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HOW SHALL WE STUDY INDIVIDUAL DIFFERENCES IN COGNITIVE ABILITIE--ETC(U)

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HOW SHALL WE STUDY INDIVIDUAL DIFFERENCES IN COGNITIVE ABILITIES?--METHODOLOGICAL AND THEORETICAL PERSPECTIVES

John B. Carroll

University of North Carolina at Chapel Hill

January 1978

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Selected (recent) studies in (the) ~~recent research~~ literature ~~are~~ (were) examined with regard to: (1) their methodological adequacy in establishing dimensions of individual differences in information-processing abilities; and (2) the theoretical problems involved in inferring psychological processes from individual difference dimensions. Many methodological inadequacies ~~are~~ (were) noted, including use of small N's, questionable procedures in establishing variables (e.g., use of raw difference scores), improper or inadequate use of factor analysis and

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SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered) other multivariate statistical techniques, and poor presentation of results. On the whole, little progress has been made thus far in identifying psychological processes through research in individual differences, even though this research approach is viable and potentially useful. Serious theoretical difficulties arise in attempting to infer the nature and operation of psychological processes merely from the identification of individual difference trait dimensions. Promising research, however, is represented by studies in which an effort is made to analyze tasks into their components, to vary task characteristics, and/or to consider the strategies that individuals can employ in performing the tasks.

HOW SHALL WE STUDY INDIVIDUAL DIFFERENCES IN COGNITIVE ABILITIES?--
METHODOLOGICAL AND THEORETICAL PERSPECTIVES

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January 1978

How shall we study individual differences in cognitive abilities?-- Methodological and theoretical perspectives

"How shall we study individual differences?" This was the question considered by S.S. Sargent (1942) in a paper of that title published in the Psychological Review in 1942. It is a question that needs to be asked again and again, for it is one that has no final answer. Certainly Sargent was not the first to have raised it--the problem goes back to the time of Galton, James McKeen Cattell, Edward L. Thorndike and other founders of psychometrics. The particular concerns addressed by Sargent in his 1942 article, however, were somewhat novel at the time, and they are particularly pertinent to our current interest in the analysis of "intelligence" in the light of information-processing theories. Sargent was pointing out that

> ...quantitative approaches do not give an accurate picture of individual differences. ... quantitative treatment, per se, does not describe the methods of work used by a subject as he performs a task;...it does not depict adequately the pattern of behavioral processes involved; [and] preoccupation with quantitative method causes one to lose sight of important aspects of individual personalities and therefore of differences between personalities." (Sargent, 1942, p. 171).

"Information-processing" had not become the catch-phrase it is today, but we can assume that Sargent was thinking of what we now call information-processing when he wrote about "methods of work" and "behavioral processes." And when he mentioned "important aspects of personalities," perhaps he was thinking of what we would now call "cognitive styles." In this paper I will not concern myself with cognitive styles, but I will address the question of how we can get at the methods of work and the behavioral processes that presumably underlie individual differences in cognitive abilities.

Let us remind ourselves that Thurstone, in his use of factor analysis, was much concerned with the explanation of mental abilities in terms of psychological processes. This will be evident from the most casual examination of his attempts to infer the psychological meanings of the factors he identified. But Thurstone was dissatisfied with purely intuitive interpretations of factors. At one point he stated that his preference would be "to head as soon as possible to direct forms of laboratory experiments in terms of which the primary factors may eventually be better understood" (Thurstone, 1940, p. 204).

Within the last few years, psychologists have gone into the laboratory in droves to look at possible relations between mental abilities and variables in the kinds of experimental tasks that are characteristically studied in cognitive psychology. How are these psychologists faring in arriving at a better understanding of mental abilities?

Currently, I am engaged in preparing a review of individual difference research for the 1979 volume of the Annual Review of

Psychology. This work has prompted me to assemble a large parcel of recent psychological research literature pertinent to an information-processing view of mental abilities. Surveying and carefully examining this literature has caused me to conclude that little progress has been made thus far in understanding mental abilities in terms of processes. It can be argued, to be sure, that there has been some success in identifying psychological processes, but the interpretation of these processes often stands or falls depending upon whether one can accept the information-processing models upon which the identification of a particular process is based. Further, the experimental identification of a process often depends chiefly upon the finding of individual differences in the parameters of the process, which in turn are frequently quite specific to that process. This has led, in effect, to the identification of a whole new series of individual "traits" that are little related to the mental abilities isolated in classical psychometric studies. Even if the relations are found to be of substantial magnitude, it is not very revealing or informative merely to establish correspondences between traits and processes that are defined largely on the basis of those traits. There is an obvious danger of circularity in all this.

In what follows, I want to expand this point of view and offer some thoughts on how we can avoid circularity and make independent determinations of processes and individual difference variables. In preparing my review, however, I have been disturbed to discover that information-processing psychologists, and even some psychometrically-oriented psychologists, have in many cases misapplied traditional psychometric methodologies and have drawn inferences and conclusions that I believe are unjustified or at least questionable. Before I discuss problems of studying psychological processes, I aim to clear the ground by offering an extensive critique of the statistical methodologies that are now being utilized in the attempt to study individual differences in mental abilities from an information-processing point of view.

Statistical Methodologies in Individual Differences Research

For many years, statistical methodology in studying individual differences has been essentially correlational, resting usually on bivariate or multivariate linear models. Perhaps it is inevitable that this methodology is correlational, for a first approach to the study of individual differences is to examine the generality of those differences over sets of observed variables, leaving aside the effects of differential treatments or manipulated conditions. Since the time of Spearman and Pearson, it has been assumed that if two measures taken on a given sample are significantly associated, they may be regarded as to some extent measuring "the same thing"; further, that if two acceptably reliable measures are not significantly associated, they are measuring "different things." If the observations are taken over a sample of persons, these "same" and "different" things are often assumed to represent attributes, characteristics, or "traits" of the persons. Such an assumption lies at the base of factor-analytic methodology, which is claimed to permit a detailed analysis of the multiple determination of observed variables by inferred latent traits. The theory of mental tests makes appeal to such an assumption in its postulation of "true scores" on latent traits that underlie observed measurements.

A persisting source of controversy in psychometrics is over whether such latent traits are mere statistical artifacts or, rather, represent real entities in the makeup of individuals. Let us for the moment leave this controversy aside and focus on purely statistical aspects of individual difference methodology.

Simple correlations

The literature of research in individual differences provides countless examples of the use of simple correlation to support the claim of a common element existing between two observed variables. It is tempting to assume that this common source of variance is a single entity. It is often forgotten that a single correlation could reflect the common operation (or lack of operation) of numerous sources of variance--sources of variance that could, presumably, be teased out only by some appropriate multivariate design.

I will cite and comment on two examples of the use of simple, zero-order correlations in current literature, chosen because they well illustrate certain points I wish to make.

Cohen and Sandberg (1977) were concerned with the relation between intelligence and short-term memory (STM). Their method was, essentially, to dissect the supraspan memory test into certain components, searching for those one or more components that showed correlations with IQ as measured by a certain intelligence scale constructed for Swedish children. A typical finding was a correlation of .68, highly significant, between IQ and performance on probed recall of the last three digits of a 9-digit sequence, but a correlation of .06, not significant with N = 38, between IQ and probed recall of the first three digits. Actually, their paper includes replication of this finding on a number of groups in a variety of conditions, and I am not doubting its reliability. I merely wish to point out that (except in plotting trends by score categories to demonstrate "continuity" of the function) they did not go beyond the use of simple correlations to explore further implications. The significant correlations could have reflected a number of different sources of variance; since the "IQ" test actually consisted of six subtests, it might have been fruitful to inquire whether the correlations were higher for certain subtests than others. One could question whether the subtests are truly measures of IQ; as described, the subtests were "designed to measure verbal performance (synonyms and antonyms), abstract-logical (inductive) reasoning, and spatial performance" (Cohen & Sandberg, 1977, p. 538). Some of the subtests, therefore, could have reflected special kinds of learning.

My second example comes from a series of investigations by Hock and various coauthors. In the first of these, Hock (1973) reported a correlation of .60, $p < .05$, between what he called a "symmetry effect" and a "rotation effect," in a study of individuals' reaction times (RTs) in a same-different comparison task involving dot patterns. Hock interpreted this result as reflecting individual differences in modes of processing the stimuli. Individuals whose RTs were affected by asymmetry and by rotation of the stimuli were said to be using "structural processing," whereas individuals whose RTs were not affected by these variables were thought to be using an "analytic" mode of processing. It must be pointed out, however, that the reported correlation was based on only twelve cases; furthermore, if we examine the scatterplot of scores on the two variables, it is evident that the correlation arises mainly from the presence of one or two strikingly outlying cases; omitting the most extreme of these reduces the correlation to .34, and omitting the next most extreme reduces it to .28, neither value being significant with $p < .05$. Hock himself did not comment on the unusual distribution of cases in the scatter-plot (or in the underlying distributions), but he did recognize that the correlation might have been due to an "artifact of performance level"; partialing out mean RTs to familiar, symmetrical patterns as measures of overall performance level, the resulting correlation between the symmetry effect and the rotation effect was still .60, according to Hock (1973, fn. 4). However, both these effects were measured as *differences* between mean RTs; one can question the meaning of a partial correlation between difference variables when the partialed-out variable is a variable that enters into the computation of the differences.

It should be observed, also, that RTs are themselves notoriously unreliable and variable, and their distributions are often quite skewed and loaded with outliers. My experience with RTs has been that it is wise to transform them before taking means; my preference (which for lack of space I will not attempt to justify here) is to use the reciprocal transformation, and to report mean reciprocals or the inverse of the mean reciprocal (in effect, the harmonic mean). (See Wainer, 1977, for further comment on this matter.) When *differences* between means are taken, and especially when this is done for individual Ss, one is creating variables whose reliability must be carefully examined. Even though in his later studies (e.g., Hock, Gordon, & Corcoran, 1976; Hock & Ross, 1975) Hock used slightly larger sample sizes in attempting to support his claim of an individual difference variable contrasting "structural" and "analytical" modes of processing, there are persisting methodological problems of the types mentioned here. Furthermore, in none of these studies has Hock attempted to identify an independent measure of the individual difference variable he claims.

Factor-analytic methodology

Factor analysis has been the classical method of choice in the study of individual differences, and increasingly, experimental psychologists are turning to its use. Despite its many virtues, factor analysis is a very tricky technique; in some ways it depends more on art than science, that is, more on intuition and judgment than on formal rules of procedure. People who do factor analysis by uncritical use of programs in computer packages run the risk of making fools of themselves. One can even be misled by misspellings in computer programs; I don't know how many times, in published literature, that I have seen principal components spelled "principle components," presumably because several widely-used computer programs happen to spell it that way in their print-outs. I assure you that the correct spelling is principal (Hotelling, 1933). But there are also a host of methodological problems that beset the unwary factor analyst. Many of these were discussed in an article by Thurstone, "Current issues in factor analysis" (Thurstone, 1940), but apparently this article is seldom read any more. I will cite some of the problems by commenting on the factor-analytic methodology used by Jarman and Das (1977) in an article that is almost fresh off the press in a new and hopefully prestigious journal. If I single out this study for comment, I do so only because it is a small, concise study that is easy to present and discuss. Many of my remarks could equally well be directed to various other recent studies in the literature.

Jarman and Das were concerned with establishing an "alternative model of mental abilities" that appeals to information-processing theories that claim information can be processed either by "simultaneous syntheses" or by "successive syntheses." They made separate factor analyses of seven psychometric and experimental variables obtained on 60 4th-grade boys in each of three ranges of IQ as determined by the Lorge-Thorndike Intelligence Test--Low, Average ("Normal"), and High. Principal component analysis with varimax rotation yielded three factors for the Low and High groups, and two factors for the "Normal" group. In each case, one factor was identified as representing the operation of "simultaneous syntheses;" the one or two other factors were identified with "successive syntheses" and speed, or some combination thereof. Let me make a number of observations about the methodology and presentation of these results.

(1) The small N's. Jarman and Das state (p. 154): "The selection of a group size of 60 was based on the requirement that there be a sufficiently large sample to perform within-group principal component and common factor analyses." Certainly an N of 60 is a bare minimum for establishing reliable results. It is better than the N's of around 20 to 40 that are being used by many experimenters in the individual differences field, to be sure, but one would still wish for a large sample size. I am afraid that this matter of sample size is going to plague the field for quite a time, for the kinds of experimental learning or performance tasks that we want to study in an information-processing mode require much more time to conduct or administer than the brief group paper-and-pencil tests that have traditionally been used in psychometric studies. The problem is compounded when one wants to study a large number of variables. Sometimes the number of variables actually approaches or even exceeds the sample size (e.g., Favero, Dombrower, Michael, & Richards, 1975). As a rule of thumb (for which I can give a certain justification), I recommend that to establish m factors, the sample size be at least as great as the quantity $(2m + 2^{m})$. On this basis, Jarman and Das's sample sizes were large enough to establish something like 5 factors, but they didn't have enough variables to do so. This leads me to the next observation:

(2) The small number of variables. Since Jarman and Das were interested only in establishing two factors, it could be argued that seven variables were sufficient. This is, for example, larger than what would be required by Thurstone's (1947, p. 293) criterion $n \geq [(2m + 1) + (8m + 1)^{\frac{1}{2}}]/2$ for the minimum number (n) of variables required for the determination of m factors. On the other hand, experience has shown that restricting oneself to small numbers of tests in a factor battery does not permit the kind of variation and sampling of factor domains that is desirable to provide persuasive evidence for the interpretation of any factors that may be found. Certainly Jarman and Das would be encouraged to explore the nature of their factors with a wider selection of variables.

(3) Failure to reflect variables. One of the most bothersome things, I find, in the inspection of factor analytic results is authors' failure to orient all their variables in some consistent

direction, i.e., preferably with the positive (algebraically greater) side being toward the more correct, desirable, fast, efficient, etc. kind of behavior. In Jarman and Das's factor matrices, we find a number of large negative values. One immediately wonders whether the matrices fail to exhibit what Thurstone (1947, p. 341) called positive manifold, i.e. a condition where all the loadings are positive or vanishingly zero after rotation for simple structure. It turns out, in the Jarman and Das data, that two of the variables were entered into the correlation matrices in what I call negative orientation, i.e. high values were associated with error or slowness. For one of the tests (Memory for Designs) the score was the number of errors, and for another test (Word Reading) the score was time for the subject to read 40 words. If I had been reporting and analyzing these data, I would have replaced the error score by a "number correct" score, and I would have converted the time score to a rate-of-performance score (by using some multiple of the reciprocal of the time score, e.g. words per minute). (Usually, such a transformation produces a more symmetrical distribution.) As it is, one can try to remedy the situation only by reflecting signs in selected rows of the factor matrices.

All this is mostly a matter of nicety and clarity in presentation: of course nothing is really changed in the results (except when one reverses orientation by making a reciprocal transformation, as in the case of the time score). The problem becomes particularly acute in connection with difference scores. Authors sometimes fail to report the direction in which they take differences. For example, Lunneborg (1977, p. 311) reported a "Stroop Difference" score as "the average difference in 'reading times' between the name and asterisk conditions." I am unaware of any convention whereby such a statement would convey whether the score was computed as (N - A) or as (A - N), and one cannot decide which it was on the basis of any other statement in Lunneborg's paper. (From Hunt, Lunneborg, and Lewis's report [1975] on the same data, one finds that the Stroop difference score was computed as [N - A].) Of course, sometimes one is not able to assess in advance how a difference score is best oriented, but this matter can usually be decided in terms of the configuration of factor loadings, and taken care of at the time of preparing the final results.

(4) Failure to reflect factors. Here is another matter that I find bothersome, though not really wrong. Frequently we see factor matrices with most of their large loadings negative. Or sometimes we see them with some high positive loadings, and some negative. Often this situation arises because of the failure to reflect variables, as just mentioned. But even after reflecting variables approppriately, one can still have a large number of negative loadings. Again, one immediately raises the question of a possibly non-positive manifold. In nearly every instance in my experience, the large number of negative loadings arises simply because the computer knows nothing about positive manifolds; it can make the loadings for a factor mostly all positive, or mostly all negative, depending upon certain conditions in the computation of eigenvectors or in the process of analytic rotation. The orientation of a factor is entirely arbitrary, as far as the mathematics is concerned. Regardless of whether a factor vector is oriented positively or negatively, it will make the same contribution to the reproduced correlation matrix, because in reproducing the correlation matrix, one is

multiplying entries pairwise within a vector; in matrix notation, these multiplications (for uncorrelated factors) are represented as $\underline{R}_r = \underline{FF}'$, where $\underline{F}$ is the factor matrix (variables X factors) and $\underline{R}_r$ is the reproduced correlation matrix. The situation is quite analogous to the computation of a square root, which can be either positive or negative: computers are "trained" to report positive square roots, normally, and they can be "trained" or programed to report positively oriented factor vectors. I recommend to all authors of factor analysis computer programs that they program in such a way as to change all the signs of any factor vector (either in eigenvector or in analytic rotation routines) that fails to have a positive algebraic sum. Many currently available computer package programs fail to do this. The remedy, short of changing these programs, is to change the signs by hand.

When appropriate reflections of variables and factors are made for the Jarman and Das matrices, they exhibit generally positive manifold. Even then, the matrices are of doubtful value because of another unwise procedure of analysis that these authors followed:

(5) <u>Separate factor analyses by ability strata</u>. As noted, Jarman and Das reported separate factor matrices for three groups defined by IQ. To be sure, IQ was not one of the variables included in the matrices, although one of the variables (Raven's Progressive Matrices) is often regarded as a measure of IQ, and indeed Jarman and Das's Table 2 (showing means of all variables for subgroups) suggests that all the variables were correlated with IQ to at least an appreciable extent. Now, doing separate factor analyses by ability

strata is a very risky procedure. Obviously, it entails restrictions of range and the consequent attenuation of correlations. It can also entail the creation of peculiar distributions of variables, even if there are no ceiling or floor effects in the tests themselves, because one is selecting from different portions of approximately Gaussian distributions. These peculiar distributions can affect the correlations in various somewhat unpredictable ways, as I have pointed out in an earlier publication (Carroll, 1961). And, of course, the sample sizes are automatically much reduced, with consequent loss of statistical power. Jarman and Das opted for the analyses by strata on the basis of their supposition that "different levels of intelligence ... may be characterized by different uses of simultaneous and successive syntheses for particular tasks," and their statement of the purpose of their study as being "to identify the similarities and differences, if any, in the employment of simultaneous and successive syntheses by groups of children differing in IQ" (Jarman & Das, 1977, p. 153). Unfortunately, because of the limitations just noted, factor analysis by ability strata is not in general a sufficiently reliable and effective tool to investigate hypotheses concerning differential use of processes at different levels of ability. Such hypotheses, I would suggest, could better be investigated at the level of particular correlations, e.g., by testing equality of regression slopes over ability groups for particular sets of variables, or by using contingency tables and other non-parametric techniques. Possibly Jöreskog's (1970) methods of covariance structure analysis would be useful. But for a preliminary evaluation of a set of data, I would recommend factor analysis based on a single group pooled from the several strata, even if the single group is not completely representative of some population because of gaps in the distributions, as where, for example, "high" and "low" tails or segments of some stratification variable are pooled (Hunt, Frost, & Lunneborg, 1973; Hunt, Lunneborg, & Lewis, 1975).

(6) Use of principal component analysis. On this matter there is, I acknowledge, a difference of opinion among experts, and some will say that it doesn't make much difference what factoring method is used. Jarman and Das used a principal components model (that is, I assume, an eigenvector factoring of a correlation matrix with unities in the diagonal) with varimax rotation, "for reasons of comparability to previous research" (p. 161). They also report that "high correspondence" was found between the principal components analysis and an alpha factor analysis that was also computed. There could indeed have been high correspondence in patterns of results, but principal components analysis tends to yield factor loadings that are considerably inflated over those of alpha and other types of common factor analysis, leading to overgenerous factor interpretations. I find principal components analysis useful chiefly in helping to decide on the number of factors to be used in subsequent communality estimations and common factor analyses. I much prefer some form of common factor analysis that avoids the intrusion of variance uniquely associated with each variable into the common factor space.

(7) Problems of factor rotation: orthogonal vs. oblique factors. This is another controversial problem. It happened not to present itself in the Jarman and Das data, because the factors exhibited a more or less satisfactory simple structure on orthogonal coordiantes.

Nevertheless, if one pools the data over the groups (as I have done, with the accuracy permitted by what these authors report, and doing the pooling on the basis of the reported group means and standard deviations and the correlations reproduced by the reported factor matrices), and if one uses a common factor analysis, the data are satisfactorily fit by two factors. A graphical rotation of these factors to simple structure suggests that the factors are to some extent correlated. The resulting analysis is given as Table 1, which also shows a Schmid-Leiman (Schmid & Leiman, 1957)

Table 1 about here

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orthogonalization of the data in such a way as to exhibit a "general" factor and two group factors. (I do not mean to identify this general factor with Spearman's "g", although it may well be highly correlated with it. The "general" factor is general only in the sense that it has substantial loadings on all seven variables.) The Schmid-Leiman factor matrix produces the same reproduced correlation matrix as the orthogonal two-factor solution does; that is, it accounts for the data equally well, although less parsimoniously. The Schmid-Leiman "hierarchical" procedure has been too little employed in factor-analytic studies; it provides one way of resolving the perennial controversy between those who argue for simple structure, correlated factors (when necessary), and parsimony, and those who argue for orthogonal factors because of their ease of interpretation.

In general, of course, the problem of rotation to simple structure is a very tricky one. As an old hand in factor analysis, I

Table 1

Reanalysis of Data from a Study by Jarman and Das (1977)

Estimated Correlation Matrix for Pooled Groups (N = 180)[a]

		1	2	3	4	5	6	7
Raven's Progressive Matrices	1	1.000						
Figure Copying	2	.503	1.000					
Memory for designs[b]	3	.545	.547	1.000				
Serial recall	4	.218	.206	.202	1.000			
Visual short-term memory	5	.207	.185	.193	.510	1.000		
Word reading (speed)[c]	6	.206	-.022	.022	.503	.446	1.000	
Auditory-visual matching	7	.442	.433	.410	.489	.360	.149	1.000

Solutions with Two Factors

		Common Factor Orthogonal Varimax			Oblique Simple Structure		Schmid-Leiman Hierarchical Orthogonalization		
		A	B	h^2	A'	B'	G	A"	B"
Raven's Progressive Matrices	1	.672	.178	.483	.615	.083	.447	.527	.071
Figure copying	2	.741	.042	.551	.713	-.062	.418	.611	-.053
Memory for designs[b]	3	.748	.058	.563	.716	-.047	.429	.614	-.040
Serial recall	4	.216	.770	.640	.037	.732	.494	.032	.628
Visual short-term memory	5	.189	.640	.445	.040	.608	.415	.034	.521
Word reading (speed)[c]	6	-.023	.661	.437	-.171	.658	.312	-.147	.564
Auditory-visual matching	7	.558	.384	.459	.457	.303	.487	.392	.260

Λ

A	.974	-.139
B	-.225	.990

R

A'	1.000	.358
B'	.358	1.000

[Footnotes for Table 1]

[a] These correlations were estimated by pooling correlation matrices for the Low, Normal, and High IQ groups as reproduced from the factor matrices presented by Jarman and Das, using also the data given for means and S. D.'s of each variable for each group.

[b] This variable was reflected from the original variable, which was in terms of error scores. Here, the variable may be thought of as number correct.

[c] This variable was reflected from the original variable, which was in terms of time to read 40 words.

would still prefer rotations done completely by hand, i.e., by graphical techniques, but I haven't done one of these messy jobs for any large study in about 20 years, now that analytic rotations by computer are available. My current practice is to use the Kaiser normal varimax method to produce an orthogonal solution, followed by any graphical adjustments to obliqueness that may seem desirable. In this I often use a "semianalytical" Procrustes rotation to the oblique structure suggested by the pattern of varimax results, using a method developed by Tucker (1944). The direct oblimin method (Jennrich & Sampson, 1966) is also to be recommended. I have not taken any extensive opportunity to experiment with Jöreskog's methods (1967 , 1970), but they have much appeal because they include tests of statistical significance.

While we are on the matter of rotation, however, let me mention some possibilities that might be considered. One of the more interesting factor analysis studies that I have encountered was that of Underwood, Boruch, and Malmi (1977). These investigators factored correlations of 22 scores from nine verbal learning and memory tasks, hoping to find factors associated with particular "attributes" of memory items such as concreteness/abstractness, meaningfulness, and time. In this they were largely unsuccessful, concluding that "associative memory" variance was so prominent as to swamp any effects of memory attributes, also pointing out that apparently subjects adapt themselves to use whatever attributes are relevant for a particular task. Nevertheless, the final factor analysis data are of interest, even though the five factors are all largely task-specific. That is, there is a factor that loads on scores from paired-associate and serial learning tasks, one that loads on scores from free-recall tasks, and so on. The N of 200 was respectable, and the factor analysis was performed in a sophisticated way, using Jöreskog's (1967) maximum likelihood method, among others. If one plots the factor loadings for Factor 1 (the "paired-associate" factor) and Factor 2 (the "free-recall" factor) that are shown in their orthogonal factor matrix (Table 4, p. 59), an obvious oblique simple structure rotation is possible, as shown in Figure 1.

Insert Figure 1 about here

The normals to the two hyperplanes are separated by an angle of about 138°, and this would correspond to a correlation of about .743 between rotated primary factors. (I ignore any other possible rotations.) Thus we could have a strong second-order factor underlying both the paired-associate and free-recall scores; one might interpret it as an "associative memory" factor and identify it with the associative memory factor (often symbolized as Ma; see Harman, Ekstrom, & French, 1976) that has been found by Thurstone and many others in the psychometric tradition. A Schmid-Leiman orthogonalization would yield this "associative memory" factor as a group factor, plus separate factors for the paired associate and free-recall tasks. In this case, the Schmid-Leiman procedure would be neither parsimonious nor very informative. Another kind of analysis might be more useful: let us pass a primary vector through the centroid of both the paired-associate and free-recall test vectors, letting this represent an associative memory factor, AM, underlying these two types of tasks. Orthogonal to this vector, as shown in Figure 2, we could establish

Insert Figure 2 about here

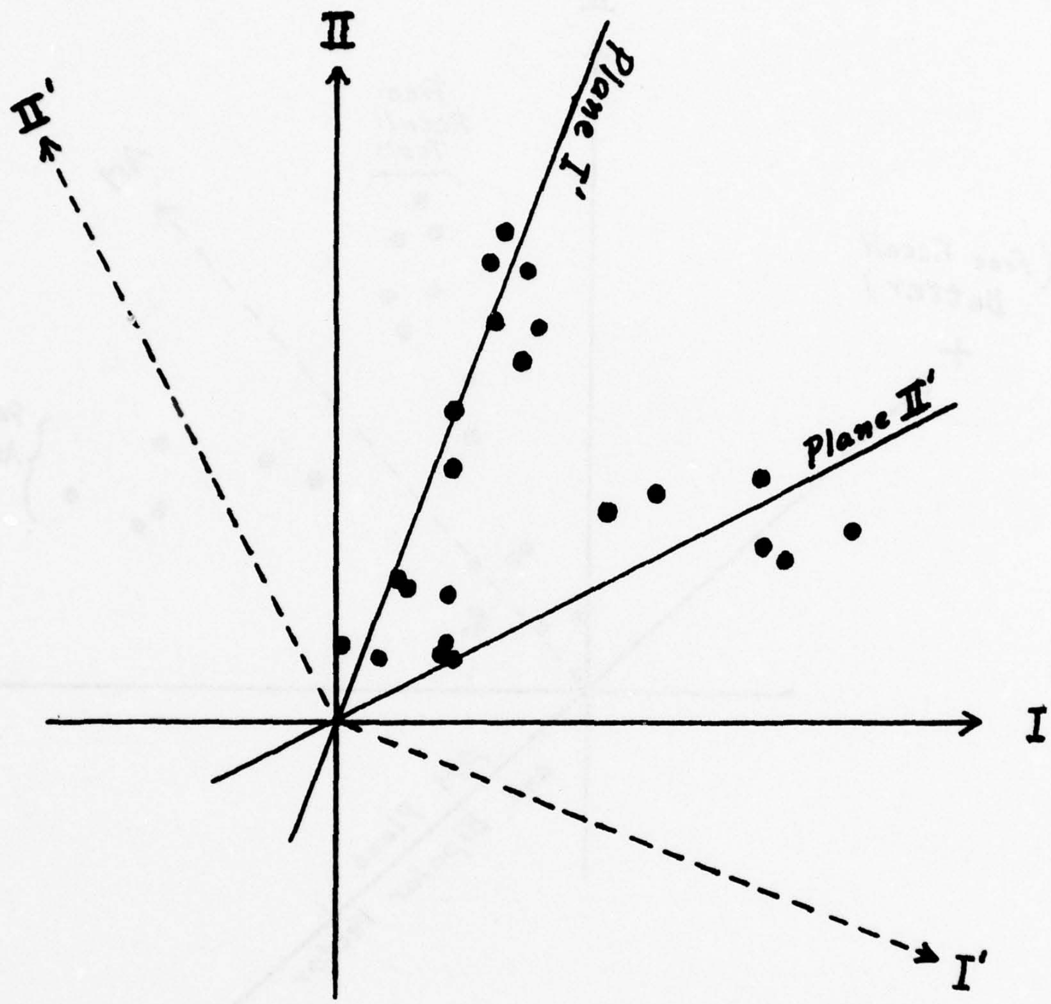

Figure 1. Plot of 22 task vectors on orthogonal rotated Factors 1 and 2, with suggested oblique simple structure rotations. Data are from a study by Underwood, Boruch, & Malmi (1977, Table 4, p. 59).

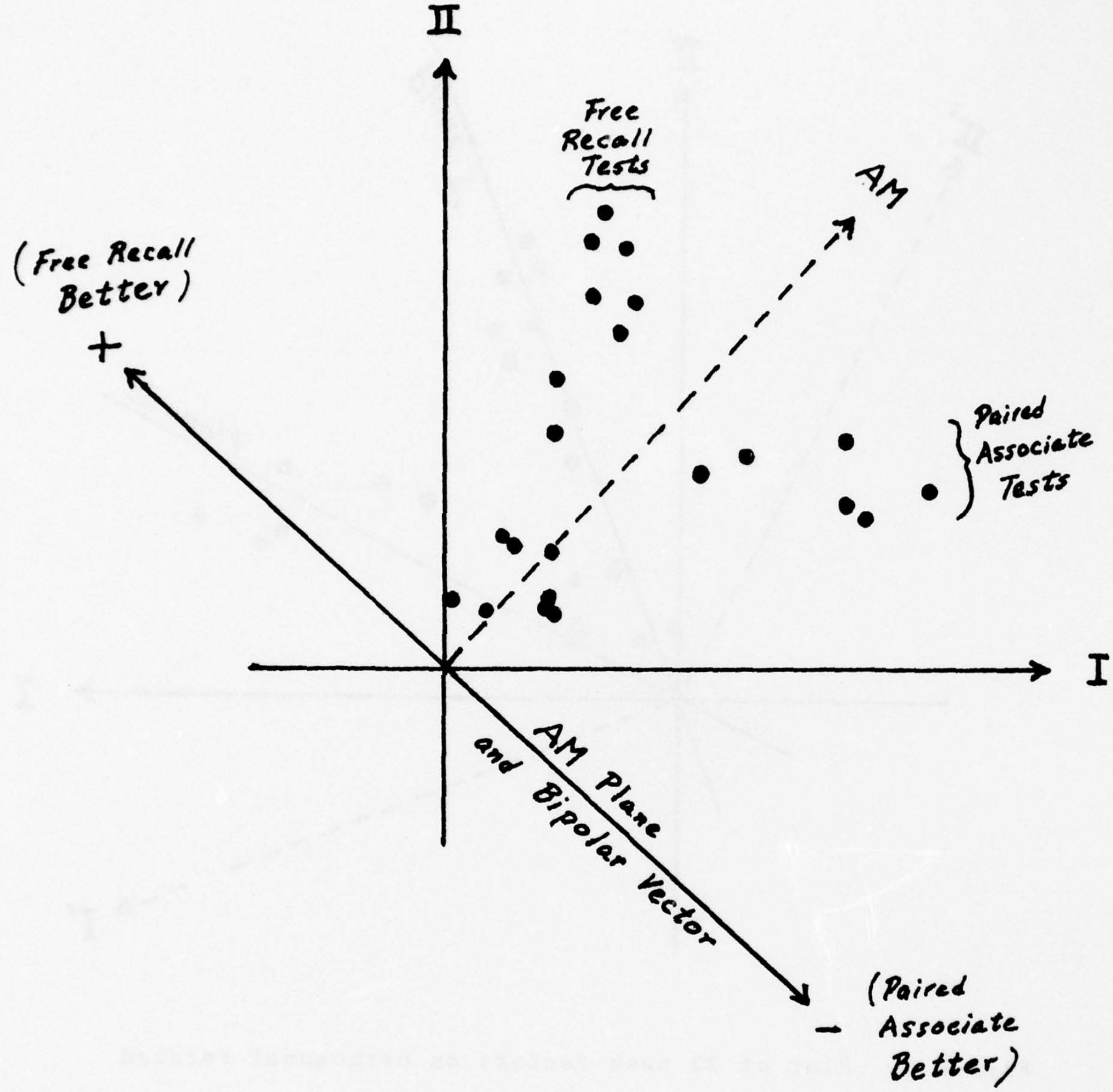

Figure 2. Plot of data as in Figure 1, but with a suggested orthogonal rotation to yield a general associative memory (AM) factor and a bipolar factor representing relative abilities in free recall and paired associate tasks.

a bipolar vector that would represent the difference between paired-associate and free-recall performances. Arbitrarily, let the positive pole represent the performance of those who are better at free-recall, relatively, than they are at paired-associates. Surely with all the information we now have about the similarities and differences of these two types of tasks, we ought to be able to make an interpretation of this vector that would appeal to the processes differentially involved in them. This illustrates a rotational maneuver that may often be desirable in studies of individual differences in an information-processing mode. (This maneuver, incidentally, preserves orthogonality in the matrix as a whole, provided factors are always rotated pairwise and orthogonally.)

In continuing this critique of factor-analytic methodology, I will refer to various studies other than those of Jarman and Das and of Underwood and his colleagues. Certain subtle methodological problems are illustrated in studies conducted by the group at the University of Washington (Hunt, Frost, & Lunneborg, 1973; Hunt, Lunneborg, & Lewis, 1975).

(8) The problem of experimental dependence. In the theory of matrices, it is shown that if any variable or linear combination of variables can be perfectly predicted from another variable or linear combination of variables, the matrix of correlations among the variables is "singular," with no inverse. In factor-analytic computations with matrices that are singular or approximately so, the effect of the singularity can show up as spurious common factor variance (i.e., common factor variance that would be unique variance if it were not for the singularities). In many cases, one finds common factors that are associated solely with the source of the

singularity, but a partially spurious common factor could be one that is associated with the variables producing a singularity plus any variables that are substantially correlated with them.

In the light of these considerations, it has sometimes been argued that one should avoid any potential source of singularity (even approximate) at all costs, even to the extent of avoiding measuring two or more variables from the same experimental task. Such variables are said to be experimentally dependent. In the light of experience, however, I believe the rule of avoiding multiple measurements from a single task is too stringent. There are many instances where one can derive a number of logically independent variables from the same task. By "logically independent," I mean "conceivably having a correlation of zero." For example, the rate at which a task is performed can be logically independent of the accuracy with which it is performed. Of course, speed and accuracy may in fact be substantially correlated, either positively or negatively, but if they are not <u>highly</u> correlated, and if there are independent measures of each of them from a variety of other tasks, the risk of obtaining spurious common factors is minimized.

One source of singularity, however, should be carefully avoided: the use of sum and difference scores when these sum and difference scores are perfectly predictable from other variables that are used in the correlation matrix. (The lower bound communality estimates for such variables would be unity.) It is an egregious error, for example, to use such sets of variables as (A, B, A + B) or (A, B, B-A). Such an error is illustrated by the use of a series of part scores on a test along with the total of these part scores. It is better, in such a case, to use only the part scores, or only the total score, but not both. But what about the situation in which it is desired to use variables (A, B-A)? This is exemplified at least twice in the Hunt, Lunneborg, and Lewis (1975) factor analysis of psychometric and experimental tasks. One pair of variables was derived from a modified Stroop task: "asterisk reading time," and "color name minus asterisk reading time." Another pair of variables was derived from the Posner task: "physical match time" and "name minus physical match time." Now, the variables in such pairs are logically independent, and they could be completely uncorrelated in the data. But notice that if the basic variables (which might be symbolized as A and B, so that for example A = name match time, B = physical match time) are uncorrelated, there is an inevitable negative correlation between the derived variables A and (B - A) that will produce artifactual common factor variance. For the general case, the correlation between A and (B - A) will be

$$\frac{r_{AB}\sigma_B - \sigma_A}{(\sigma_A^2 + \sigma_B^2 - 2r_{AB}\sigma_A\sigma_B)^{\frac{1}{2}}}$$

and the requirement on a zero correlation of the derived variables is that $r_{AB} = \sigma_A/\sigma_B$, a requirement that would not ordinarily be satisfied in practice. In fact, the correlation between the derived variables will always be negative if $r_{AB} < \sigma_A/\sigma_B$; thus, the correlation depends strongly on the ratio of the standard deviations. (Under non-linear transformation, such as the logarithmic or reciprocal transformations, this ratio can change markedly.)

It will be noticed in the Hunt et al. (1975) factor matrix just mentioned that both of these pairs of variables have negative loadings on Factor II; in each case, furthermore, there is a high loading for the difference variable and a lower loading for the A-variable. Undoubtedly there is some spuriousness in these results, but it is difficult to tell how much there was because it would appear that the difference variables were correlated across independent tasks. My recommendation for future work of this type is that pairs of variables that include one basic variable and one derived variable should be measured from independent tasks, e.g., the physical match score from one series of trials and the name-physical match difference from another series of trails, even at the cost of lessened reliability. Even so, the difference score variables might have been correlated over tasks spuriously, i.e., on the basis of an overall RT factor. The problem is similar to that encountered in the Hock (1973) study mentioned earlier. A possible solution is to compute the derived variables in terms of standardized scores of basic variables.

(9) The design of a factor analysis. Ideally, a set of variables entered into a factor analysis should conform to a hypothesized structure in which each factor has at least three or four significant loadings that are not accompanied by significant loadings on other factors, and in which each variable has a minimum of non-zero loadings--preferably, only one, unless the variable is regarded as impure *ex hypothesi*. It is not generally good science to factor-analyze any arbitrarily selected series of variables. It is not necessarily good science to frame a factor analysis according to the question "To what extent do the psychometric and the information-processing variables measure the same abilities?" To this extent

the Hunt et al. studies could be faulted. In these studies there is little evidence either that the information-processing variables were selected specifically to coordinate with the psychometric tasks, or conversely. Furthermore, the specific primary factors that are known to be measured by the several psychometric tests (Space, Number, Verbal, etc.) were not separately represented by adequate marker tests in the battery design, so that it is not surprising that the significant loadings for the psychometric tests came out chiefly on a single factor, Factor I (Hunt, Lunneborg, & Lewis, 1975, p. 222), which had experimental task loadings chiefly on scores derived from a complex mental arithmetic task, the "Sunday + Tuesday" task--surely a task that would have counterparts in the numerical and verbal reasoning psychometric tasks.

In fact, the irony of the Hunt, Lunneborg, and Lewis study is that although it claims to find common variance between the psychometric tests and the information-processing tasks, such variance is only weakly apparent in the factor-analytic results. In general, the two sets of variables have significant loadings on <u>different</u> factors; in particular, the purely "verbal" tests had no significant loadings on the factors that were chiefly associated with the information-processing tasks. It is difficult to square this observation with the generally significant contrasts between "high verbals" and "low verbals" that are reported in the descriptions of results for specific tasks, and that are supposed to tell us "what it means to be high verbal." I can possibly reconcile these sets of results by assuming that the factor analysis was not done by proper common-factor techniques, and not carried to the point of oblique rotation, second-order analysis, or hierarchical analysis.

The high-low verbal contrast might have appeared at the level of a weak but significant second-order factor.

If indeed one wants to ask the question that Hunt, Lunneborg, and Lewis asked, "To what extent do the psychometric and information processing tests measure the same abilities?", there are variants of factor analysis, and other multivariate techniques, that would be more appropriate than classical factor analysis. One of these is Tucker's (1958) interbattery factor analysis; the use of this method is well illustrated in a study reported by Hundal and Horn (1977). They administered a series of "psychometric" tests, as one battery, and a series of learning and memory tests, as another battery, to 265 14-year-old school children in India. Tucker's method was used to determine what kinds of variance were common to the two batteries. They identified two such types of variance: a "Gf" or fluid intelligence factor in the psychometric tests that was related to the primary memory storage aspects of the memory battery, and a "Gc" or crystallized intelligence factor that was more related to those aspects of the memory battery that relied on what they regarded as a "secondary acquisition process."

<u>Canonical correlation analysis</u>

One might also use canonical correlation analysis to identify sources of variance common to two sets of measures. In effect, one finds linear composites in one set of measures, say the psychometric tests, that correlate optimally with linear composites of the other set of measures, say the information-processing tasks. This technique was in fact used by Lunneborg (1977), but I do not recommend it for this purpose because canonical weights are generally difficult to interpret. The technique is highly subject to problems of collinearity, and canonical weights have the undesirable sampling characteristics of beta-weights in multiple-regression analysis. Indeed, canonical correlation analysis is a generalization of multiple-regression analysis. If canonical correlation analysis is to be used at all, it should be followed by rotation of the canonical variates and computation of correlations between each variable and the canonical variates (see Cliff & Krus, 1976; Wood & Erskine, 1976). In this case, however, the results will probably resemble those of Tucker's interbattery factor analysis, which could have been used in the first place.

Multiple regression analysis

Multiple regression is, of course, a popular technique, particularly for prediction studies. I believe, however, that it should be used only very cautiously in basic studies of individual differences. The problem of collinearity among predictor variables is especially to be attended to in the *interpretation of either* raw or standardized regression weights. This problem was encountered, for example, by Sternberg (1977), p. 219) in his attempt to determine "structural regressions" relating "component scores" from analogical reasoning tasks to overall scores on these tasks and reference ability measures. If multiple correlation techniques are to be used, I would warn particularly against the popular forward stepwise solution, which can be very misleading. It can even fail to produce the optimal subset of variables for predicting a criterion variable, by stopping short of taking account of some significant combination of variables at a point when no single variable adds significant variance. I recommend the use of a complete regression system, followed by a backward stepwise elimination technique if perchance one wants to reduce the number of variables in the prediction. With care, one can usually make some acceptable interpretations of multiple regression weights.

The Identification and Investigation of Psychological Processes

Having completed my catalogue of the major statistical sins that can be committed in individual difference studies, let me now turn to the more psychological side of the question, how shall we study individual differences?

I said earlier in this paper that I believe we have made little progress, thus far, in identifying psychological processes, at least through individual difference research. In most of the individual difference research I have surveyed, it seems that whenever one tries to find a process, one really finds a trait. That is, in simple terms, one sets up a study involving certain tasks. One notices that individuals differ in their success, speed, or efficiency in performing these tasks. Through factor analysis or other techniques, one tries to pin down the types of tasks, or components of those tasks, in which the individual differences can be observed, and then to determine, if possible, to what range of other tasks (or components of tasks) these individual differences generalize. Even if one is successful in such a venture--and there are relatively few success stories--the final data comprise the following: a description of certain tasks, or components of tasks, and a statement that individuals (in defined populations, presumably) exhibit characteristic differences in their performance of these tasks. To the extent that the differences are truly characteristic of the individuals in terms of relative stability and permanence, the individual differences comprise what we call a trait. Actually, the stability of individual differences in most of the information-processing traits found thus far has hardly been investigated, unless one assumes that correlated performances spaced a few hours or days apart signify a degree of stability. How much do we know, for example, about the long-term stability of simple or choice reaction time measures, or about the types of measures taken, to say, in the Posner physical vs. name match paradigm? Not much, I fear.

The important point, however, is that inferences from individual difference traits concerning underlying psychological processes, even when studied in an information-processing mode, seem speculative at best. Perhaps I am asking for too much, or wanting too much, in a description of a psychological process, but most "descriptions" of psychological process that I have seen seem to boil down to descriptions of tasks--perhaps in terms of the stimuli, the instructions, and the responses, with speculations about what kinds of information are involved, and how the information is transferred and manipulated. There is little talk of different strategies that individuals might employ.

Let us review some of the data that come out of the studies I have referred to in my statistical critique, to see whether my general point of view can be supported. For convenience, I will refer to the studies more or less in the order in which they were discussed earlier.

Cohen and Sandberg (1977), we noted, found that the correlation between IQ and memory span performance, when one controlled for rehearsal, interference, chunking, and other effects, was limited to that part of the task in which the subject repeats the last few stimuli. Even though there was evidence (not noted by these authors) of reliable individual difference variance in other aspects of the task, that variance was not associated with IQ. High IQ children were, however, more accurate than low IQ children in repeating the last few stimuli of a supraspan digit memory task, regardless of the modality and the rate of presentation. This was particularly true when the individuals had had a prior opportunity to practice a span task requiring complete recall. These results were in considerable conflict with those of prior studies, which had located the IQ/STM correlation mainly in rehearsal effects. Assuming that the Cohen and Sandberg results are reliable, what can we infer about processes? We have an individual difference variable that is in some way related to whatever IQ is, and perhaps the results tell us something about IQ, but they do not directly tell us about processes. Cohen and Sandberg themselves argue for what they call an "availability explanation":

> In a sequence of known length, inclusion in the rehearsal buffer would be least likely for the final items, so that individual differences in decay rate would exert their greatest influence on the most recent items. The same argument can be made for the final items in the running memory sequences, since they are also unlikely to be maintained in the buffer. (p. 552)

The "availability explanation," then, refers to an assumption of a "decay rate" for recency items, which, according to the theory the authors have adapted from Atkinson and Shiffrin (1968), are "either nonbuffer items from STS [short-term store] or preattentional items from SR [sensory register]" (p. 537). Presumably, further study would be required to decide whether the decay rate applies to the former or to the latter, or to both. In any case, we are offered only speculation about what processes are involved in the found IQ/STM correlation; the only solid finding is that one aspect of some trait called IQ is the efficiency of repeating the last few digits of a supraspan series. I suppose this is progress, however, all things considered.

The series of alleged findings by Hock and his associates (e.g., Hock, 1973; Hock, Gordon, & Corcoran, 1976; Hock & Ross, 1975) might seem a little more promising in allowing inferences about processes, even though their statistical methodologies and results appear open to question. In a variety of tasks, these authors claim to find a contrast between what they call "structural" processing and "analytic" processing. That is, in the same-different comparison of visual displays of various kinds (dot patterns, pictures and scenes, degraded alphabet letters, etc.), some people are thought to make point-by-point or "analytic" comparisons, while others make comparisons based on a Gestalt perception of the total stimulus, these latter being called "structural" comparisons. I will not attempt to describe here the exact kinds of experimental data that are offered to support these inferences; I will say only that they are highly speculative, based on certain intuitions about how certain stimulus variables might have an effect on behavior. For example, certain few subjects in Hock's (1973) experiment seemed to have longer RTs when the stimuli were asymmetric or rotated; it was inferred that they were perceiving the stimuli "structurally" and thus the asymmetry and rotation interfered with their perceptions. The remaining subjects' RTs were relatively unaffected by rotation or asymmetry; it was <u>inferred</u> that they were making point-by-point comparisons of the stimuli (whether rotated or asymmetric or not), thus doing "analytic" comparisons. Nevertheless, an alternative interpretation of the results is that all subjects were making point-by-point comparisons, but that some subjects were less able to find the loci of these comparisons when the stimuli were rotated. Perhaps, for example, these subjects would be found to be low on the Spatial Orientation factor isolated in many psychometric investigations. Hock and his associates made no attempt to identify an independent variable to illuminate their results.

I say that these results are promising, but this is not because of anything that Hock and associates have done with their experiments and data. "Structural" and "analytic" processes sound like distinctly different mental operations. Let us suppose that they are. If one could arrange a situation where a subject could be successful only if he made a "structural" comparison, and another situation where a subject could be successful only by making an "analytic" comparison, and if some independent way could be found of predicting subjects' performances in the two situations, we might have a case for the reality of the postulated processes.

Let us now examine the theories of Jarman and Das (1977) concerning "simultaneous" and "successive" syntheses. Their results concerning differential use of such processes in groups at different IQ levels are almost completely unconvincing because of limitations in their methodology. Leaving this aside, however, let us look at the reanalysis of their results that I have made by pooling data from the three IQ strata (Table 1). If we consider these results from the standpoint of classical factor-analytic interpretations, it appears that we have a general factor that enters all the tests, plus two group factors, A and B. Factor A looks like a standard, ordinary Space factor; that is, it has high loadings only on tasks in which the subject has to perceive, remember, or otherwise manipulate visual spatial designs involving geometric figures. The Figure Copying and Memory for Designs tests contain this feature, as does also the Raven Progressive Matrices (whatever else it may involve). Whatever process is involved in such a factor can only be described, somewhat tautologically, as "dealing with spatial designs." Apparently a characteristic trait of individuals is their ability to deal with spatial designs, and little more can be said about this because there are not enough variations among the tests to permit further analytic interpretations. We can say, however, that this trait does not extend to <u>any</u> visual display, because it does not extend, for example, to the Visual Short-Term Memory task that requires the subject to remember the digits that have been displayed on a five-section grid (essentially a visual digit-span test) . It is a pure inference that what is involved here is a "simultaneous synthesis," i.e. apprehending parts of a display simultaneously. To establish that simultaneous synthesis is a process would require the demonstration that it operates in a variety of settings, not restricted to the tasks involving visual patterns and designs that were used in this study. Even then, use of individual differences methodology in such a demonstration would permit only the inference that people differ in their characteristic use of this process when it is appropriate, and thus we would end up with a statement that refers more to a trait than it does to a process.

Similar remarks can be made about the other group factor, B, in the reanalyzed Jarman and Das data. This factor looks like a memory factor that arises when the serial order of the stimuli is important. It is only an inference that some special "successive synthesis" operation is involved.

In my methodological critique, I next considered the study by Underwood, Boruch, and Malmi (1977), which I though was a very well done job. From an information-processing standpoint, however, the study was a failure; what these investigators came out with was a series of traits, not processes. That is, it seems that there is some sort of "associative memory" trait whereby some people are better than others at paired-associate and free-recall tasks, although there may be an additional trait that determines which of these two types of tasks one does better at (at least, this is what my reanalysis suggests). This associative memory trait, I believe, is pretty much the same as the associative memory factor (<u>Ma</u>) that has been identified in psychometric researches that are too numerous to list here. (See Ekstrom, 1973, for a recent review.) Also, the memory span factor identified in the Underwood et al. study looks to be the same as the <u>Ms</u> or Memory Span factor identified previously. The fact that <u>Ma</u> and <u>Ms</u> are largely independent would suggest, however, that memory span ability does not operate in free recall tasks. On its face, this conclusion sounds rather counterintuitive--couldn't one do a free-recall memory task as a <u>succession</u> of memory span tasks? But the results say not; if memory span operations are attempted in a long free-recall task, they don't work, perhaps because they interfere with each other. I am not enough of a specialist in memory theory to penetrate deeply into this question. At any rate, the identification of traits of associative and memory span abilities tells us very little about the processes involved; it tells us only that different processes operate in different task settings, and this information might conceivably help in assigning individuals to different task settings.

Underwood et al. made a valiant attempt, however, to design their study in such a way as to reveal the operation of what they call "memory attributes" (imagery, associative, acoustic, temporal, affective, and frequency). They failed to find factors corresponding to these attributes, and I refer the reader to their report for their apparently quite reasonable explanations for this failure. But I would also urge consideration of the possibility that their study was not adequately designed to produce factors for imagery and the other attributes. Consider the imagery attribute, for example. Unless I am mistaken, they had too few measures in which a concrete-abstract attribute would have operated distinctively, too few, that is, for an adequate factor-analytic design. (Remember Thurstone's recommendation that there be at least three variables to represent a postulated factor in as pure a form as possible.) An alternative design that might have been considered is the multitrait-multimethod design originated by Campbell and Fiske (1959; see also recent refinements proposed by Ray & Heeler, 1975). This design would have required a more systematic crossing of traits with attributes (the attributes being considered analogous to "methods").

Suppose, however, Underwood et al. had been successful in identifying an "imagery" factor, derived, presumably, from sets of variables that would contrast learning of concrete stimuli with learning of abstract stimuli in different kinds of learning tasks. Essentially, such a factor would represent a trait--the extent to which a person "uses" imagery (or some similar process) in a learning task in which opportunity to use that process was offered. The description of the trait would refer to a process, but it is not at all clear whether that process had been correctly identified. One could conceive a number of processes that might account for differences in the way people handle concrete words vs. abstract words in a learning task. For example, concrete words might be handled more easily than abstract words by attaching attributes to them, and people might differ in their readiness or predisposition to do so. (Experimentalists often attempt to control such variance by equating words in "associative value," but in an individual differences context such control might actually be counterproductive.)

Now consider the factor interpretations made by Hunt, Lunneborg, & Lewis (1975). Factor I was loaded with a variety of psychometric test scores and also some parameters from the "Sunday + Tuesday" mental arithmetic task. They called it "rapid reasoning," since "it is characterized by tasks which involve transformation of information in short term memory, typically in a sequence of steps" (p. 222). They contrasted Factor I with Factor II, which "had its highest loadings on the clerical speed tests, upon scores for naming colors less scores for naming asterisk colors in the Stroop task, and the name identity minus physical identity scores in the Posner *et al*. paradigm," tasks that were "characterized by a requirement that overlearned codes be accessed, but not by the (Factor I) requirement that the codes thus accessed be transformed in any way" (pp. 222-223). Although these interpretations are plausible, they are at the same time problematical. Undoubtedly the psychometric tests on Factor I involve reliance on the presence of codes in long-term memory (meanings of words in the verbal tests, for example). A more sharply focused series of experimental settings would be required to demonstrate that the factor involves transformations of codes in STM. The interpretation of Factor II as speed in accessing untransformed codes in STM could be defended only if it could be shown that it is irrelevant whether codes in LTM, or transformed codes in STM, are involved. In the case of both Factors I and II, the factors are defined by the individual trait findings, and the inferences about processes rely on theories about information-processing operations that would need to be confirmed in procedures that would not rely on individual differences. Perhaps Hunt and his colleagues would argue that these theories have indeed been confirmed in various investigations that do not rely on individual differences. My only point is that from the factor analysis results alone we do not have a confirmation of those theories; we have only certain plausible descriptions of individual difference traits whose appearance is elicited by certain types of tasks.

Similar remarks can be made about the interpretation of Factors III, IV, and V in the Hunt, Lunneborg, and Lewis study and I shall not pursue this line of discussion further. Let me dispel any possible tensions about my remarks by saying that I consider the Hunt, Lunneborg, & Lewis study very valuable. Aside from the methodological and theoretical limitations to which it is subject, it is an important venture into a relatively new field of scientific endeavor, and it should inform us all about research needs and possibilities in this field.

Studies by R. Sternberg (1977) and J. Frederiksen (1978; Note 1) move us into an almost totally new methodology, that of what

Sternberg calls "componential analysis." This methodology appears to offer a greater possibility of isolating processes than traditional factor-analytic methodology, and should be examined closely.

Briefly, Sternberg's methodology involves arranging experimental situations that systematically vary the subject's opportunities to engage in different information-processing tasks. The verbal analogies task that Sternberg has chosen to study is particularly well suited for use of this methodology. Thus, if the verbal analogies task is symbolized as the presentation of a series of stimuli that have relations A : B :: C : [D], where [D] represents a number of alternative stimuli, only some of which exhibit "true" analogies, Sternberg can present the stimuli in different temporal sequences such that the processing times that a subject requires at different stages of the solution can be determined. For example, one can present only the stimuli A and B, allowing subjects to process those stimuli (determining a possible relationship) before the presentation of the C and D stimuli. These processing times can then be contrasted with those required if three stimuli, A, B, and C, are presented simultaneously, and the parameters of the processes can be determined on the basis of several alternative models of these processes. Essentially the procedure is a very sophisticated application of the Donders subtraction method.

Sternberg has presented results that suggest that this method can illuminate the description and explanation of the individual differences found on standard psychometric tests, in particular, the verbal analogies test that often appears in mental abilities batteries. I have some problems with his methodology, in particular the very small number of subjects (for example, N = 16 in the

"People Piece" experiment) and the shakiness of multiple regression methods employed with such a small number of subjects. Nevertheless, the results are of high interest and are suggestive, and if one limits oneself to the results intrinsic to the experiments, the fits to models are impressive.

J. Frederiksen's (1978; Note 1) methodology is highly similar, except that he has been concerned with component processes in reading. Reading does not easily lend itself to the type of sequential stage presentation that is possible in the case of Sternberg's verbal analogies task; instead, it was necessary for Frederiksen to present a number of different tasks that represent, according to a theoretical analysis of the task of reading (in particular, word identification and recognition), the several phases of this process. For example, it is assumed that there is a stage of Perceptual Encoding, with two subphases, Grapheme Encoding (access of letter codes) and Encoding Multi-letter Units. The processing times for these phases are measured from two tasks, a Letter Matching task (analogous to the Posner task) and a Bigram Identification Task. Further stages are Decoding, with two subphases, Phonemic Translation, and Articulatory Programming (since an oral response is required), and final Lexical Access stage; measures of these processes are taken from word and pseudoword naming tasks. A maximum likelihood factor analysis of eleven measures exhibited good fit to the five hypothesized sources of variance; these five sources of variance were found to be somewhat correlated, but it is of great interest that, for N = 20, these five factors yielded high multiple correlations with scores on standardized reading tests that involved sentence comprehension processes that were not at all tapped by the experimental tasks, which went no further than requiring word identification. The multiple regression weights, producing multiple correlations ranging from .53 (for the Gray Oral Reading Test) to 1.00 (!, for the Total Score on the Nelson-Denny Reading Test), seemed to indicate that the multiletter encoding and the articulatory programming measures were the major predictors. These results can be interpreted as suggesting that good readers use strategies of phonemic decoding, while poor readers recognize words more on the basis of whole-word appearance. They need to be cross-validated, however, with larger sample sizes.

It appears to me that in both the Sternberg and Frederiksen studies, a good case can be made for the confident identification of psychological processes. The theorized stages are operationalized by arranging the experimental tasks in such a way as to permit a subject to perform only on the basis of a definable set of processes. For example, in the Sternberg task the presentation of only the A-stimulus permits only an encoding operation; the later presentation of the B and C stimuli permits performance of operations that Sternberg describes as inference and mapping, but not application, which is possible only when the D stimulus is supplied. In Frederiksen's work it is clear that no lexical access operation is normally possible if the stimuli are only single letters.

Nevertheless, in both Sternberg's and Frederiksen's work the identification of stages is almost crucially dependent upon the presence of individual differences that are specific to those stages. This is quite explicit in Frederiksen's work, where the confirmation of the stage analysis is based on a maximum likelihood factor analysis of a correlation matrix of individual difference variables. In Sternberg's work a stage could in principle be identified by associating it with additive constants in which individual differences are negligible, but that seldom happened in Sternberg's data, if at all. We have here, then, the same kind of parallelism between traits and processes that we have observed in other types of methodologies.

Now, perhaps this is the way things are--the way world is. That is, perhaps processes are clearly identifiable only through their association with individual differences, and perhaps it is inevitable that there should be individual differences associated with any given psychological process. (Surely the converse of this is not true.) But if this is really the case, it presents a discouraging prospect for any efforts to modify individual differences, or even to utilize individual differences in instruction or training in innovative ways. It would appear that we would have to fall back on the old routines of task analysis, and selection of individuals for training or assignment in terms of their known characteristics in relation to our task analyses. The prospects for meaningful attribute-treatment interactions in learning processes would also appear to be dim.

I do not think the picture is really as dark as it would appear to be, or as I have presented it. I have deliberately presented it in a rather bleak form in order to stimulate thinking. There are a number of considerations that might alleviate the situation.

For one thing, I have painted an undeservedly harsh picture of the potentialities of psychometric and information-processing research for the study of psychological processes. Actually, much of this research lends itself more readily to solid inferences about processes than I have led the reader to think. Processes have a distinct relation to the requirements and characteristics of a particular task; just as in the Sternberg and Frederiksen studies, the tasks studied in both the psychometric and the information-processing literatures have characteristic requirements and constraints that enable one to make fairly confident inferences concerning the processes that can and cannot occur in performing those tasks. For example, in the several tasks loaded on Factor A of my reanalyzed factor analysis of the Jarman and Das (1977) data (Table I), it is obvious that some kind of apprehension and matching of spatial forms must occur for a subject to be successful in performing the task. At the same time it is obvious that use of long-term memory for retrieving historical dates, say, is irrelevant and useless in performing these spatial tasks. Even if individual differences are inextricably linked with processes, individual difference methodologies should enable us to narrow down the kinds of processes associated with particular tasks, and to investigate the generality of those processes over different tasks. This requires only a systematic effort to vary tasks in such a way that the relevant individual difference traits and the processes asssociated with them are adequately defined in terms of task characteristics. (This is what factor analysts, in fact, have said all along; unfortunately there are few examples of a systematic series of factorial investigations, other than Guilford's (1967; Guilford & Hoepfner, 1971)perhaps, that demonstrate the utility of this investigative strategy.)

Secondly, it is possible in principle to distinguish processes that are required by a task from processes that are optional. This distinction is seldom observed in psychometric individual difference research, and it appears with low frequency also in the information-processing research that I have surveyed. In psychometric research, the significant loading of a variable on a process-related factor tell us only that on the average, subjects tend to use the process that is associated with the factor, rather than some other process that might be effective in performing the task, if indeed there are several alternative optional processes. Earlier, I cited Thurstone's observation that factor analysis cannot tell us which of several alternative processes might be used by a subject in performing a task. In information-processing research, there are few investigations of alternative processes in performing tasks, although one example that comes to mind is Groen and Parkman's (1972) demonstration of alternative processes (counting vs. use of addition facts) in children's solution of arithmetic problems. It may be useful to call optional processes "strategies," restricting use of the term process for those that are required by the task. It was with this distinction in mind that I (Carroll, 1976) analyzed the tasks represented by the French, Ekstrom and Price (1963) Kit of Reference Tests for Cognitive Factors in terms of operations (processes required by the task) and strategies, although I may have failed to observe the distinction with sufficient rigor.

It also occurs to me to mention that in the study just referred to, I was careful to distinguish between contents of the memory stores that are operated on and the operations and strategies that perform the processing. In varying tasks to observe the functioning of processes and strategies, it will be important to control or otherwise take account of differential contents of memory stores that may be involved in tasks. Many of the factors isolated in traditional psychometric research seem to be associated with particular content constellations, i.e. with the presence or absence of, or with the quantity and type of, information that is processed. Information-processing research can similarly fall into the trap of failing to distinguish between processes and the content of the information that is processed.

As an illustration of a promising methodology that has thus far been little used in individual difference research in an information-processing mode, I may refer to a study by C. Frederiksen (1969). This study has apparently received little attention among information-processing theorists, although it has been hailed by Messick (1972, p. 368) as a "milestone study in [the] multivariate experimental probing of complex learning processes." Frederiksen studied college students' learning of a 60-word list, under three experimental conditions: (a) a standard serial anticipation task, (b) a "clustering" task in which the words were artificially presented in clusters of five, and (c) a free-recall task in which the 60 words were presented, as it were, all at once. There was a separate group of 40 cases assigned to each experimental condition. The response data consisted of the scores on each of the 18 trials that were given to each group; these learning curve data were, however, transformed into five components that explained nearly all the variance of the scores according to a principal-component technique for such data devised by Tucker (1966), a technique that has also been used by Leicht (1972) in the analysis of free-recall data. Immediately after the learning task, Frederiksen administered to his subjects a questionnaire on the strategies they thought they employed in performing the learning; these data were also reduced to a series of five components, representing typical strategies such as "Organization by Grouping," "Active Sequential Organization," and Modification of Strategies." Frederiksen also administered his subjects a series of psychometric tests that yielded scores on seven of the factors represented in the French, Ekstrom, and Price (1963) Kit of Reference Tests for Cognitive Factors. Thus, Frederiksen had three sets of data: the psychometric tests, the strategy scores, and the learning component scores. Using mainly canonical correlation analysis, Frederiksen determined relationships between these sets of data in each of the experimental groups pairwise, i.e., between the ability tests and the learning components, between the ability tests and the strategy components, and between the strategy components and the learning components. Although there were some significant relationships, they showed no very clear pattern overall. Different experimental conditions elicited markedly different strategies, but the effects of the strategies showed up only weakly and inconsistently in the learning components. In the main, the only ability factor that showed reasonably consistent and substantial relationships with learning component data was the associative memory factor Ma; but then, it might be said that such a relation could only be expected, since the memory tasks in the test and in the learning trials had major similarities.

This study by Frederiksen has its frustrating aspects. The sample sizes (40 in each group) were relatively small, and it is difficult to interpret the meaning of the learning curve components. In fact, Frederiksen suggested that "the amount of information about human learning obtainable from the behavior of learning curves may be limited, and that precise prediction of learning performance curves may not be the most important function of a learning theory" (p. 68)--a sentiment echoed by Leicht (1972), among others.

Strangely, Frederiksen apparently never thought to put together his ability factor data and his strategy component data to establish predictors of the learning component data. It occurred to me to do this, using the data reported in his monograph. I hoped to find linear combinations of ability factor scores and strategy component scores such that the combination would predict a learning component score significantly better than the use of either alone. I investigated the five ability scores (<u>Cs</u>, <u>Fa</u>, <u>Fe</u>, <u>Ma</u>, and <u>V</u>)and the two strategy components (3, Active Sequential Organization and 4, Order Preserving Mnemonics) that seemed to show the stronger and more consistent predictive relations with learning components, and put these in multiple regressions for predicting each of the five learning components, in each of the three experimental groups. In all, there were fifteen multiple regressions. As one may see, this was purely a fishing expedition. But one can enjoy a fishing expedition even if it is unproductive, as this one was. About the closest I came to catching the kind of fish I wanted (one where an ability factor and a strategy component would each have a significant regression weight) was in the case of the Free-Recall group, where Strategy 4, Order Preserving Mnemonics, had a clearly significant weight ($t(32)=3.17$, $p < .01$), but factor Fa, Associative Fluency, had a weight that only approached significance ($t(32)=1.98$, $p < .10$), in the prediction of Learning Component III. If this finding is regarded as having significance, it would mean that people with high Associative Fluency do particularly well in a certain phase of free recall learning if they adopt a strategy of using "order preserving mnemonics," such as trying to make sentences from the words presented, rather than merely attending to their sounds. Actually, in this group, there was a negative correlation ($r = -.34$) between Fa and the use of this strategy.

Although my fishing expedition was unsuccessful, it illustrates, as does the Frederiksen study as a whole, the potential use of a methodology whereby information about both abilities and strategies might be collected to predict or account for differences in task performance. Possibly one condition that mitigated against obtaining significant results in my fishing expedition was the use of a purely linear model; I was of course limited to such a model by the data available to me in Frederiksen's publication. Some sort of non-linear model that would simultaneously predict the probabilities of using alternative strategies on the basis of ability scores and predict performance data on the basis of strategy selection and ability scores would seem to be desirable.

Conclusion

After this critical examination of a range of studies that employ correlational methodologies in the study of individual differences in information processing, these methodologies, especially factor analysis and its relatives, can be judged still viable and effective procedures. If studies are designed to exploit the full potentialities of these methodologies, and if the methodologies are properly applied, it seems possible to arrive at reasonable and probably confirmable conclusions concerning the identification and description of a variety of cognitive operations. Thus far, however, many errors have been made in applying these methodologies. Even when they have been correctly applied, their potentialities have not been fully exploited due to poor or inadequate study design.

Results thus far have suggested the existence of a series of cognitive processes that show correspondences to individual difference traits, but the stability and the generality of these traits have not been firmly established. There has been little attention to the possibility that individuals can use alternative, optional processes or strategies in test performance, and to the possibility that these processes or strategies are amenable to manipulation through variation of task characteristics, instruction, practice, or other maneuvers.

Although the state of the art in individual difference research in an information-processing mode can be thought of as little more than embryonic, this type of research appears to have a promising future if the recommendations made in this review can be followed, and if logistic considerations are not insuperable.

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