by the treatment of the Born–Oppenheimer approximation, which is implicit in the Hückel method but need not be made explicit in an elementary treatment. Coulson accordingly does not mention it at all. Yates mentions it twice — once to say that it muust be used, and once to say that it has been used. He does not say what it is. Another example is the treatment of the Coulson–Rushbrooke pairing theorem, which is fundamental to many of the applications of Hückel theory, and important in understanding the unexpectedly wide validity of the theory. Coulson naturally proves the theorem, carefully and precisely, so that there is no doubt about the exact assumptions which are required. Yates has no reference by name to the theorem, either in the index or, as far as I can discover, in the text; and although he could have stated the essence of the theorem, as Coulson does, in a dozen lines, he spreads the statement over several pages, calls it a series of generaliations rather than a theorem, and moreover, by introducing a further weaker generalisation, for which he gives an invalidating counter-example, leaves the reader with the impression that all of these 'generalisations' share the same lack of generality.

Yates claims, with some justice, that organic chemistry students frequently apply important ideas and approaches such as the Woodward–Hoffman rules "without a sufficiently sound understanding of their theoretical basis". Yet he fobs his readers off with phrases like "it can be shown that . . . " or "it turns out that . . . ", even where the result could be derived quite readily. Other arguments are presented in a thoroughly superficial manner. For example, the important class of sigmatropic reactions is dealt with on the basis that "the important molecular orbital of the π framework involved in the migration is considered by Woodward and Hoffman to be the [highest occupied one]". Is the student to take this *ex cathedra* statement as the basis of his sound understanding? There is no discussion of the reasons for the validity of this assumption; nor is there any cross-reference, or index reference, to a later chapter where this class of reaction is studied by another method.

Indeed, there is an irritating lack of cross-references throughout: the reader is continually being told that a certain topic will be treated "later", with no cross-reference to lead him to the right place. The index too, is thoroughly unhelpful: for example, of the four entries under "Woodward–Hoffman rules" one leads the reader to the statement that they will be treated "in the following chapters", and another to the statement that they will be described "later". None of them directs the reader to the full derivation of the rules or even to a statement of what they are.

This lack of consideration for the reader is illustrated in a different way by a sentence in the first chapter, which reads: "For the second molecular orbital . . . a similar treatment would give $E_2 = (\alpha-\beta) / (1-S)$, and since β is generally greater in absolute magnitude then α (and both are negative energy terms) this corresponds to a positive energy level or an antibonding molecular orbital". Now the author has slipped up in his statement (repeated in a diagram) that antibonding orbitals have positive energy. They merely need to have energy greater than α. If the energy had to be positive, and α and β were both negative, then it would certainly be necessary that $|\beta| \gg |\alpha|$. But this is not the case; for with the normal choice of energy scale (adopted implicitly if obscurely on the previous page) α is typically around −15 eV and β around −3 eV. Any attempt to check the relative values of $|\beta|$ and $|\alpha|$ would have revealed this fact, and, eventually, the error about antibonding energies; but the author seems to have been content to state what "must" be true. He has advertised his uncertainty by adding the word "generally", but evidently without contemplating the consequences of $|\beta|$ being less than $|\alpha|$. He also seems to be unaware that it is meaningless in any case to compare values of α and β without specifying the energy datum precisely, as they change in different ways when the datum is changed, and indeed either can be made to vanish by a suitable choice.

Let us then turn back to Coulson, O'Leary and Mallion. They will not take the reader through the important modern ideas in theoretical organic chemistry, but they will provide him with a secure foundation on which to base his study of the more specialised monographs in the field.

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Nature-nurture

Intelligence: Heredity and Environment. By P. E. Vernon. Pp. 390. (Freeman: San Francisco and Reading, 1979.) Hardback £10; paperback £5.30.

VERNON sets out to reconcile the conflicting claims regarding intelligence and its transmission and to adopt an eclectic position that gives due weight to all the relevant facts. To this end he presents a critical appraisal of a very large number of studies with admirable lucidity.

Although such eclecticism may sound impersonal, there certainly is no lack of personal involvement on Vernon's part, and it is this as much as anything which gives the book its fascination. In many ways it is an intensely personal statement, from the preface in which he speaks of this book as his last, to the finale in which he mounts an emotive attack on recent trends in childrearing and education and contemplates the breakdown of Western mores and standards.

As the author looks back on the half century during which he himself has made such a distinguished contribution to psychometrics, he clearly perceives that all is not well. To take a specific example, intelligence testing is being banned in some parts of the United States on grounds of cultural bias. Vernon devotes a chapter to effectively countering this accusation. But more generally, psychometrists seem to be an increasingly isolated group, disowned on the one hand by many geneticists and on the other by many developmental psychologists. From the beginning quantification has been the primary goal in psychometrics, and has arguably hindered rather than helped conceptualisation. Vernon argues that "psychometry is quite entitled to use its own brand of operationalism, regardless of philosophical theories of scientific method, provided it works". But does it work?

The bulk of the book is devoted, as the title suggests, to the nature-nurture issue. Vernon makes a plea for scientific objectivity, suggesting that questions of social and political reform are not the business of the psychologist; it is his business simply to supply scientific data. In the context of his argument it is particularly unfortunate that Vernon has been forced to acknowledge (in a prefatory notice) the evidence of systematic fraud having been perpetrated by Burt. Vernon's citations of Burt are exceeded only by those of Jensen, and even given the author's attempts to minimise his dependence on Burt's studies the affair hardly encourages us to accept the impartiality of scientific data.

The book gives detailed coverage of the heritability studies and the criticisms which have been raised against them. Although Vernon offers a stalwart defense of the attempt to estimate heritability, the sheer variety of methodological and statistical doubts raised by critics of the various studies may tend further to undermine the reader's confidence.

Vernon's equivocates on racial differences in intelligence, suggesting that it is highly probable that some genetic differences are involved, but that owing to the confounding of race with environmental differences it does not seem possible to separate their effects. To paraphrase the ancient Zen argument, it may be that the real problem lies not so much in our question being unanswerable, as in our remaining in the state of mind that led us to ask it.

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