximations to the strategies people use in solving items of the kinds routinely found on tests of intelligence.

A second major contribution is in the emphasis upon information and its relationships to process. Processes must always act upon a knowledge base, and understanding of one without the other is not really possible, except, perhaps, of a superficial kind. Information-processing psychology, especially in the seventies, has expressed a serious concern with elucidating the ways in which processes act upon information in order to render an information-processing system "intelligent." This concern is likely to continue into the eighties and beyond.

A third major contribution--the last one I shall discuss--is the emphasis in information-processing psychology upon individual performance. This contribution may sound somewhat paradoxical, in that information-processing psychology has never been noted for its great concern with individual differences, whereas psychometrics, of course, has been. Yet, a great deal of information-processing theorizing has been generated for and tested upon single cases (see Newell & Simon, 1972). By modeling each subject's data individually, it has been possible to study multiple sources of individual differences that would not have appeared if one had analyzed data only at the level of the group. Psychometrics, despite its concern with individual differences, has generated and tested models almost exclusively on group data. Differences elucidated through psychometric analysis of test performance have generally been of a quantitative nature (e.g., relative magnitudes of factor scores) rather than of the qualitative (as well as quantitative) nature elucidated through information-processing analysis of individual cases.

26. The strictly serial execution of components seemingly assumed by componential analysis represents an unlikely model of human information processing. I agree with GUILFORD, HUNT, and LANSDELL, all of whom have stated in different ways that strictly serial execution of components is an unlikely turn of events in many, if not most, information-processing tasks. All information-processing models at the present time--serial and parallel ones alike--can at best be viewed as rough approximations to the undoubtedly more complex strategies subjects actually use. I doubt we are ready yet even to approach the complexity of human thinking in our information-processing accounts. What we are ready for are first-pass accounts, and the relatively simple serial models I have proposed for a variety of different tasks such as analogies and linear syllogisms seem like reasonable places to start. Indeed, at the present time, we do not even have good means for distinguishing these serial models from parallel ones.

It is important to draw a distinction between componential metatheory and componential methodology. Most of the methodologies I have proposed for isolating components assume serial processing in at least some parts of task performance. But componential metatheory is indifferent to whether processing is serial or parallel (see, e.g., Sternberg, 1977, 1979a). Componential models can easily be formulated that are serial, parallel, or a combination of the two.

27. The componential framework cannot handle the kind of division in attentional resources that is required by interfering-task methodologies. This point, made by HUNT, is simply incorrect. Indeed, his own theoretical framework (Hunt, 1978) might be characterized as a componential one, and has

been so characterized by KLINE. I am at a loss as to why he would believe that his framework can handle divided attention but that the one proposed in my article cannot. I note in the article that

> the metacomponents are able to process only a limited amount of information at a given time. In a difficult task, and especially a new and different one, the amount of information being fed back to the metacomponents may exceed their capacity to act upon this information. In this case, the metacomponents become overloaded, and valuable information that cannot be processed may simply be wasted. The total information-handling capacity of the metacomponents of a given system will thus be an important limiting aspect of that system. Similarly, capacity to allocate attentional resources so as to minimize the probability of bottlenecks will be part of what determines the effective capacity of the system.

The very same notions apply a fortiori to divided-attention situations (although I have not studied such situations in my own research). In trying to handle two tasks at once, the capacity of the metacomponents is especially likely to be overtaxed, with resulting loss of efficiency and degradation of task performance.

28. The componential framework cannot handle spreading activation (HUNT). I have not proposed any spreading activation models in my own work, and I am unaware of any other componential theorists who have either. I don't know whether this is because others, like myself, have not found spreading activation models useful in the domains to which componential models have been applied, or because some incompatibility of componential metatheory with spreading activation models insidiously leads componential theorists away from such models. I suspect the former explanation is closer to the truth

than the latter. In any case, at this point in time, the compatibility between spreading activation and componential models has simply not been explored. We have no more basis for evaluating their compatibility at this time than we did some years back for evaluating the compatibility of components and factors. Some investigators, myself included, were too quick to jump to conclusions regarding the degree of compatibility between the two units of analysis, and more generally, the two metatheoretical systems of which they are parts. I believe that the interrelations between various systems should be studied very carefully and slowly before one jumps to conclusions regarding their level of compatibility. We may find ourselves creating divisions between schools of thought and their adherents that do more harm than good, and that have no legitimate basis in fact.

The Contribution of Componential Analysis

29. The proposed theory overemphasizes the role of components in human intelligence. This point seems to be at least implicit in several commentaries, e.g., those of BORKOWSKI and HUNT, although other commentaries, such as those of BRODY and KEATING, see the emphasis upon components primarily as a positive contribution. I wish to reply by restating part of the second caveat in my article, presented in the third paragraph of the text: "Second, the article presents a limited subtheory, not a comprehensive, full theory of intelligence. Even if the proposals were in an approximation to a final form, they would still constitute a subtheory, because there is almost certainly much more to intelligence than is covered by the scope of the present proposals. These proposals do not deal at all with issues of motivation, initiative, and social competence, and they deal only minimally with issues of creativity and generativity...." In short, I fully agree with this criticism. In the context of a complete theory of human intelligence,

the role of components is overemphasized. The subtheory I present deals with those aspects of intelligence that I believe are felicitously understood in terms of a component-based system.

30. It is not clear just what the contribution of componential analysis is. This point has been made in slightly different forms by ROYCE and by PELLEGRINO. ROYCE notes that the contribution of an article such as mine should be either in its working in an established paradigm ("normal science") or in its proposal of a different, nonestablished paradigm ("revolutionary science"). ROYCE concludes that my article really does neither. He therefore despairs in his attempt to answer the question, "Is [Sternberg] doing normal science, or is he giving us a new paradigm?" If I am truly working neither in an established paradigm nor in a non-established one, then I believe my contribution is far greater than any ROYCE might have thought to give me credit for. In this case, philosophers, if not psychologists, should come flocking to New Haven to pay me homage, because I am the first person finally to disconfirm the law of the excluded middle.

PELLEGRINO'S statement that it "is not clear...just what has been gained over current theory by Sternberg's organization of components and by his assignment of various cognitive functions to one or more components" merits a more serious response. What I have called "componential analysis," like other packages of metatheory, theory, and methodology, does build upon current conceptualizations in science. It borrows heavily from both differential and information-processing psychologies, and proposes one possible organization of concepts drawn from these two approaches. I view the main contributions of the system I propose in my article as being these: First, I have proposed what I believe to be a useful typology of components, and a plausible way of integrating these, both with respect to their relative functions and with respect to their relative levels of generality. Second, I have accounted in componential terms for some of the major (albeit tentative) generalizations in the literature on intelligence, showing, in particular, how factor-analytic and componential conceptions of intelligence can be mapped into each other. Finally, I have shown in a fairly large number of theoretically-driven empirical studies that for some purposes, at least, componential analysis works. It has been successful in accounting for performance on a fairly large variety of tasks requiring intelligent performance, and has given us at least some insights into the theoretical interrelations between the psychological performances underlying these tasks. If these contributions are wholly within what PELLEGRINO calls "current theory" (whatever that may be), then I am satisfied that "current theory" and componential analysis are on the right track in helping us understand the nature of intelligence and its manifestations in human behavior.

References

Cole, M., Gay, J., Glick, J. A., & Sharp, D. W. The cultural context of learning and thinking. New York: Basic Books, 1971.

Cole, M., & Scribner, S. Culture and thought: A psychological introduction. New York: Wiley, 1974.

Frederiksen, J. R. Component skills in reading: Measurement of individual differences through chronometric analysis. In R. E. Snow, P.-A. Federico, & W. E. Montague (Eds.), Aptitude, learning, and instruction: Cognitive process analysis. Hillsdale, N.J.: Erlbaum, 1980.

Guyote, M. J., & Sternberg, R. J. A transitive-chain theory of syllogistic reasoning. (NR 150-412 ONR Technical Report No. 5.) New Haven: Department of Psychology, Yale University, 1978.

Horn, J. L. On subjectivity in factor analysis. Educational and Psychological Measurement, 1967, 27, 811-820.

Horn, J. L., & Knapp, J. R. On the subjective character of the empirical base of Guilford's structure-of-intellect model. Psychological Bulletin, 1973, 80, 33-43.

Humphreys, L. G. The organization of human abilities. American Psychologist, 1962, 17, 475-483.

Humphreys, L. G. The construct of general intelligence. Intelligence, 1979, 3, 105-120.

Hunt, E. B. Mechanics of verbal ability. Psychological Review, 1978, 85, 109-130.

Hunt, E. B., Frost, N., & Lunneborg, C. Individual differences in cognition: A new approach to intelligence. In G. Bower (Ed.), The psychology of learning and motivation (Vol. 7). New York: Academic Press, 1973.

Hunt, E. B., Lunneborg, C., & Lewis, J. What does it mean to be high verbal? Cognitive Psychology, 1975, 7, 194-227.

MacLeod, C. M., Hunt, E. B., & Mathews, N. N. Individual differences in the verification of sentence-picture relationships. Journal of Verbal Learning and Verbal Behavior, 1978, 17, 493-507.

Mulholland, T. M., Pellegrino, J. W., & Glaser, R. Components of geometric analogy solution. Cognitive Psychology, in press.

Newell, A., & Simon, H. A. Human problem solving. Englewood Cliffs, N.J.: Prentice-Hall, 1972.

Pellegrino, J. W., & Glaser, R. Cognitive correlates and components in the analysis of indidividual differences. In R. J. Sternberg & D. K. Detterman (Eds.), Human intelligence: Perspectives on its theory and measurement. Norwood, N.J.: Ablex, 1979.

Pellegrino, J. W., & Lyon, D. R. The components of a componential analysis. Intelligence, 1979, 3, 169-186.

Royce, J. R. Toward a viable theory of individual differences. Journal of Personality and Social Psychology, 1979, 37, 1927-1931.

Royce, J. R. Factor analysis is alive and well. American Psychologist, 1980, 35, 390-392.

Rumelhart, D. E. Toward an interactive model of reading. In S. Dornic (Ed.), Attention and performance VI. Hillsdale, N.J.: Erlbaum, 1977.

Schustack, M. W., & Sternberg, R. J. Evaluation of evidence in causal inference. Journal of Experimental Psychology: General, in press.

Sternberg, R. J. Intelligence, information processing, and analogical reasoning: The componential analysis of human abilities. Hillsdale, N.J.: Erlbaum, 1977.

Sternberg, R. J. Isolating the components of intelligence. Intelligence, 1978, 2, 117-128. (a)

Sternberg, R. J. Intelligence research at the interface between differential and cognitive psychology. Intelligence, 1978, 2, 195-222. (b)

Sternberg, R. J. The nature of mental abilities. American Psychologist, 1979, 34, 214-230. (a)

Sternberg, R. J. A review of "Six authors in search of a character:" A play about intelligence tests in the year 2000. In R. J. Sternberg & D. K. Detterman (Eds.), Human intelligence: Perspectives on its theory and measurement. Norwood, N.J.: Ablex, 1979. (b)

Sternberg, R. J. Componentman as vice-president: A reply to Pellegrino and Lyon's analysis of "The components of a componential analysis." Intelligence, 1980, 4, 83-95. (a)

Sternberg, R. J. The development of linear syllogistic reasoning. Journal of Experimental Child Psychology, 1980, 29, 340-356. (b)

Sternberg, R. J. Intelligence and nonentrenchment. (NR 150-412 ONR Technical Report No. 23.) New Haven: Department of Psychology, Yale University, 1980. (c)

Sternberg, R. J. A proposed resolution of curious conflicts in the literature on linear syllogisms. In R. Nickerson (Ed.), Attention and performance VIII. Hillsdale, N.J.: Erlbaum, 1980. (d)

Sternberg, R. J. Representation and process in linear syllogistic reasoning. Journal of Experimental Psychology: General, 1980, 109, 119-159. (e)

Sternberg, R. J. Toward a unified componential theory of human intelligence: I. Fluid ability. In M. Friedman, J. Das, & N. O'Connor (Eds.), Intelligence and learning. New York: Plenum, 1980. (f)

Sternberg, R. J. Factor theories of intelligence are all right almost. Educational Researcher, in press.

Sternberg, R. J., & Gardner, M. K. Unities in inductive reasoning. (NR150-412 ONR Technical Report No. 18.) New Haven: Department of Psychology, Yale University, 1979.

Sternberg, R. J., Guyote, M. J., & Turner, M. E. Deductive reasoning. In R. E. Snow, P.-A. Federico, & W. Montague (Eds.), Aptitude, learning, and instruction: Cognitive process analysis. Hillsdale, N.J.: Erlbaum, 1980.

Sternberg, R. J., Ketron, J. L., & Powell, J. S. Componential approaches to the training of intelligent performance. Intelligence, in press.

Sternberg, R. J., & Nigro, G. Developmental patterns in the solution of verbal analogies. Child Development, 1980, 51, 27-38.

Sternberg, R. J., & Powell, J. S. The development of intelligence. In P. Mussen (Ed.), Carmichael's manual of child psychology. New York: Wiley, in press.

Sternberg, R. J., & Rifkin, B. The development of analogical reasoning processes. Journal of Experimental Child Psychology, 1979, 27, 195-232.

Sternberg, R. J., Tourangeau, R., & Nigro, G. Metaphor, induction, and social policy: The convergence of macroscopic and microscopic views. In A. Ortony (Ed.), Metaphor and thought. New York: Cambridge University Press, 1979.

Sternberg, R. J., & Turner, M. E. Components of syllogistic reasoning. Acta Psychologica, in press.

Sternberg, R. J., & Weil, E. M. An aptitude-strategy interaction in linear syllogistic reasoning. Journal of Educational Psychology, 1980, 72, 226-234.

Tourangeau, R., & Sternberg, R. J. Aptness in metaphor. Cognitive Psychology, in press.

Whitely, S. E. Latent trait models in the study of intelligence. Intelligence, 1980, 4, 97-132.

Footnote

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1	Intelligence Research at the Interface between Differential and Cognitive Psychology. January, 1978.	Sternberg, R. J. Intelligence research at the interface between differential and cognitive psychology. Intelligenc 1978, 2, 195-222.
2	Isolating the Components of Intelligence. January, 1978.	Sternberg, R. J. Isolating the componen of intelligence. Intelligence, 1978, 2, 117-128.
3	Deductive Reasoning. January, 1978.	Sternberg, R. J., Guyote, M. J., & Turner, M. E. Deductive reasoning. In R. E. Snow, P.-A. Federico, & W. Montague (Eds.), Aptitude, learnin and instruction: Cognitive process analysis (Vol. 1). Hillsdale, N.J.: Erlbaum, 1980.
4	Toward a Unified Componential Theory of Human Reasoning. April, 1978.	Sternberg, R. J. Toward a unified compo nential theory of human intelligence: I. Fluid ability. In M. Friedman, J. Das, & N. O'Connor (Eds.), Intelligence and learning. New York: Plenum 1980.
5	A Transitive-Chain Theory of Syllogistic Reasoning. April, 1978.	UNPUBLISHED TO DATE
6	Components of Syllogistic Reasoning. April, 1978.	Sternberg, R. J., & Turner, M. E. Components of syllogistic reasoning. Acta Psychologica, in press.
7	Metaphor, Induction, and Social Policy: The Convergence of Macroscopic and Microscopic Views. April, 1978.	Sternberg, R. J., Tourangeau, R., & Nigro, G. Metaphor, induction, and social policy: The convergence of macroscopic and microscopic views. In A. Ortony (Ed.), Metaphor and thoug New York: Cambridge University Press, 1979.
8	A Proposed Resolution of Curious Conflicts in the Literature on Linear Syllogisms. June, 1978.	Sternberg, R. J. A proposed resolution c curious conflicts in the literature on linear syllogisms. In R. Nickerson (E Attention and performance VIII. Hillsdale, N.J.: Erlbaum, 1980.
9	The Nature of Mental Abilities. June, 1978.	Sternberg, R. J. The nature of mental abilities. American Psychologist, 1979, 34, 214-230.

Technical Reports Presently in this Series

NR 150-412

No.	Name	Published Reference
10	Psychometrics, Mathematical Psychology, and Cognition: Confessions of a Closet Psychometrician. June, 1978.	UNPUBLISHABLE.
11	Understanding and Appreciating Metaphors. June, 1978.	UNPUBLISHED TO DATE.
12.	Representation and Process in Transitive Inference. October, 1978.	Sternberg, R. J. Representation an process in linear syllogistic reasoning. Journal of Experiment Psychology: General, 1980, 109, 119-159.
13	Aptness in Metaphor. October, 1978.	Tourangeau, R., & Sternberg, R. J. Aptness in metaphor. Cognitive Psychology, in press.
14	Contrasting Conceptions of Intelligence and their Educational Implications. November, 1978.	Sternberg, R. J. Factor theories o intelligence are all right almost Educational Researcher, in press.
15	An Aptitude-Strategy Interaction in Linear Syllogistic Reasoning. April, 1979.	Sternberg, R. J., & Weil, E. M. An aptitude-strategy interaction linear syllogistic reasoning. Journal of Educational Psychology, 1980, 72, 226-234.
16	Intelligence Tests in the Year 2000: What Forms will they Take and what Purposes will they Serve? April, 1979.	Sternberg, R. J. Six authors in search of a character: A play about intelligence tests in the year 2000. Intelligence, 1979, 3, 281-291.
17	New Views on IQs: A Silent Revolution of the 70s. April, 1979.	Sternberg, R. J. Stalking the I.Q. quark. Psychology Today, 1979, 13 42-54.
18	Unities in Inductive Reasoning. October, 1979.	UNPUBLISHED TO DATE.
19	Components of Human Intelligence. October, 1979.	Sternberg, R. J. Sketch of a componential subtheory of human intelligence. Behavioral and Brain Sciences, in press.
20	The Construct Validity of Aptitude Tests: An Information-Processing Assessment. October, 1979.	Sternberg, R. J. The construct validi ty of aptitude tests: An information-processing assessment. In

Technical Reports Presently in this Series

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No.	Name	Published Reference
20	(Continued)	A. P. Maslow, R. H. McKillup, & M. Thatcher (Eds.), *Construct validity in psychological measurement*. Princeton: Educational Testing Service, in press.
21	*Evaluation of Evidence in Causal Inference*. October, 1979.	Schustack, M. W., & Sternberg, R. J. Evaluation of evidence in causal inference. *Journal of Experimental Psychology: General*, in press.
22	*Componential Approaches to the Training of Intelligent Performance*. April, 1980.	Sternberg, R. J., Ketron, J. L., & Powell, J. S. Componential approaches to the training of intelligent performance. *Intelligence*, in press.
23	*Intelligence and Nonentrenchment*. April, 1980.	UNPUBLISHED TO DATE.
24	*Reasoning, Problem Solving, and Intelligence*. April, 1980.	Sternberg, R. J. Reasoning, problem solving, and intelligence. In R. J. Sternberg (Ed.) *Handbook of human intelligence*. New York: Cambridge University Press, in press.
25	*Claims, Counterclaims, and Components: A Countercritique of Componential Analysis*. June, 1980.	Sternberg, R. J. Claims, counterclaims, and components: A countercritique of componential analysis. *Behavioral and Brain Sciences*, in press.

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