

## FOUR WAYS FIVE FACTORS ARE *NOT* BASIC

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**Summary**—This is a reply to the Costa and McCrae article entitled: “Four ways five factors are basic” [(1992) *Personality and Individual Differences*, 13(6), 653–665]. This article takes up the challenge and discusses four major criticisms of the 5-factor model. The first criticism relates to the level of the hierarchical model of personality at which different factors arise, suggesting that 3 of the 5 factors in the Costa and McCrae model are essentially primaries, often highly intercorrelated, and linked closely with psychoticism. The second criticism is directed at the failure of Costa and McCrae to discuss the overwhelming evidence from meta-analyses of factorial studies that 3, and not 5 factors emerged at the highest level. The third criticism is directed at the lack of a nomological network or theoretical underpinning for the 5 factors, and the fourth is directed at the failure of providing a biological link between genetic causation and behavioural organization. All four criticisms suggested that the postulation of the 5-factor model is a premature crystallization of spurious orthodoxy.

The search for the fundamental dimensions of personality has gone on for a long time since its beginning in the empirical studies of Heymans and Wiersma in the first decade of this century (discussed in detail by Eysenck, 1970), or the 2000-yr-old typology of the ancient Greeks and Romans. The advent of factor analysis, so it was hoped, would supply a method for arriving at an agreed paradigm; this hope has not materialized (Eysenck, 1991). The same traits appear and reappear in the writings of different authors, combine or separate randomly to mark different levels of the hierarchical model implicitly or explicitly adopted by most psychologists working in this field (Eysenck, 1947). Claims to have discovered some paradigmatic solution clearly requires criteria to judge such claims, and it was my intention to suggest such criteria which prompted a recent publication (Eysenck, 1991), where I suggested 14 such criteria, and discussed their application to the 3 major models seriously in competition at the moment.

Costa and McCrae (1992) claim that their 5-factor model “represents basic dimensions of personality”, and suggest 4 criteria. They claim (a) that longitudinal and cross-observer studies demonstrate that all 5 factors are enduring dispositions that are manifest in patterns of behaviour; (b) that traits related to each of the factors are found in a variety of personality systems and in the natural language of trait description; (c) that the factors are found in different age, sex, race, and language groups; and (d) that evidence of heritability suggests that all have some biological basis. What I shall suggest in this brief rejoinder is that all four claims are correct, but say nothing about the acceptability of the system. I will then go on to consider four arguments that contradict the claims made by Costa and McCrae.

Let us consider the first criticism. It is agreed that the 5 factors in question are enduring dispositions that are manifest in patterns of behaviour, but the same is true of Cattell’s and Eysenck’s rather different models, and of many others as well (Eysenck & Eysenck, 1985). Thus this claim is irrelevant to a discussion of the merits of different models; it is a necessary but not a sufficient criterion. The same is true of the second criterion; indeed, it carries the seeds of its own destruction. If the traits in question are found “in a variety of personality systems”, why should we prefer the one presented by Costa and McCrae?

The same arguments apply to claims (c) and (d). The 5 factors are found in different age, sex, race and language groups, but so are the Cattell superfactors and the Eysenck factors. Evidence of heritability is strong for all personality factors studied; it does not single out the Costa and McCrae factors (Eaves, Eysenck & Martin, 1989). In other words, all the criteria suggested by Costa and McCrae are necessary but not sufficient to mark out one model from the many which also conform to these criteria. It is for this reason that I suggested additional criteria (Eysenck, 1991) which unfortunately Costa and McCrae hardly mention. What Costa and McCrae cite as

evidence for the unique status of the 5-factor model does not single it out from other contestants, using their own criteria. We must now look at inherent weaknesses of their model which their presentation obfuscates. Corresponding to their "four ways five factors are basic", I shall list four ways five factors are *not* basic.

Let us begin in an examination of the factor structure of personality. It is assumed that we are all agreed that such a structure must be hierarchical, starting with simple (primary) traits at the bottom, and through correlations between them working up to more complex structures at the intermediate level to the major dimensions at the top. The distinction between levels is vital, but difficult to make in practice. Where does sensation seeking lie, or impulsivity, or social shyness, or suggestibility? Sensation seeking breaks down into 4 subfactors only intercorrelating around 0.3; so did the original impulsivity scales. If we regard correlations around 0.3 as justifying us in grouping together traits into supertraits, what shall we make of the correlations of  $-0.49$  between neuroticism and conscientiousness, or that of  $0.43$  between extraversion and openness, reported by Costa, McCrae and Dye (1991)? Should we not regard lack of conscientiousness as a primary factor forming part of neuroticism, and openness as a primary factor forming part of extraversion? Costa and McCrae (1992) do not address this problem, which seems absolutely fundamental; are openness and conscientiousness unique dimensions of personality, or are they just primaries forming part of "true" major dimensions? What is our criterion for making such a decision? Without an answer to such questions, how can we take seriously the Costa and McCrae claims?

Of particular relevance is the relationship between psychoticism in the Eysenck system, and agreeableness and conscientiousness in the "Big 5" system. McCrae and Costa (1985) reported correlations of  $-0.45$  and  $-0.31$ , respectively, and Goldberg (personal communication) has reported a disattenuated correlation of  $-0.85$  between P and agreeableness and conscientiousness combined! This suggests very strongly that agreeableness (and to a lesser extent conscientiousness) are primaries which form part of psychoticism. Figure 1(a) shows the suggested relationship. Goldberg (personal communication) suggested a rather different model, illustrated in Fig. 1(b). Here P is pictured as a composite of two fundamental factors, agreeableness and conscientiousness. Neither Costa and McCrae, nor Goldberg, give any hint as to why we should prefer (b) to (a), or on what grounds we should judge between them. (Agreeableness also correlates very significantly with neuroticism ( $r = -0.25$ ); it is not unexpected to find that mentally disturbed people are disagreeable!)

Our first reason, then, for *not* regarding the A, C and O factors in the 5-factor model as basic is that no reasons are given for preferring the model on psychometric grounds; choice seems to be made on quite arbitrary grounds, and disregards high correlations *between* factors, or *with* factors arguably of a higher order. Thus far the argument has remained on psychometric grounds, and it was my main point that psychometric arguments are *in principle* incapable of leading to final decisions between models. Costa and McCrae do not even discuss this primary objection.

Our second reason for *not* regarding the A, C and O factors as basic harks back to my seventh criterion originally suggested as basic (Eysenck, 1991); "meta-analysis of results must show convergence" (p. 760). Yet outside the narrow circle of 5-factor enthusiasts, research has completely failed to find basic factors similar to A, C or O. Royce and Powell (1983) carried out a meta-analysis of factorial studies to date, emerging with 3 major dimensions which showed great similarity to P, E and N. Costa and McCrae fail to mention the fact that their scheme finds no support in the mass of empirical work surveyed by Royce and Powell. Such a failure to consider the great majority of past research is inadmissible. The most recent systematic model incorporating many divergent scales is the Tellegen and Waller (1991) one; they too emerge with 3 main factors clearly identified by P, E and N as markers.

So is the failure to consider the work done on the Cattell model, surveyed in my original presentation (Eysenck, 1991). Cattell, like the 5-factor model proponents, started with a lexical approach; the major results of properly done factor analysis of his scales and items has been to isolate three major factors, of which two (E and N) are clearly in common with Eysenck and the 5-factor model, and hence hardly in dispute; the third resembles P, but somewhat amputated because Cattell's item pool did not include many relevant items (Eysenck, 1991). Costa and McCrae fail to comment on this discrepancy between the outcomes of two major approaches both based on lexical principles.

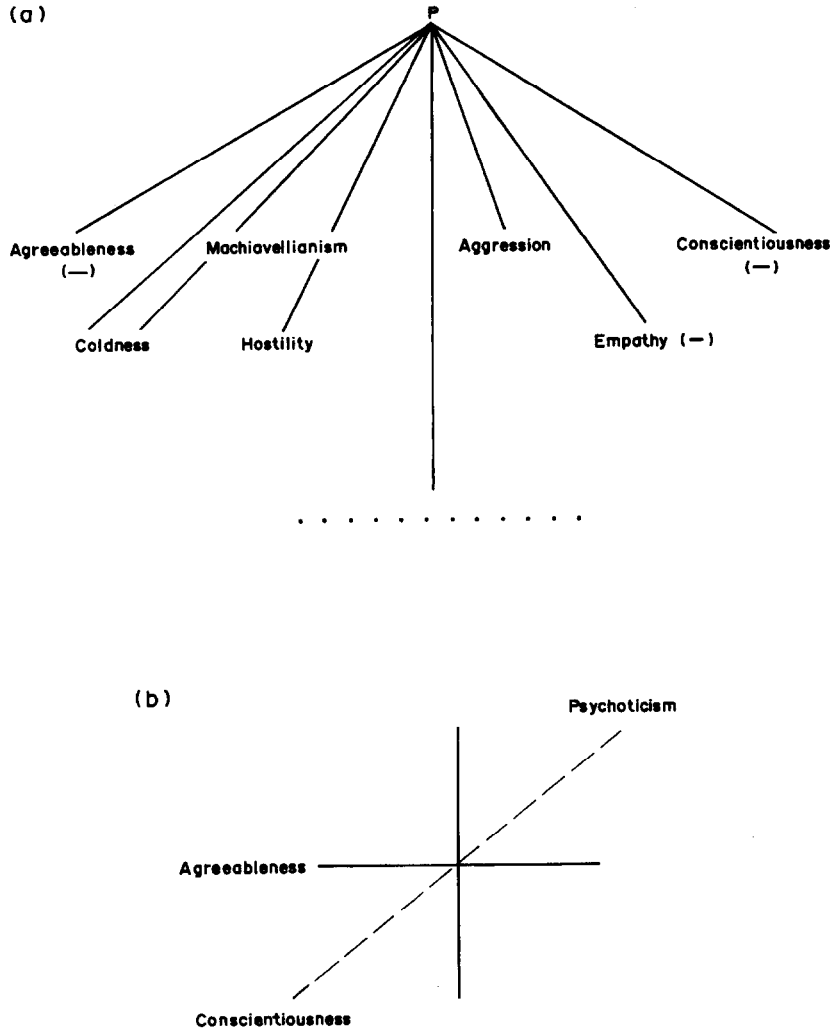


Fig. 1. Alternative ways of conceptualizing the observed correlations between P (psychoticism), A (agreeableness) and C (conscientiousness). Figure 1(a) considers A and C as primary factors, correlating with others to give rise to higher order factor P. Figure 1(b) considers P as a combination of fundamental factors A and C.

Altogether, Eysenck and Eysenck (1985) have surveyed many studies of different models, questionnaires and inventories, reporting in most cases a break-down into 2 or 3 major factors; never 5, and certainly none identical with A, C or O. Since then, the work of Zuckerman, Kuhlman and Camac (1988) and Zuckerman, Kuhlman, Thornquist and Kiers (1991) has been particularly interesting and relevant, because they used many different questionnaires for their correlations, and because they specifically addressed the issue of 3 or 5 factors. Our first consideration is the scree test, which may be used to assess the number of factors. Zuckerman *et al.* (1991) state that “the first abrupt shift in eigenvalues occurs at the fifth position” (p. 931), but it is difficult to agree. Eigenvalues decrease from 6.3 to 5.9; then abruptly to 3.0, 2.3, 1.8, 1.3, 1.1, 1.1, 0.8. The most obvious break is after the second eigenvalue; the rest form a perfectly smooth curve. Unless we agree on 2 factors, the test is inconclusive.

Of the solutions considered, the 3- or 5-factor models were found equally robust between sexes and in 4 different samples; this again does not help us to decide. The 3-factor model is well in line with the Eysenck model—sociability, N-emotion, and P-ImpUSS (Impulsive-Unsocialized Sensation Seeking). The 5-factor model does not bear much relation to the Costa and McCrae model: sociability and N-anxiety of course mirror E and N. Activity, Agg-Host (aggression and hostility) and P-ImpUSS hardly match A, C and O. Actually Zuckerman *et al.* (1991) did not include markers for O, so that Costa and McCrae would only have expected 4 factors to agree with theirs. Again

sociability and N-anxiety fit with E and N, but do Agg-Host and P-ImpUSS fit with agreeableness and conscientiousness? The fit, if any, is certainly much less well marked than that of the 3-factor model.

Our second criterion thus fails to support Costa and McCrae. Analyses done outside the small circle of 5-factor model factorists give evidence in favour of a 3-factor model, and hardly any support for a 5-factor model. Even if we could identify P-ImpUSS as the obverse of agreeableness, we would still have a P factor and its inverse. To identify Agg-Host with the inverse of conscientiousness would seem too forced a match to carry conviction, although conscientiousness does correlate with N  $-0.49$ , and Agg-Host merge with N-anxiety to form N-emotion in the Zuckerman *et al.* (1991, p. 948). But this again suggests that conscientiousness is at a lower factorial level than N, and forms part of it.

I do not wish to suggest that my own interpretations of the existing evidence are conclusive in any sense, but a reasonable case can be made to suggest that consideration of all the existing factorial studies of personality would lead to the postulation of a 3-factor model, not a 5-factor one, but that in the nature of the case no final conclusion is possible because of the limitations of the psychometric criteria. Using *only* such criteria will inevitably leave us forever searching for a path through the maze of correlational and factorial hedges which we have no way of finding, or knowing if and when we have found it.

The third major point concerns the need for a nomological or theoretical network to accompany and be part of any model. Only thus can we avoid the problems of subjectivity and the possibilities of misinterpretations attaching to the conceptualization and naming of factors. How do we conceptualize the fact of correlations between self-rated competence, order, dutifulness, achievement-striving, self-discipline and deliberation? Costa and McCrae name the factor "conscientiousness"; Cattell might have called it "super-ego", sceptics might consider it a "halo" factor, or a "lie" factor. The high correlation with neuroticism ( $r = -0.49$ ) suggests simply an absence of N. Cross-observer correlations are derisively small—peer/peer correlation is 0.30, i.e. there is less than 10% agreement between observers! (Costa *et al.*, 1991). This suggests a halo or Lie Scale interpretation. (It correlates 0.34 with the L Scale in Table 4 of the Costa and McCrae article. Agreeableness has an even larger Lie correlation  $-0.53$ !) I am not arguing for any of these divergent interpretations; I am suggesting that in the absence of a nomological network, and lacking any theoretical underpinning, the Costa and McCrae interpretation is subjective, contestable, and hence unscientific. It fails to consider alternative interpretations, other than which there can be no more serious objection to a given interpretation. The interpretation of the C factor as a negative aspect of N seems much more inviting (together with certain negative aspects of P). There is a well-supported theory of N, based on genetic, learning theory and conditioning principles (Eysenck & Martin, 1987). It is not only possible but easy to deduce the C behaviour patterns from the theory, thus giving "artistic versimilitude to an otherwise bald and unconvincing narrative", to quote Gilbert and Sullivan's 'Mikado'. The aberrant emotional responses of the neurotic lower his competence, make it difficult for him to keep things in order, lower his compliance with duties, and his self-discipline, interfere with his achievement-striving, and make him impulsive and less deliberate. Why have *two* major dimensions, highly intercorrelated and with the theory underlying one explaining the content of the other? Why did Costa *et al.* (1991) fail to factor analyse their Table 5, to show what really is the dimensionality of their matrix? Clearly, even on their own showing there are not 5 independent factors in their data!

As an example of the way a nomological network can be constructed, related to wide-ranging theories, and submitted to experimental testing, consider psychoticism as a major dimension of personality. There are two presuppositions underlying the theory. The first is that all functional psychoses (schizophrenia, manic-depressive illness, schizoaffective disorders, monopolar depression) are *related* and do not form independent categories; evidence on this point bears out the assumption (Crow, 1986, 1990). The second presupposition is that psychoses as such are continuous with a whole spectrum of abnormal states (schizoid disorders, psychopathy, alcoholism, criminality) which occur significantly more frequently in relatives of psychotics, and shade into perfectly normal forms of behaviour (Eysenck & Eysenck, 1976). This notion of a continuum can also be tested more directly by means of *criterion analysis* (Eysenck, 1950, 1952), which has given strong support to the notion.

Based on these well-supported sets of assumptions, we created a test to measure the hypothesized continuum of psychoticism (Eysenck & Eysenck, 1976), and produced a scale which formed the third of our major dimensions of personality. How can one test the hypothesis that the P Scale is actually a measure of psychoticism, rather than of psychopathy, or disagreeableness, or ImpUSS, as Zuckermann *et al.* (1991) would have it?

The suggested answer relies on the fact that we have a criterion group, namely psychotics at our disposal. The theory demands that if P actually measures psychoticism, then *markers which clearly divide psychotics from normals should also divide high P and low P scorers in the normal (and perhaps also the psychotic) population*. This proportionality criterion should work *if and only if* the 3 major hypotheses outlined above are correct, and would thus provide an *objective* criterion for the identification of the factor.

The evidence strongly favours such a view. Studies have been done successfully using hormonal-biochemical substances such as HLA B-27 (Gattaz, 1981; Gattaz, Seitz & Beckman, 1985), low platelet monoamine oxydase (Klinterberg, Schalling, Edman, Orelund & Aesberg, 1987), and Serotonin (Schalling, Edman & Aesberg, 1983); using Pavlovian concepts trying to explain psychotic behaviour such as negative priming (Beech & Claridge, 1987), latent inhibition (Baruch, Hemsley & Gray, 1988; Lubow, Inberg-Sachs, Zalstein & Gewirtz, 1992); physiological measures, such as inverted autonomic and perceptual functioning (Claridge & Chappa, 1973), electromyography (Hinton & Craske, 1976), and lateralized cerebral dysfunction (Jutai, 1988); and psychological experiments, such as eye-tracking (Lipton, Levy, Holtzman & Levin, 1983; Bosch, 1984; Iacono & Lykken, 1979; Simon & Katkin, 1985; Siever, Haier, Coursey, Sostek, Murphy, Holzman & Buchsbaum, 1982), dichotic shadowing (Rawlings & Borge, 1987); backward masking deficit (Badcock, Smith & Rawlings, 1988), word association singularity (Upmanyu & Kaur, 1986; Ward, McConaghy & Catts, 1991), and many more, including hallucinatory experience (Slade, 1976; Launay & Slade, 1981) and other mental symptoms. These studies serve to *test* and *verify* the hypothesis that P is an actual measure of the hypothesized dimension of personality identified as psychoticism, including among the primary factors on which it is based impulsivity, sensation-seeking, Machiavellianism, and lack of agreeableness, with perhaps a dash of lack of conscientiousness.

Let us now turn to our fourth point. Costa and McCrae state that “some, like Claridge (1986), have argued that no personality dimension can be taken seriously unless it is supported by theory linking it to biological mechanisms”. They go on to say: “We believe that this latter view is profoundly mistaken. The fact is that we know much more about personality structure than we do about the functioning of the brain, and it is poor science to try to explain the known on the basis of the unknown. Consider for a moment the far-reaching developments in our understanding of neurophysiology that have occurred since 1961. In retrospect, it would have been folly for Tupes and Christal to attempt to explain their 5-factors in terms of the comparatively primitive neuroscience of the day. Their factors have survived the past 30 yr very well; any biological explanation they might have proposed would surely be hopelessly outdated. Will today’s neurobiological explanations far any better?”

On this point it is necessary to disagree. If we were to wait until neuroscience was perfect before making use of its findings, we would not even try to fit together the biological and psychological bits and pieces of our jigsaw. Of course our early hypotheses are unlikely to be right in some metaphysical sense, but they do lead to experiments, the results of which may then be used to improve the theories involved, and in turn help clarify neurophysiological principles. Work on the biological basis of personality has come a long way, and to dismiss it in this categorical fashion is to fly in the face of recent advances (Eysenck, 1990b; Zuckerman, 1991). This approach is what Lakatos (1970) has called a “progressive programme shift”, meaning that while subject to criticism, improvement and even large-scale change of emphasis, the research programme is advancing in the right direction, discovering new facts, and integrating these with its theories. It is the opposite of a degenerating research programme, such as the Freudian, which merely attempts to stifle criticism, explain away failures, and fails to produce any new findings.

A good example here is Dalton (Greenaway, 1966) who by elaborating his theory of atomism became the father of modern chemistry. All that Dalton said about atoms—apart from the bare fact of their existence, which was not novel—was wrong. They are not indivisible nor of unique

weight; they need not obey the laws of definite or multiple proportion, and anyway his values for relative atomic weights and molecular constitutions were for the most part incorrect. Yet he set chemistry on the right path, and "ordinary science" soon corrected his theory where it was faulty. "There is nothing as practical as a good theory", Lewin is often quoted as saying, and such a theory implicating psychophysiological, hormonal, and electro-encephalographic measures forms an indispensable part of the nomological networks surrounding any meaningful modern theory of personality. "Measurement without theory is blind", as Kant said, just as "theory without measurement is lame"; we need to anchor our dimensions of personality in something more concrete than the morass of factor analysis, and biology supplies us with the necessary tools (Eysenck, 1990b). To throw these away because they are not yet perfect seems unreasonable, although advocates of purely psychometric concepts like A, C and O may not cherish the challenge of providing the required theories and tests.

It is not suggested that A, C and O are not likely to be correlated with biological measures of various kinds. A high level of platelet MAO is almost certain to correlate with A, for instance. If A is indeed the obverse of P, then what is true of P must largely be true of A, but in the opposite direction. The crucial point is that all the theoretical deductions flow from P, E and N, none from C, A and O. This alone would suggest that these aspects of the 5-factor theory are not basic factors of personality.

This reply to Costa and McCrae has not been written in a spirit of finality, but rather to keep open a debate on the fundamental issue of just what criteria to use in judging a theory of personality. Is it really sufficient to have some psychometric support, or is much more required to firmly anchor our dimensions? What demands should be made on the objectivity of our factor identifications, and how can we avoid subjectivity? What is the role of biological intermediaries in clarifying the theoretical and practical issues involved? It is sad that there has been very little discussion of such fundamental questions, and that the idle practice of producing new personality scales and tests continues unabated, making it less and less likely that we will ever arrive in the promised land of the paradigm which alone would endow our efforts with scientific respectability.

## REFERENCES

- Badcock, J. C., Smith, G. A. & Rawlings, D. (1988). Temporal processing and psychic proneness. *Personality and Individual Differences*, 9, 709-719.
- Baruch, I., Hemsley, D. R. & Gray, J. (1988). Latent inhibition and "psychotic proneness" in normal subjects. *Personality and Individual Differences*, 9, 777-784.
- Beech, A. & Claridge, G. (1987). Individual differences in negative priming. *British Journal of Psychology*, 78, 349-356.
- Bosch, R. van den (1984). Eye-tracking impairment: Attentional and psychometric correlates in psychiatric patients. *Journal of Psychiatric Research*, 18, 277-286.
- Claridge, G. (1986). Eysenck's contribution to the psychology of personality. In: Modgil, S. & Modgil, C. (Eds), *Hans Eysenck: Consensus and Controversy* (pp. 73-85). London: Falmer Press.
- Claridge, G. S. & Chappa, H. J. (1973). Psychoticism: A study of its biological basis in normal subjects. *British Journal of Social and Clinical Psychology*, 12, 175-187.
- Costa, P. T. & McCrae, R. R. (1992). Four ways five factors are basic. *Personality and Individual Differences*, 13(6), 653-665.
- Costa, P. T., McCrae, R. R. & Dye, D. A. (1991). Facet scales for agreeableness and conscientiousness: a revision of the NEO personality inventory. *Personality and Individual Differences*, 12, 887-898.
- Crow, T. J. (1986). The continuum of psychosis and its implication for the structure of the gene. *British Journal of Psychiatry*, 149, 419-429.
- Crow, T. J. (1990). The continuum of psychosis and its genetic origins. *British Journal of Psychiatry*, 156, 788-797.
- Eaves, L., Eysenck, H. J. & Martin, N. (1989). *Genes, Culture and Personality: An Empirical Approach*. New York: Academic Press.
- Eysenck, H. J. (1947) *Dimensions of Personality*. London: Routledge & Kegan Paul.
- Eysenck, H. J. (1950). Criterion analysis: an application of the hypothetico-deductive method to factor analysis. *Psychological Review*, 57, 38-53.
- Eysenck, H. J. (1952). Schizothymia-cyclothymia as a dimension of personality: II. Experimental. *Journal of Personality*, 20, 345-384.
- Eysenck, H. J. (1970). *The Structure of Human Personality*, 3rd Ed. London: Methuen.
- Eysenck, H. J. (1990a). Genetic and environmental contributions to individual differences: The third major dimension of personality. *Journal of Personality*, 98, 245-261.
- Eysenck, H. J. (1990b). Biological dimensions of personality. In: Pervin, L. A. (Ed.), *Handbook of Personality: Theory and Research*. New York: Guilford Press.
- Eysenck, H. J. (1991). Dimensions of personality: 16-, 5- or 3-Criteria for a taxonomic paradigm. *Personality and Individual Differences*, 12, 773-790.
- Eysenck, H. J. & Eysenck, M. W. (1985). *Personality and Individual Differences: a natural science approach*. New York: Plenum Press.

- Eysenck, H. J. & Eysenck, S. B. G. (1976). *Psychoticism as a Dimension of Personality*. London: Hodder & Stoughton.
- Eysenck, H. J. & Martin, I. (Eds.) (1987). *Theoretical Foundations of Behaviour Therapy*. New York: Plenum Press.
- Gattaz, W. F. (1981). HLA-B27 as a possible genetic marker of psychoticism. *Personality and Individual Differences*, 2, 57–60.
- Gattaz, W. F., Seitz, M. & Beckman, H. (1985). A possible association between HLA-B27 and vulnerability to schizophrenia. *Personality and Individual Differences*, 6, 283–285.
- Greenaway, F. (1966). *John Dalton and the Atom*. London: Heinemann.
- Hinton, J. & Craske, B. (1976). A relationship between Eysenck's P scale and change in muscle action potentials with attention to perceptual tasks. *British Journal of Psychology*, 67, 461–466.
- Iacono, W. G. & Lykken, D. T. (1979). Eye-tracking and psychopathology: new procedures applied to a sample of normal monozygotic twins. *Archives of General Psychiatry*, 36, 1361–1369.
- Jutai, J. W. (1988). Spatial attention in hypothetically psychosis-prone college students. *Psychiatry Research*, 27, 207–215.
- Klinterberg, B., Schalling, D., Edman, G., Orelund, L. & Aesberg, M. (1987). Personality correlates of platelet monoamine oxidase (MAO) activity in female and male subjects. *Neuropsychobiology*, 18, 89–96.
- Lakatos, I. (1970). Falsification and the methodology of scientific research programmes. In: Lakatos, I. and Musgrave, A. (Eds), *Criticism and the Growth of Knowledge*. Cambridge: University Press.
- Launay, G. & Slade, P. (1981). The measurement of hallucinatory predisposition in male and female prisoners. *Personality and Individual Differences*, 2, 221–234.
- Lipton, R. B., Levy, D. L., Holtzman, P. S. & Levin, S. (1983). Eye movement dysfunction in psychiatric patients: a review. *Schizophrenia Bulletin*, 9, 13–32.
- Lubow, R. E., Ingberg-Sachs, V., Zalstein, N. & Gewirtz, J. (1992). Latent inhibition in low and high "psychotic-prone" normal subjects. *Personality and Individual Differences*, 13(5), 563–572.
- McCrae, R. R. & Costa, P. T. (1985). Comparison of EPI and Psychoticism Scales with measures of the five-factor model of personality. *Personality and Individual Differences*, 6, 587–597.
- Rawlings, D. & Borge, A. (1987). Personality and hemispheric function: two experiments using the dichotic shadowing technique. *Personality and Individual Differences*, 8, 483–488.
- Royce, J. R. & Powell, A. (1983). *Theory of Personality and Individual Differences: Factors, Systems and Processes*. Englewood Cliffs: Prentice-Hall.
- Schalling, D., Edman, G. & Aesberg, M. (1983). Impulsive cognitive style and inability to tolerate boredom: psychobiological studies of temperamental vulnerability. In: Zuckerman, M. (Ed.), *Biological Bases of Sensation-Seeking, and Anxiety*, (pp. 123–145). Hillsdale: Erlbaum.
- Siever, L. J., Haier, R. J., Coursey, R. D., Sostek, A. J., Murphy, D. L., Holzman, P. S. & Buchsbaum, M. S. (1982). Smooth pursuit tracking impairment: Relation to other "markers" of schizophrenia and psychologic correlates. *Archives of General Psychiatry*, 39, 1001–1005.
- Simon, R. F. & Katkin, W. (1985). Smooth pursuit eye movements in subjects reporting physical anhedonia and perceptual aberrations. *Psychiatry Research*, 14, 275–289.
- Slade, P. D. (1976). An investigation of psychological factors involved in the predisposition to auditory hallucinations. *Psychological Medicine*, 6, 123–132.
- Tellegen, A. & Waller, N. G. (1991). Exploring personality through test constructions: Development of the Multidimensional Personality Questionnaire. In: Briggs, S. R. and Checks, J. M. (Eds), *Personality Measures: Development and Evaluation* (Vol. 1). Greenwich, CN: JAI Press.
- Upmanyu, V. V. & Kaur, K. (1986). Diagnostic utility of word association emotional indicators. *Psychological Studies*, 32, 71–78.
- Ward, P. B., McConaghy, N. & Catts, S. V. (1991). Word association and measures of psychotic-proneness in university students. *Personality and Individual Differences*, 12, 473–480.
- Zuckerman, M. (1991). *Psychobiology of Personality*. Cambridge: Cambridge University Press.
- Zuckerman, M., Kuhlman, D. M. & Camac, C. (1988). What lies beyond E and N? Factor analyses of scales believed to measure basic dimensions of personality. *Journal of Personality and Social Psychology*, 54, 96–107.
- Zuckerman, M., Kuhlman, D. M., Thornquist, M. & Kiers, H. (1991). Five (or three) robust questionnaire scale factors of personality without culture. *Personality and Individual Differences*, 12, 929–941.