COMMENT

ON

THE RELATION OF NEUROTICISM AND EXTRAVERSION TO INTELLIGENCE AND EDUCATIONAL ATTAINMENT

A brief comment on the controversy between J. B. Biggs and R. Lynn.

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In the controversy between Lynn and Biggs, the crucial part appears to be played by the exact status of ' anxiety.' Biggs assumes that ' anxiety ' and ' neuroticism ' are used interchangeably by me, but this is incorrect. Neuroticism is regarded as an inherited psycho-physical disposition, closely linked with the lability of the autonomic system, which governs a person's emotional reactivity, and may predispose him to the development of neurotic disorders under suitable circumstances. Anxiety is a conditioned fear reaction which is particularly characteristic of dysthymic neurotics, i.e., of persons who are high on the factor of neuroticism and also on the factor of introversion, which is significantly correlated with conditionability. Anxiety, therefore, is a ' mixed ' concept, being related both to neuroticism and to introversion. (The position is rendered rather confused by Cattell's use of the terms ' neuroticism ' and ' anxiety,' which is exactly the opposite of mine.)

My theory, which links introversion and conditionability, is not necessarily antagonistic to that of Spence, which links neuroticism, regarded as a drive, to conditionability. On the theoretical level either theory, or both, or neither, might be right. My belief that Spence's theory is inadequate is based on the experimental evidence, and may very well be mistaken ; the experimentally demonstrated correlation between the Manifest Anxiety Scale and tests of introversion make all the Spence-Taylor experiments using the M.A.S. equivocal. In our own work we have never succeeded in finding correlations between neuroticism and conditionability, and neurotics as a group are more conditionable than normals *only* when they are selected in such a way that introverts are over-represented.

I agree with Biggs that a great deal more work is needed at the theoretical level, but I feel even more strongly that what is needed is a greater concentration on *experimental* studies, with particular reference to parametric investigations. The frequently contradictory results obtained by investigators testing both Spence's hypothesis and my own suggest that neither hypothesis is universally valid, but requires to be closely circumscribed with respect to its validity by detailed specification by such parameters as strength of CS and UCS, CS-UCS interval, proportion of reinforced trials and so on. Lynn's study appears to me to be a logical deduction from principles which appear to have at least some experimental support, although this support may be rather circumscribed in the manner just outlined, and although it may, of course, be possible to account for his results in other terms than those premised in his theory. This hardly detracts from the value of his important demonstration.