

A REPLY TO COSTA AND McCRAE. P OR A AND C—THE ROLE OF THEORY

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Summary—The differences between advocates of the Big Five and the PEN (psychoticism, extraversion, neuroticism) system are most clearly apparent in the problem of whether agreeableness and conscientiousness are to be classed as major dimensions of personality, or as primary factors forming part of P. It is argued that advocates of the Big Five neglect a large volume of research which is crucial to this debate, and which tilts the balance in favour of the PEN system.

Costa and McCrae in their reply (this issue, pp. 861–865), do not deal with what I conceive to be the major point at issue, although they do reject “the idea that personality structure should be determined by theory”. But this is not really my position; early taxonomic studies lead to theoretical formulations which should guide better taxonomic studies which in turn should lead to theoretical advances—very much as happens in the more advanced sciences. In this effort factor analysis is a good servant, but a bad master; Revenstorff (1978) has spelled out many of the justified criticisms which it incurs. Its findings are precarious unless and until they can be tested against deductions from theory, either in the laboratory or in real-life situations.

Let us apply this argument to what I believe is the major difference between the Big Five and the PEN system. In the Big Five, A (agreeableness) and C (conscientiousness) are two of the five dimensions recognized as being at the same level as E (extraversion) and N (neuroticism). Now there is a huge network of experimental and empirical (life situation) findings to support the conceptualization of E and N (Eysenck & Eysenck, 1985); there is nothing comparable for A and C. Such a disproportion should give us time to think; would strong claims for A and C not be better delayed until a similar nomological network of theories and factual support was available to support such claims? The same would be true for openness (O) where there is also a desperate lack of independent support.

I have suggested that A and C are most likely to be primary factors, rather than being at the highest level of the factor hierarchy. The field they cover is clearly much narrower, as befits a primary, than that covered by P, E or N. Little in the way of theory is available to cover their nakedness. And most importantly of all, they correlate highly with P, which has for long been one of the three major dimensions of personality. Goldberg and Rosolack (1991) report a disattenuated multiple correlation of 0.85 between P and A/C; there can be no doubt about a very strong relationship between these factors.

Psychometrically we can look upon this correlation from two points of view. We can say with Goldberg that P is simply an artifactual combination of two major factors, A and C, or we can say, as I would prefer, that A and C are primaries which, together with many others, go to make up superfactor P. As far as psychometric evidence goes, there is no way of deciding between the two interpretations; choice is purely subjective. We have to step outside the self-contained circle of correlation studies to take into account the large body of evidence relevant to this problem.

Let us note, first of all, that a great deal is known about psychosis which is relevant to any theory of psychoticism. It is known, for instance, that different functional psychoses are not separated by strict Kraepelinian boundaries, but that they form a definite continuum (e.g. Crow, 1986). This continuum extends beyond the functional psychoses to “spectrum disorders”, borderline traits, schizoidy and other related functions (Claridge, 1988). Finally, these pathological states merge into normal types of behaviour (Claridge, 1985). Thus there is a very large, very rich literature pointing to the existence of a dispositional variable essentially similar to the concept of “psychoticism”. No

such evidence is available for concepts like agreeableness or conscientiousness to suggest that they are anything other than primaries covering a small area in the rich field of personality.

We must next turn to the vexed question of how to *prove* scientifically that our interpretation of a given factor is correct. Clearly just to look at the items will not do; this amounts to simple subjective interpretations, relying on common sense, but not providing testable deductions from a firmly established theory. Can P do any better?

I have argued that such a proof is available in what I have called the "proportionality criterion". Let us take schizophrenics and normals as extremes on the P continuum. Let us now take a meaningful test T (hormonal, psychophysiological, behavioural, experimental) which clearly differentiates the two groups. Now if P measures the same continuum, then T will separate in the predicted direction high-P normals from low-P normals, and high-P schizophrenics from low-P schizophrenics. As an example, consider the human leucocyte antigen HLA-B27, which fulfills the first requirement of discriminating between schizophrenics and normals (Gattaz, Ewald & Beckman, 1980). It also fulfills the second condition, discriminating between high- and low-P normals (Gattaz, 1981), and the third, discriminating between high- and low-P schizophrenics (Gattaz, Seitz & Beckman, 1985). This is the kind of proof I would suggest as my support for the interpretation of P as "psychoticism".

I have elsewhere (Eysenck, 1992) listed over a dozen similar experiments, extending to psychophysiological studies, psychological laboratory experiments, studies using neurotransmitters, psychopharmacological studies, conditioning (latent inhibition) experiments, and many more. Can it be seriously maintained that failure to show latent inhibition can be explained as being due to, or productive of, a lack of agreeableness and conscientiousness? There are strong theories linking it with schizophrenia and psychoticism (Lubow, 1989), none to link it with A or C.

It is on arguments of this kind, not even considered by Costa and McCrae, that I would rest my case. I have always agreed with Cronbach (1957) that the two disciplines of scientific psychology (experimental and correlational) must be united to achieve a truly scientific product. Factor analysis by itself is at best suggestive, but it cannot decide between theoretical interpretations differing like those of Costa and McCrae, and myself. We need more inclusive theories, and testable deductions from these theories, to answer this type of question, at least provisionally. To reject the use of theory is to reject the scientific method altogether, and to render insoluble such disagreements as that under discussion.

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