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How valid is the psychoticism scale? A comment on the Van Kampen critique

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Abstract

Van Kampen (1993) has published a paper in which he criticizes the concept of psychoticism and the use of the P scale as a measuring device, and advances measures for his own S scale and its validity. This reply attempts to clarify the notion of 'validity', and to demonstrate that there is a considerable body of experimental evidence to show that P has a high degree of construct validity, whereas the Van Kampen S scale lacks completely any degree of construct validity. Several examples are given of how the experimental evidence legitimates both the P scale, and the theory (nomological network) on which it is based.

In a recent paper, Van Kampen (1993) has criticized the Eysenck P scale, and produced some evidence in favour of an alternative S (schizoid or insensitivity) scale. Both the critique and the discussion of the new scale fail to convince because in both the concept of *validity* as understood by Eysenck is completely missing. Instead, we have reliance on what is essentially *reliability*. Thus the new scale is considered *valid* (p. 92) because it correlates negatively with a 'self-defensive test-taking attitude' scale (−0.40) and a 'friendliness' scale (−0.32); it also correlates positively (0.53) with a scale made up of MMPI items related to 'schizoid with a strong emphasis on hostility and aggression' (Van Kampen, 1993, p. 92). All that that means is that a scale constructed to reflect schizoid–schizophrenic behaviour correlates positively but rather poorly with another scale constructed to measure schizoid–schizophrenic behaviour, and very poorly with two scales constructed to measure negative aspects of schizoid–schizophrenic behaviour. This may demonstrate (rather weakly) *content* validity; it does not demonstrate *experimental* or *criterion oriented* validity. Above all, there is no evidence of *construct validity*, which is usually agreed to be the most important in the personality field.

As Pervin (1980, p. 403) points out: 'The relationship between personality theory and test validity becomes quite critical, however, in relation to *construct validity*. While in criterion-oriented validity a definite criterion (grades, brain damage, vocational success) can be established, in many areas of personality this is not the case. Some theories use constructs that are merely postulated attributes of people or theoretical concep-

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tions as to the qualities people possess (motives, drives, traits). Here there are no absolute criteria for the constructs; they are defined in relation to the theory of which they are a part. For example, there are no absolute criteria for the concept of introversion–extraversion, but the relevant criteria are part of the theory in which the construct is embedded'.

The construct validity of a test becomes more and more certain as the test is found to be useful in confirming relationships derived from a theory (Cronbach and Meehl, 1955). Just as a theory postulates a construct, such as anxiety, and assumes that this attribute can be found in people, so a test such as Eysenck's test of neuroticism is assumed to measure that construct. If the test is useful in research in relation to the theory, it gains construct validity. The history of the concept of construct validity in recent times, and its relevance to scientific research in personality and psychopathology, have been discussed in detail by Garber and Strasberg (1991), whose account will no doubt be familiar to anyone interested in this field.

In dealing with construct validity, we start out with an explicit theory, called by Cronbach and Meehl a 'nomological network'; this they define as an interlocking system of lawful relations that comprise a theory, and consists of both theoretical constructs and observable properties or operations. In other words, we cannot provide evidence of construct validity unless the concept we are concerned to measure is part of a more *general theory* from which *testable deductions* can be made. It is implicit that these testable deductions must go beyond the narrow scope of the construct's definition. In other words, if we start with a concept like psychoticism, we must be able to deduce from this construct predictions that link it in many different ways with the nomological network of which it is a part. This I have tried to do along many different lines (Eysenck, 1992a), and I will return to this point shortly. It is precisely the entire *absence* of such a nomological network from constructs such as 'agreeableness' and 'conscientiousness' that has caused me to criticize their postulation as two of the major dimensions of personality (Eysenck, 1992b; c). Agreeableness and conscientiousness are simply descriptive terms of certain elementary types of behaviour; the descriptions do not follow from any more inclusive theory, and they carry no deductive implications other than those implicit in their designation—i.e. that people having high scores should prove to be agreeable or conscientious. The only link with behaviour somewhat different from that contained in the terms themselves would be with synonymous or semantically related behaviour. But extraversion is causally linked with low cortical arousal, and there are literally hundreds of deductions which follow from that theory, creating a huge nomological network, many of whose predictions have been tested successfully (Eysenck and Eysenck, 1985). The same applies to neuroticism.

Does a similar nomological network exist as far as P is concerned, does it mediate a large number of predictions, and have they been successful in the main when tested experimentally? That, one might have expected, would have been the central theme of Van Kampen's (1993) paper, together with a similar analysis of his own construct, S. But there is not one word in his account dealing with this absolutely central aspect of the concept of psychoticism. If we can show it to be *valid*, then the frequent arguments about less than a perfect reliability and other psychometric weaknesses fall to the ground; validity is the major (I would say the only) aspect of a new measuring device that is of genuine scientific interest. Yet this major concern is missing from Van Kampen's account!

Let me mention just a few of the deductions made by myself, my colleagues, and

many researchers unconnected with my department. Consider what I have called the 'proportionality criterion', a development of my method of criterion analysis (Eysenck, 1950; 1952). According to this theory, if P is a measure of psychoticism, and *only* if it is a measure of psychoticism, then, if we take any test or measure that discriminates psychotics from normals, then score differences between psychotics and normals should be parallelled by similar score differences between high and low P scorers. Let us call psychotics S and normals N. Then the theory states that S:N = P+:P−. I have discussed a dozen or more attempts to test this deduction (Eysenck, 1992d), and nearly all have been successful. In the list of variables used to illustrate the proportionality criterion, I have on purpose included several different types of measures. One class deals with biological variables (HLA B27, MAO; serotonin). A second deals with laboratory behaviours (eye-tracking; dichotic shadowing; sensitivity levels). A third is concerned with learning–conditioning variables (latent inhibition, negative priming). Yet a fourth is concerned with psychological variables (creativity, hallucinatory activity; word association). Physiological variables (EMG, autonomic–perceptual inversion) constitute yet a fifth set of variables. It is the variety of variables which makes the results impressive, together with the theoretical congruence; to obtain successful results over such a wide array of variables suggests that the underlying hypothesis may be along the right lines' (Eysenck, 1992d, p. 77). One would have expected that Van Kampen might have mentioned this unique success of the theory and its associated measuring instrument, the P scale, in his evaluation, but any such hope would have been disappointed.

The general nomological network outlined above leads to many more deductions, some of them involving direct causal arguments of great force. Only one of these will be here considered, as an example, namely the theory of creativity (Eysenck, 1993; 1995). Consider Bleuler's (1978) description of the schizoid personality: 'He is taciturn or has little regard for the effect on others of what he says. Sometimes he appears tense and becomes irritated by senseless provocation. He appears as insincere and indirect in communications. His behaviour is aloof and devoid of human warmth; yet he does have a rich inner life. In this sense he is introverted ... Ambivalent moods are more pronounced in the schizoid than in others, just as he distorts the meanings of, and introduces excessive doubts into, his own concepts. But on the other hand, the schizoid is also capable of pursuing his own thoughts and of following his own interests and drives, without giving enough consideration to other people and to the actual realities of life. He is autistic. The better side of this autism reveals a sturdiness of character, and inflexibility of purpose, and independence, *and a predisposition to creativity*. The worse side of it becomes manifest in a lack of consideration for others, and occasionally even cruelty'. (My italics.)

There is here a fairly clear similarity to the P character, but note in particular the reference to *creativity*. There has been much evidence to support this notion (Eysenck, 1983), and if P is indeed a measure of psychoticism, then P should correlate with creativity. There are many studies to demonstrate that this is so. Woody and Claridge (1977) have found correlations (in the .60s) between P and divergent thinking tests of creativity, and Eysenck (1995) has found positive correlations between P and other measures of creativity, such as the word association test (remote associations) and the Barron-Welsh Art Test preference for complexity. Goetz and Goetz (1979a; b) found that leading German painters and sculptors had unusually high P scores. Many other studies are quoted by Eysenck (1993).

So far, this would merely be another example of the proportionality criterion, demonstrating that 'great wits are near to madness oft allied'. But our nomological networks allow us to take one further step. Creativity is characteristically linked with a shallow associative gradient, allowing the creative person a much wider choice of associations than would be available to the less creative person with a steep associative gradient (Mednick, 1962; Mednick and Mednick, 1964). Such a shallow associative gradient corresponds to the cognitive property of 'overinclusiveness' characteristic of schizophrenic thought disorder (Cameron, 1939; Cameron and Magaret, 1951; Payne and Hewlett, 1960). What causes such flattening of the associative gradient, and produces the symptoms of overinclusiveness? One strong candidate is *latent inhibition* (Lubow, 1989). Latent inhibition limits the range of attention to the most closely relevant variables, and hence should be absent from schizophrenics. The evidence shows that that is indeed so, and the obvious parallel has also been verified several times—high P scorers have significantly less latent inhibition, in experimental trials, than do low P scorers. The case is argued in much greater detail elsewhere, and the relevant literature cited (Eysenck, 1993; 1995); the point is that we now have a *causal* theory implicating P, and a series of studies demonstrating that all the relations between P, creativity, and latent inhibition are found to be substantiated.

We can take the matter one step further. If P is indeed related to schizophrenia, then theories of schizophrenia should give us a lead as to the variables that might give rise to high P scoring. Gray, Feldon, Rawlins, Hemsley and Smith (1991) have put forward a theory of schizophrenia, which, like many others, links it with dopamine function. Following up the obvious corollary, Gray, Pickering and Gray (1994) found a strong correlation between P and dopamine D2 binding in the basal ganglia, using single-photon emission tomography.

Without going into detail, it will now be obvious what is meant by the term *nomological network*, in connection with the isolation and definition of personality dimensions. It is only the existence of such a nomological network, together with a history of successful confirmations of deductions from the theory in question, that enables us to posit any particular concept as a major dimension of personality (Eysenck, 1991). To argue that Van Kampen's S factor can take the place of P in the complete absence of such a network, and without any evidence of construct validity, is not a serious undertaking. Similarly, to argue that agreeableness and conscientiousness should replace P as major dimensions of personality, in the absence of a nomological network or any evidence of construct validity, is to abandon scientific methodology completely. Agreeableness and conscientiousness, singly or in combination, *may* correlate with creativity (negatively), latent inhibition (positively), and dopamine functioning (negatively) because they correlate with P (negatively) and may be considered to be primary factors forming part of the group of primaries *defining* P, but in their own right they do not enable us to make any testable predictions having a theoretical background. To say that creativity is *caused* by lack of agreeableness and lack of conscientiousness would simply be an absurdity.

There have, of course, been other attempts to supplant P by other concepts and tests, particularly schizotypy and its various measures. I have recently looked at the relationship between these variables (Eysenck and Barrett, 1993). I analysed correlations between nine schizotypy tests, P, N, and a depression and an anxiety scale, reported originally by Kendler and Hewitt (1992). The outcome is of interest because it is relevant to Van Kampen's (1993) concern about the use of N and E to identify different

types of psychothymic disorder. Three clear-cut factors emerged, identified as P, E, and N by the high loading of these three scales (in spite of the fact that shortened versions of these scales were used, reducing reliability and hence the size of correlations with other variables). Most important was the fact that in a battery of schizotypy tests, the highest communality was shown by the P scale; in other words, whatever schizotypy scales have in common is best expressed by the P scale! It is also interesting to note that second in communality was the depression scale, suggesting that there is an important link between schizophrenia and depression, the fundamental idea encapsulated in the concept of psychoticism.

To summarize, Van Kampen (1993) has criticized the P scale, and suggested the validity of his S scale, with complete disregard for the universally recognized need for *construct validity*. His account of the P scale never mentions the large body of evidence supporting its construct validity, and his claim for the validity of his S scale simply refers to low correlations of his scale with other measures of similar concepts. Without evidence of construct validity, based on a properly formulated theory from which testable deductions can be made, personality traits and their corresponding questionnaire type measuring scales are merely correlational artifacts without a firm hold on reality. It is sad that fundamental notions, put forward by Cronbach and Meehl (1955) 40 years ago and accepted as guidelines by the APA, should still be disregarded. Even odder, it is over 80 years ago that Heymans conducted his epoch making studies of personality which in embryo exemplified all the dictates of the search for construct validity (Eysenck, 1992d). There is thus nothing terribly novel about this approach to *validity*; had there been more attention paid to it since the days of Heymans, the psychology of personality would have advanced at a much faster rate.

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