THE LOGICAL BASIS OF FACTOR ANALYSIS

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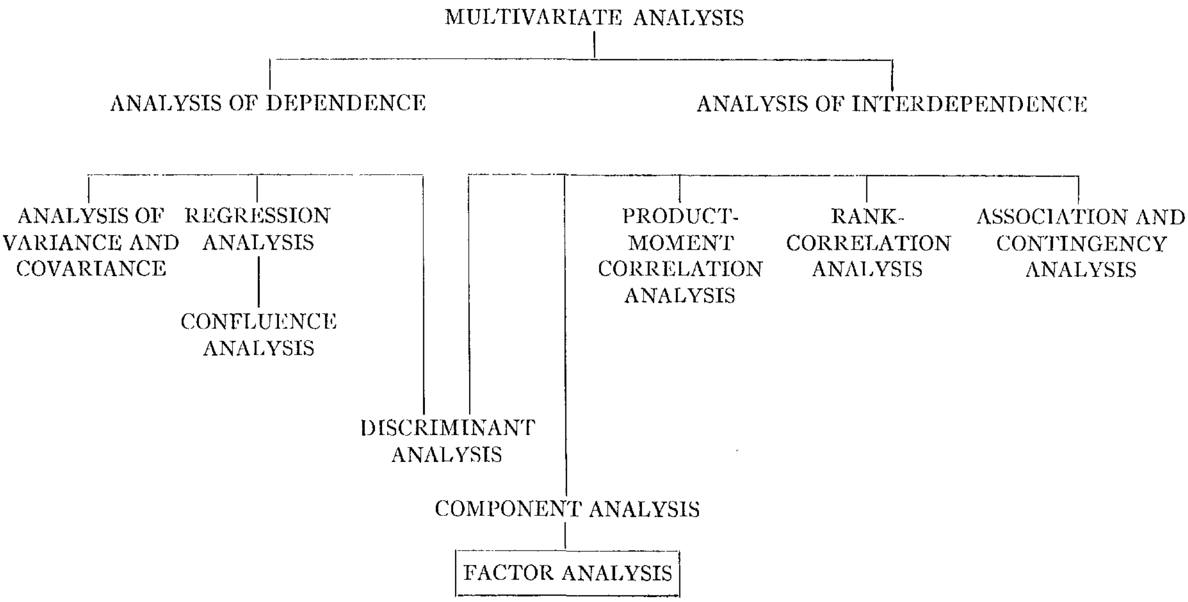
FEW methods of statistical analysis have encountered as much resistance among both statisticians and psychologists as has factor analysis. In addition to this critical attitude towards factor analysis as a whole, there is an internecine quarrel amongst practitioners which has split the whole field into schools, factions, and individual snipers. Is this turmoil due to any inherent flaws in the method, or is it due to some confusion about aims and techniques? It will be the burden of this paper to show that the latter possibility is the more likely cause, and to attempt the construction of a rational scheme into which all the existing methods of analysis can be fitted. The stress is throughout on the *logic* as opposed to the *mathematics* of factor analysis; disputes about the latter are much less fundamental and much more easily settled than discussions about the former.

In assigning a place to factor analysis in the general field of statistics, we may with advantage follow Kendall (12, p. 60), who draws a distinction between analysis of *dependence* and analysis of *interdependence.*

> In the latter we are interested in how a group of variates are related among themselves, no one being marked out by the conditions of the problem as of greater prior importance than the others, whereas in the analysis of dependence we are interested in how a certain specified group (the dependent variates) depend on the others. The distinction is perhaps seen at its simplest in the bivariate case: correlation between two variates is a matter of interdependence, and is a symmetrical relationship between them; the regression of one on the other is a matter of dependence and is not a symmetrical relationship—the regression of x on y is not the same as the regression of y on x.

The position of factor analysis in the group of techniques using analysis of interdependence is shown in the accompanying figure quoted from Kendall (12, p. 61).

I have said: "*The* position of factor analysis," but the use of this clause suggests erroneously that there is one technique, one method, and one aim underlying the quite variegated activities of factor analysts. In actual fact, there are three main aims which factor analysts try to achieve, three main views regarding the nature of factors which are closely related to these aims, and a large variety of methods of extraction and techniques of rotation.



If we would understand these many different approaches, we must start with a statement of the questions which factor analysis is trying to answer. This is particularly important because of the tendency of many critics to reject the factorial answer to a certain question, not because the answer is inadequate, but because the question is misunderstood.

This point is well made, in quite another connection, by the philosopher Collingwood who writes:

> You cannot tell whether a proposition is "true" or "false" until you know what question it was intended to answer . . . a proposition which in fact is "true" can always be thought "false" by any one who takes the trouble to excogitate a question to which it would have been the wrong answer, and convinces himself that this was the question it was meant to answer. And a proposition which in fact is significant can always be thought meaningless by any one who convinces himself that it was intended as an answer to a question which, if it had really been intended to answer it, it would not have answered at all, either rightly or wrongly (5, p. 30).

The three aims of factor analysis are the same three aims which give rise to other branches of statistics. As Kelley (13, p. 22) puts it: "The first function of statistics is to be purely descriptive, and its second function is to enable analysis in harmony with hypothesis, and its third function to suggest by the force of its virgin data analyses not earlier thought of." Kelley makes clearer his second and third points by adding: "We may say that there are two occasions for resort to statistical procedures, the one dominated by a desire to prove a hypothesis, and the other by a desire to invent one" (13, p. 12). We may exemplify this threefold use of statistics by reference to an example. We find that in a given population there exists a correlation of .6 between height and weight; this fact serves to *describe* this population in just the same way that the mean height or the mean weight would be descriptive constants characterizing this population. We find that in a given population there exists a correlation of .2 between height and intelligence; this is also descriptive of this population, of course, but it may in addition suggest a hypothesis to us—the hypothesis, for instance, that favorable environmental circumstances are conducive to greater bodily height and to better performance on intelligence tests. This hypothesis may suggest to us that intelligence should also be correlated with weight (deduction 1), and that greater educational homogeneity within the group over which the correlation between height and intelligence is being run should reduce that correlation (deduction 2). We therefore calculate further correlations to prove or disprove our hypothesis—disprove in this case, because both deductions are falsified by the facts—thus using statistics to carry out "analysis in harmony with hypothesis."

Most psychologists and statisticians are aware, either explicitly or more frequently implicitly, of these three uses of statistics; it is in their application to factor analysis that problems of communication arise. I shall therefore discuss the use of factor analysis at these three levels in some detail, giving a formal definition of the term "factor" appropriate to each level.

Factors as descriptive statistics. Whatever else may be the function of a factor, it is always descriptive of a given sample or population. It is small wonder, therefore, that many definitions of factor analysis stress this point to the exclusion of any other. Thus Holzinger and Harman (11, p. 1) write: "Factor analysis is a branch of statistical theory concerned with the resolution of a set of descriptive variables in terms of a small number of categories or factors. . . . The chief aim is . . . to attain scientific parsimony or economy of description." Similarly Kelley (15, p. 120): "[Factor analysis] represents a simple straightforward problem of description in several dimensions of a definite group functioning in definite manners." We may thus arrive at the following definition: *A factor is a condensed statement of (linear) relationships obtaining between a set of variables which can be used mathematically to stand for these variables.*

There is no implication in factors so defined of any psychological meaning, of any causal implications, or of any hypotheses, either suggested or proved. Few psychologists have found this view of factor analysis very attractive, and examples of its use are few and far between. Probably the best known is the work of Adams and Fowler (1) and of Kelley (14) on vocational interests. Correlating 35 interests on 800 men, they extracted five factors which accounted for all the significant covariation. These factors were not rotated or in any way interpreted, but were simply given meaningless names based on the initial letters of some of the interests having high loadings on each factor. Thus factor "NEVCOM" contrasts nature-loving, religious, and salesmanship with power, mechanical, spatial, orderliness, verbal, and musical interests. The fact that psychologically these factors make no sense would not be a correct criticism of the analysis which was intended to be purely descriptive; descriptively these nonsense factors are as good as any other set. The obvious convenience of having five factors instead of 35 variables in a regression equation, for instance, is undoubted, and may furnish justification for this very limited type of analysis. (We would of course lose whatever contributions specific factors might have made, possibly a very serious loss indeed.)

Factors suggesting a hypothesis. A factor, however it may have been obtained, may suggest a hypothesis to the investigator. In so far as it does that, the factor ceases to be merely descriptive and becomes part of theoretical psychology. We may give a formal definition of factor analysis from this point of view and say: *A factor is a condensed statement of (linear) relationships obtaining between a set of variables, suggestive of hitherto undiscovered causal relationships.*

As an example of this type of approach, contrasting with Kelley's analysis mentioned above, we may cite Thurstone's (22) analysis of the intercorrelations between 18 of Strong's interest scales. Thurstone emerged with four factors, labelled interest in science, interest in language, interest in people, and interest in business. Later work, summarized elsewhere (9), has on the whole confirmed Thurstone's analysis, and there is little doubt that the hypothesis suggested to him by the analysis has psychological meaning, and fits into psychological theory, while Kelley's analysis does neither. In other words, Thurstone's work adds to the merely descriptive function a psychological hypothesis which appears reasonable on a priori grounds, and which can be checked and submitted to experimental verification or disproof.

It may be argued that hypotheses can be generated in other ways than by factor analysis. That of course in undeniable, and there is no guarantee that factorial hypotheses will be superior to hypotheses arrived at by simple observation, by theoretical analysis, or even by *Schreibtischexperiment.* As an example of a hypothesis thus derived, we may quote Spranger's (20) purely theoretical analysis of interest patterns into the theoretical, economic, aesthetic, social, political, and religious. A factorial investigation of this hypothesis by Lurie (16) gave rise to four factors: social, theoretical, religious, and what he calls Philistine, i.e., combining the economic and political interests, and opposing them to the aesthetic interests. This investigation thus gives results in line both with Thurstone's and with Spranger's hypotheses, but as Lurie's work is essentially an example of factor analysis supporting or disproving a hypothesis, no more will be said about it.

While it must be admitted that hypotheses may be formed in a variety of ways, the factorial method has one definite advantage. It provides *ab initio* data relevant to the formation of such hypotheses, and it rules out a large number of possible hypotheses which might otherwise have been entertained. Something similar, of course, is done when hypotheses are based on a simple observational study; indeed, it will be argued later on that much observational and clinical work is essentially similar to factor analysis in principle, though inferior to it because of its lower degree of rigor and accuracy. In a well-studied field, there are probably enough well-documented observations to make hypothesis-formation easy; in a relatively new field, the help of factor-analytic methods may be very important in accelerating the formation of reasonable, worthwhile hypotheses, and in discarding poor ones.

Factors supporting or disproving a hypothesis. It is obvious that factor analysis cannot be used as a formal part of the hypothetico-deductive process in relation to just any type of hypothesis. The great majority of psychological hypotheses require some form of analysis of dependence, and thus rule out the factor-analytic approach. But there are a number of hypotheses, particularly those concerned with structure and organization, which require factor-analytic methods, and which are difficult at the moment to disprove or support by nonfactorial methods. All type- and trait-hypotheses, for example, fall into this category, as I have tried to show elsewhere (6, 9), and even Freudian theories have shown themselves amenable to the factorial method of proof (10).

Our definition of a factor as supporting or disproving a hypothesis follows directly from these considerations. We may say that *a factor is a condensed statement of (linear) relations obtaining between a set of variables which is in agreement with prediction based on theoretical analysis.*

Such predictions may be of varying degrees of exactitude. We may predict merely that certain items or tests will be found to have positive projections on a factor, while other items or tests will have negative projections; this is the most elementary level of prediction. Much more refined prediction is possible in relatively well-studied fields. Thurstone would probably be able to specify with considerable precision the position of a newly constructed test in the cognitive multi-factor space, or to construct a new test to specification, i.e., to lie at a particular place in the multi-factor space. The writer has been able to write social-attitude items to specification within a defined two-factor space with negligible errors. Many other examples of relatively precise predictions could be given; the majority of cases, however, would undoubtedly be at a much lower level of precision.

Frequently psychologists fail to state the exact nature of their hypotheses, and discuss their findings as if they had selected their tests at random, without any kind of hypothesis in mind. Occasionally such blind empiricism does seem to lie at the back of factorial work; factor analysis is sometimes used as a last resort to try and rescue worthless data accumulated at random from the fate such data so richly deserve. It need hardly be said that such use of factor analysis is valueless, but the fact that it occurs should not be used as an argument against the method as such; similar faulty use may be the fate of all statistical methods. It is probably safe to say that in the great majority of cases items and tests are included in a factor analysis on the basis of fairly specific hypotheses which are seldom verbalized in the write-up of the experiment because (*a*) lengthy discussions would be required, which most editors would refuse to print, and (*b*) results are already available to show which hypotheses have been verified, so that there seems little point in discussing those which have been disproved.

It will have been noticed that in passing from the purely descriptive use, there has been a definite change in the implication of the term *factor*. For Kelley, there is no causal reference implied in a factor; for Spearman, Thurstone, and those who follow their methodology there quite clearly is such a reference. This causal implication characterizes not only the interpretation of factors as suggestive of a hypothesis, but also the next level of factors as proving a hypothesis, and since from the psychological point of view this causal implication is precisely what lends interest and value to factor analysis, it may be opportune here to give a definition of a factor which brings out this element. We may therefore offer the following definition: *A factor is a hypothetical causal influence underlying and determining the observed relationships between a set of variables.*

This definition serves a useful function in drawing attention to the close link between the hypothesis-generating and the hypothesis-proving functions of factor analysis, as opposed to the purely descriptive. It will often be found that in one and the same investigation there will be factors which support a hypothesis and factors which generate one. Elsewhere (6) a factor analysis has been reported of a large matrix of neuroticism tests designed to test a hypothesis regarding this particular factor. The analysis did indeed confirm the hypothesis; it also gave rise, however, to another factor which suggested that pencil-and-paper, verbal-type tests are separated in a clear-cut fashion from nonverbal, objective-behavior tests. Such a verbal–nonverbal factor, well known in the cognitive field, suggests various hypotheses which require testing; thus we find in the same analysis confirmation of one hypothesis, and suggestions for further hypotheses. This mutual stimulation between proof and suggestion might indeed be regarded as a prominent feature of factor-analytic work, and may recommend the method to those used to this interplay among hypothetico-deductive lines in other sciences.

While this interplay of proof and suggestion is valuable and important, it has often led to interpretations highly vulnerable to criticism. As has been pointed out elsewhere (7), it has been one of the worst abuses of factor analysis that practitioners have often carried out an analysis *suggesting* a hypothesis, and have then gone on to argue that their analysis has *proved* this hypothesis. The distinction is fundamental, and much of the criticism often made of factor analysis is ultimately referable to this failure to be clear about the status of the factors isolated.

We have discussed so far the *aims* of factor analysis; we must now turn to the *nature* of the factors isolated. A factor may be regarded as a purely statistical concept, an "artifact" if you like, akin to an average, a variance, or an epsilon. This view of the nature of a factor clearly corresponds with the descriptive aim as outlined above. As such, the concept is clear and does not require further discussion.

Secondly, a factor may be regarded as a principle of classification. This is the view of Burt (3), who likens factors to lines of longitude and latitude. According to this view, factor analysis first removes whatever is common to all the tests or items correlated, and then proceeds by means of a series of bipolar factors to disclose the principal ways of classifying the material under discussion. This is done without rotation of axes, it being assumed apparently that the principles of classification would remain invariant under change of tests or items correlated. This assumption is almost certainly mistaken in the majority of cases, but does not seem to be an indispensable part of this view of the nature of factors.

The third way of looking at factors is to regard them "as if" they were causal agencies. This view is implied in the definition given of the term "factor" a little while ago; it is given clear expression by Thurstone (22, p. 54), who writes: "One of the simplest ways in which a class of phenomena can be comprehended in terms of a limited number of concepts is probably that in which a linear attribute of an event is expressed as a linear function of primary causes." Spearman's (19, p. 75) view is similar: ". . . if meaningful as opposed to statistical, a factor is taken to be one of the circumstances, facts, or influences which produce a result."

There are two main criticisms of this view. Some writers hold the view that even if such causes could be identified in mental life, factor analysis could still not be relied upon to identify and isolate them. Thus Kelley, continuing the quotation in which he defined factors as being purely descriptive, says: ". . . he who assumes to read more remote verities into the factorial outcome is certainly doomed to disappointment" (13, p. 22). Burt (3, p. 231) objects to the use of causal terms on philosophical grounds; he admits, however, that in certain cases "the language of causation is not only convenient, it is almost unavoidable, if we are to remain comprehensible." His main objection appears to be not to the language of causation as such, but rather to the reification of factors.

It is at this point that we encounter the central problem in our quest for the logical basis of factor analysis. It is here, also, that most critics have claimed to find the most vulnerable spot in the armour of factor analysts. If a given factorial solution is "purely arbitrary," just one of an innumerable number of possible solutions, and if it carries no causal implications, then it appears to many critics to differ fundamentally from other types of mathematical solutions, and to give rise to concepts much more insecurely based than those in other sciences.

This type of criticism appears to be based on a profound misunderstanding of the nature of scientific laws and concepts. As Thurstone (22, p. 51) points out:

> . . . the constructs in terms of which natural phenomena are comprehended are man-made inventions. To discover a scientific law is merely to discover that a man-made scheme serves to unify, and thereby to simplify, comprehension of a certain class of natural phenomena. A scientific law is not to be thought of as having an independent existence. . . . A scientific law is not a part of nature. It is only a way of comprehending nature.

In a sense, therefore, the concepts and laws to which factor analysis gives rise are "statistical artifacts"; they are so in the same way that all other scientific concepts and laws are "artifacts." Spearman's *g* (general intelligence) is a statistical artifact to precisely the same extent, and for the same reasons, that Newton's *g* (gravitational force) was a mathematical artifact. Neither has any actual existence, in the sense that a falling stone or an individual who is acting intelligently can be said to exist; both concepts are abstractions which serve to unify and simplify a complex class of phenomena, and both had to be discarded or amended when new facts showed them to be incapable of accounting for all the phenomena. It does not appear reasonable to criticize factor analysis for showing features which are characteristic of all science.

Nor is the alleged "subjectivity" of factor analysis absent in universally accepted forms of dimensional analysis in physics. The physicist Bridgman (2, p. 1) points out that "there is nothing absolute about dimensions—they may be anything consistent with a set of definitions which agree with experimental fact." And Scott-Blair (18) has given an example of alternative dimensional analyses of the phenomena of heat. We may, therefore, dismiss this criticism also as applying to all science equally, rather than just to factor analysis. All science, in a sense, is an "artifact" and "subjective"; the important point is that this artificiality and subjectivity are closely circumscribed by the need always to remain in accord with the facts science sets out to unify and simplify. Those who have had experience in trying to formulate a hypothesis, whether factorial or otherwise, which would account for a large number of different phenomena will not usually be worried about having to choose one of a very large number of such theories; they will be thankful indeed if even one theory can be found which is not decisively contradicted by several indisputable facts. Psychologists sometimes tend to overcome this difficulty by disregarding those facts not in accord with their particular theory; there is little in the history of science that would encourage such a policy of neglect.

Granted that the most usual objections to factor analysis and the "causal" status of factors are invalid and based on an imperfect understanding of scientific methodology as a whole, our argument cannot be based entirely on disproof of criticism; it would seem desirable to argue more directly from positive evidence. There are four such proofs. The first proof relates to conditions where the causal relations are relatively well understood, and where we can compare the results of factor analysis with independent knowledge of the conditions responsible for the results. An excellent proof of this nature is supplied by the outstanding work of Wenger (24) on the "autonomic imbalance" factor. Following Eppinger and Hess, whose theory of "sympatheticotonia" postulated a predominance in certain subjects of sympathetic innervation, Wenger gave a battery of tests involving measures of the effects of autonomic innervation to various groups of subjects, including children and normal and neurotic adults, and carried out a Thurstone-type factor analysis of the resulting intercorrelations. Simple structure revealed in each of several analyses a clearly marked factor of "autonomic imbalance," having high saturations in the predicted direction on the predicted variables. Here we have an intelligible "cause" underlying the observed correlations, and the coincidence of factor saturations with theoretically predicted pattern is surely too striking to be ascribed entirely to chance. It may be noted incidentally that until taken up by factor analysts the Eppinger-Hess theory lay dormant, except for theoretical discussion, for some thirty years, because no other statistical-experimental procedure lent itself to the investigation of this type of hypothesis.

The second type of proof relates to the simultaneous change of scores on all the tests defining a factor when the hypothetical physiological basis of that factor is experimentally altered. As an example we may quote the work of Petrie (17) on the after-effects of lobotomy in neurotic subjects. Basing her work on the hypothesis that patients after operation showed changes on the factors of neuroticism and extraversion-introversion along the lines of *decreasing* amount of neuroticism and *increasing* amount of extraversion, she administered before operation two sets of six tests defining these two factors respectively, and predicted the direction in which change would take place. In all cases tests carried out after operation verified the prediction; in other words, all the tests defining the factor of neuroticism showed changes in the direction of lessened neuroticism, and all the tests defining extraversion showed changes in the direction of increased extraversion. This dynamic proof for the functional unity and biological reality of the factors in question is particularly impressive because of the paucity of statistically significant changes on personality tests previously reported in the literature.

The third method of proof is based on the following argument. It is possible to calculate an approximate index of hereditary determination, such as Holzinger's h^2, for any test which has been applied to a sufficiently large sample of identical and fraternal twins. It is difficult to see how a factor, which is merely a linear combination of test scores, can have a higher h^2 than any of the constituent tests, unless this factor is based on some very definite, underlying biological reality or function which is itself inherited. There is at least one study (6) in which it has been shown that the factor of neuroticism has a higher h^2 (.810) than any of the constituent tests which are combined to give that factor score; the highest individual test h^2 was .701. It seems difficult to dismiss as "subjective" and as a "statistical artifact" a factor having such very definite and obvious relation to biological reality.

The last method of proof suggested here is more indirect than the others, but logically equally important. Factor analysis is often considered to be a complete innovation, something different from, and possibly even contrary to, the usual methods of scientific investigation. It is the burden of this paper to point out that quite on the contrary methods logically identical with factor analysis, though mathematically less exact and rigorous, have been used from the very dawn of science to deal with the type of problem involved in the study of "interdependence." In doing so, they have led on to hypotheses regarding "causes" and to analyses of "dependence" which have greatly clarified the field, and which would have been impossible without the preceding "factorial" investigations. As this point is crucial to my argument, I shall give two examples of what I have in mind.

Let us take first of all the concept of disease. If we take a particular disease, such as tuberculosis, we know now that it is caused by an identifiable "cause," namely the tubercle bacillus, acting on a human body which may vary from case to case in its resistant properties. This particular disease, however, was known and isolated long before the "cause," the bacillus, was discovered; indeed, unless the disease had already been known as a unitary entity it is difficult to see how its "cause" could have been discovered. How, then, was the disease identified? It was identified essentially in factorial terms, i.e., by the fact that certain physical symptoms—loss of weight, breathing difficulties, high temperature, coughing-up of blood, etc.—tended to go together (intercorrelate) in a certain manner as a "syndrome." No symptom by itself is decisive (none is factorially pure), but the syndrome (factor) suggests one underlying cause which gives rise to the various symptoms, and which may sometime be identified. In a similar way, we identify mental diseases in what is essentially a factorial manner, i.e., in terms of the observed intercorrelation of various symptoms; anyone reading Kraepelin or Bleuler will be able to follow this process in its clearest and most obvious manner. In the mental field we have not yet discovered the underlying cause of the various patterns we observe; until we do we have to rest content, as we had to in the case of physical medicine before the advent of Pasteur, with syndromes (factors). All that factor analysis does is to make explicit and rigorous what the clinician does in any case, often implicitly and without full understanding of his methodology. Both the clinician and the factor analyst may be mistaken, and group together what does not in fact (causally) belong together; medical history indicates a number of errors as well as a remarkable number of successes in this preliminary method of grouping together symptoms in terms of underlying "diseases." It seems reasonable to assume that greater rigor and awareness of the pitfalls involved may decrease the number of errors; there is no way of guaranteeing complete success. The point to stress is that this "factorial" stage is an indispensable preliminary to the "causal" stage; our factor or syndrome tells us what symptoms go together in such a way that we can with some hope of success go on to look for a single underlying cause.

My second example relates to the field of taxonomy in flora (21) and fauna (4). Until the advent of Darwin and the theory of evolution, the only way of telling "what goes with what" in the plant and animal kingdoms was by means of morphology, i.e., by noting degrees of similarity of a large number of outwardly observable characteristics. Thus, specimens agreeing on a large percentage of such characteristics (correlating highly together) were considered to be closely related; specimens agreeing on a small percentage of characteristics only (correlating together at a low level) were considered to be only remotely related. By means of an implicit and nonrigorous factor analysis of these similarities or correlations the whole elaborate system of 19th century systematics was built up. The theory of evolution made it possible to check the resulting taxonomic picture with its implied causal influences against the directly observable causal development shown by Darwinian research. The remarkable result was that in its main outline the picture required very little change; there were many details which had to be modified, but by and large morphology had been an extremely accurate guide to causal relations (4, 20). So here also we find that subsequent causal "analysis of dependence" verifies in considerable detail the results of "analysis of interdependence" carried out along essentially factorial lines. And again, the advances made by Darwin would not have been possible without the spade work of the "systematists" and their implicit factorial approach.[1]

[1] It might be pointed out that factorial logic plays a part even in such apparently remote fields as in the definition of a metal. A metal is electropositive, forms metallic crystals, its halides generally form ionic aggregates and are non-volatile, but give conducting solutions in water, and its oxides are usually basic. There are, however, exceptions to these rules. Thus $SnCl_4$ is a volatile liquid; ZnO and Al_2O_3 are amphoteric, and some higher oxides such as CrO_3 are acid. Graphite, arsenic, and tellurium, on the other hand, exhibit metallic properties, while counted among the non-metals. The concept "metal," therefore, rests on the intercorrelation of the various indices enumerated; these correlations are far from perfect, and the only reason for using the term "metal" is the logical implication of a fundamental common feature which unites all metals in a group, and sets them off in comparison with the other elements. This may be an unusual way of looking at chemical concepts, but the logical similarity of derivation is too striking to be passed over.

It may seem fanciful to regard these time-honored methods of analysis as similar in essence to modern factor analysis; yet it would be difficult to deny the essential identity between past and present as long as we consider the logical basis of the procedures involved. It is widely recognized that the correlation coefficient is merely the statistical expression of what Mill called the "method of concomitant variation," and Mill's fifth canon—"Whatever phenomenon varies in any manner, whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation"—certainly preceded in time the statistical superstructure erected on this logical foundation. It is difficult, therefore, to see why a similar process of growth from logical implication and nonrigorous use to statistical elaboration and explicit formulation should not have taken place with respect to factor analysis.

We may conclude, then, that there is both direct and indirect evidence that factorial procedures may lead to genuine causal determinants. It would not be reasonable to say that such a happy outcome would inevitably attend the application of factor analysis; under certain circumstances it may be predicted with confidence that no causal hypotheses will be suggested or proved by factorial methods. In part the outcome of a factorial investigation is determined, of course, by such imponderable factors as the sagacity of the investigator, his skill in framing hypotheses and constructing tests, and his desire to use factor analysis as a hypothetico-deductive tool, rather than as a purely descriptive method. But in part there is no doubt that the actual method of analysis itself will determine the outcome.

At the purely descriptive level, there is little to choose between the methods of analysis advocated by Hotelling, Tryon, Thurstone, or Kelley; the slight advantages of "principal components" are offset by the greater ease of computation of "centroids," and so forth. It is when we come to the problem of rotation that the crucial step occurs. It is clear that we cannot expect a factor to be related in any direct manner to a hypothetical cause unless the factor is unique and invariant in its derivation. Unrotated factors of any kind are usually neither unique nor invariant. This problem is disregarded by writers like Kelley and Burt because the aim for which they use factor analysis is not that of isolating causal determinants. It would appear, however, that Burt at least is not justified in regarding unrotated factors as giving rise to stable "principles of classification" any more than they give rise to causal determinants; they may occasionally bear a superficial resemblance to such more stable factors but logically a Burtian solution is at the same level of pure *ad hoc,* elementary description as is Kelley's.

If we wish, then, to obtain factors which are not ruled out *ab initio* from fitting into a general descriptive-causal scheme because of lack of uniqueness and invariance, rotation becomes necessary. Here the only scheme which deserves serious consideration is Thurstone's suggestion of rotation into "simple structure," with its attendant concepts of "oblique factors" and "second-order factors." I have on occasion been somewhat critical of Thurstone's earlier work, and it is only right to say here that his recent amplification and development of the more rigid framework of "Vectors of the Mind," together with experimentation of my own, have led me to a reversal of this attitude, into almost complete agreement with Thurstone's latest position. Logically, his method of rotation and experimentation generally amount to this. If we can treat our test domain as if its communality were due to a small number of isolable causes, then our best way of isolating and measuring these causes is by purification, i.e., by selection of tests whose variance is due, not to all these causes at once, but only to one or at most two. This should give us clear-cut differentiation and separation of factors; at the same time, the fact that such selection is practicable provides an impressive proof for the usefulness of the original assumption. Logically this argument seems faultless; mathematically, the scheme has not been worked out to perfection, but there seems to be no difficulty in principle.

In certain practical situations, the full Thurstonian procedure may not be practicable for various reasons, and when we have available an external criterion which embodies a certain hypothesis which we are interested in testing, the method of "criterion analysis" which I have described elsewhere (8) may serve as a substitute. This method appears particularly apposite in personality research outside the cognitive field, for reasons which also have been given elsewhere.

We must now pull together what of necessity has been a somewhat rambling discussion of a large number of related points. This summary can best be put as a series of numbered propositions; these are not meant to be taken as definitive in any sense, but they may serve to give some orientation to the very discursive criticisms of factor analysis which appear from time to time in the literature, and which are almost wholly concerned with the logic, rather than with the mathematics, of factor analysis.

1. Factor analysis is a mathematical procedure which resolves a set of descriptive variables into a smaller number of categories, components, or factors. These factors themselves, in the first instance, may be regarded as having a purely descriptive function.

2. Under certain circumstances, factors may be regarded as hypothetical causal influences underlying and determining the observed relationships between a set of variables. It is only when regarded in this light that they have interest and significance for psychology.

3. The logical justification for inferring a causal factor from observed correlations is identical with the general scientific justification for inferring causes from effects; more specifically, there is formal identity between factorial procedures in psychology and taxonomic and nosological work in other sciences (medical, botany, zoology).

4. The term "cause," in this context, is a concept which aids in the simplification and unification of natural phenomena; like all scientific concepts it is abstract and consequently an "artifact." A scientific concept is not a part of nature, it is rather a way of comprehending nature.

5. Factors, and the causal determinants which they suggest, are "subjective" in the same sense that physical dimensions are "subjective"; they "may be anything consistent with a set of definitions which agree with experimental facts." Their value and importance arises from the objective reference given them by this agreement "with experimental fact."

6. Criticism of factor analysis as a whole, or of one method of analysis by a writer favoring another method, is often vitiated by (*a*) lack of historical perspective, (*b*) lack of scientific sophistication, (*c*) lack of understanding of the particular problem which the factor analyst is trying to solve. It is usually easy for the critic to invent a problem which the analyst did not try to answer, but to which his answer would have been wrong or nonsensical. This is not a useful form of criticism.

7. The factorial method, no more than any other, cannot guarantee the correctness of the causal hypotheses suggested by it. Historical evidence reviewed suggests, however, that it is more successful than any alternate method, and that the hypotheses generated by it have proved remarkably accurate when direct experimental test became possible.

8. As indicated above, much nosological work in medicine and psychiatry is essentially of a factorial kind, although lacking the rigor and explicitness of factor analysis. It seems likely that a more formal use of these recent mathematical developments will improve more intuitive "clinical" types of analysis.

9. Methods of statistical analysis, and particularly questions of rotation, are dependent on one's views of the aims of factor analysis, and of the nature of factors; implications of causality require rotation into simple structure, while purely descriptive aims are satisfied equally by nonrotated factors.

10. In the present stage of development of psychology, factor analysis is an indispensable method of taxonomic and nosological research. Knowledge of its historical roots, its logical basis, as well as its statistical methodology, should form part of the training of every psychologist who wishes to understand the standard scientific method of defining concepts in personality research.

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