the Jones-Mussen comparison of early and late maturers, he states that their conclusion holds up for some types and is reversed for others (which, being less numerous, were outweighed in the aggregate analysis).

This is important, but it also brings into sharp relief the weakness of Block's book. Block gives almost none of the quantitative summaries needed. Group D boys "may be regarded as 'early maturers'" but they became insecure men, not the confident men Jones and Mussen lead us to expect. Very well—but of the 11 group D boys *how many* were early maturers? And if fewer than 11, what was the correspondence within this group of biological maturing and adult personality?

Another fault: the writing is awkward to the point of agony. The sentences writhe about the content, strangling thought. There are innumerable clauses like this one: " . . . differential parental impactfulness eventuates in a personality product that is incomplete and imbalanced."

The types, the descriptions of which constitute the main report, are achieved by a tour de force. Other groupings with an equal degree of coherence could have been "clustered" around almost any set of four to six well-spaced individuals. Orthogonal varimax rotation of axes is a mathematical convenience; unless the original correlations have a remarkably strong structure, the factors that result deserve no special consideration as a basis for theory. Block's broad generalizations would probably hold up if he had formed the types differently, but his types are themselves little better than arbitrary.

A deeper issue arises from Block's requiring members of a type to have roughly similar patterns both in adolescence and adulthood. It would have been wiser to group on the basis of adolescent similarity, and then to report the frequency of various adult personalities for each group. Practically and theoretically, one wants to know what alternative futures are likely for a teenager of a given pattern. Block's scheme tempts us to recognize the next boy we see as belonging to, say, type E, and then to forecast that the boy will have a type E future. Block's methodology leaves him no way to report divergences among persons who are similar at age 13.

Block has been headstrong too in his refusal to provide trait scores for individuals by weighting relevant to Q-sort items. In his view, a 100-item Q description reports on 100 dimensions and much would be lost in trait scoring. But about five traits account for most of the variance in a judge's impression; Warren Norman, for example, has suggested extroversion, agreeableness, conscientiousness, emotional stability, and culture as an adequate list for men. Do Block's Q descriptions contain reliable variance in more than a few dimensions? Probably not; the statistical check that could easily have been made is not mentioned. If dimensionality is low, one could score each Q sort on a short list of traits, and then regress adult scores onto the profile of earlier scores, separately for early and late maturers, for example. Perhaps this would have made an arbitrary typology unnecessary.

The investigator who agrees to do his utmost with an archive laid down before he came on the scene is doomed to frustration. Midway in the work, questions came to the fore that were not anticipated and for which data are fragmentary or missing. Even if the original investigator is no longer on the scene, he has left behind him a set of visions and a loyal team of former associates; the massive, long-range study thus becomes an institution with which the analyst must compromise. In a rewardingly human postscript, Block is frank about the stresses he experienced as the intractability of the data and the human factors caused the analysis to drag on a dozen years beyond the time of the follow-up. He wonders aloud whether longitudinal studies are worth it all (as does Sontag of Fels in a recent retrospective article in *Child Development*).

Jones, Macfarlane, Block, and all the others who contributed to this research deserve our thanks. An investigator starting a study of adolescents today might ask additional questions, but he would find none of the original questions uninteresting and he would not be able to improve much on the quality of the data. Block's careful reduction of the files to Q descriptions is a contribution few others would or could have made. His broad conclusions are significant, and even though his typologies constitute a set of 11 stereotypes they still give the psychologist and the layman much food for thought. Perhaps the greatest yield of the study is yet to come, since Block tells us that others will be making more focussed, methodologically conventional studies of the Q descriptions, which now constitute an archive important in their own right.

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A Mode of Force

Affinity and Matter. Elements of Chemical Philosophy, 1800–1865. TREVOR H. LEVERE. Clarendon (Oxford University Press), New York, 1971. xviii, 230 pp. + plates. $14.50.

The discovery of the voltaic pile in 1800 attracted great interest among chemists of the day, and since an exciting property of current electricity was its ability to decompose compound substances it is not surprising that this discovery gave rise to many new ideas concerning the electrical nature of chemical affinity. Affinity can be considered a fundamental concept which brought unity to the often disparate areas of chemistry in the years between 1800 and 1865, after which other factors such as valence and structure became more important. Therefore it is hoped that a consideration of affinity theories will be of use to the historian in bringing order to this confusing period in the development of chemistry. Because of the author's belief that the concept of chemical affinity did not exist independently of those men who contributed to its development, he has written this book as a series of chiefly biographical essays and has not attempted to broadly trace the history of affinity theory. Consequently the book's importance must be limited to the detailed analysis of the affinity theories developed by Humphry Davy and Michael Faraday, and to a lesser extent those of J. J. Berzelius and others whose ideas formed the structure for speculation on the nature of chemical force and matter during the first half of the 19th century.

In an attempt to relate these ideas on chemical affinity to the general intellectual background of the 19th century, the author discusses at length the origin of the religious and philosophical views of Davy and Faraday which "exerted a demonstrable influence on the formation and development of assumptions about the nature and interrelation of matter and chemical force." In this he enters an area of recent controversy in the history of science, which has centered around criticism of the work of L. Pearce Williams on the influence of the ideas of Roger Joseph Boscovich and *Naturphilosophie* upon the experimental researches of Michael Faraday. Levere's treatment of Faraday follows closely that of Williams and betrays a somewhat uncritical acceptance of his position. Levere does, however, consider other factors, such as a

typically British thread of speculation on the unity of nature's forces which originated in the works of Isaac Newton.

The author illustrates the problems during this period in the development of a theory of affinity relating chemical and electrical force by a statement of W. R. Grove in 1874, "CHEMICAL AFFINITY . . . is that mode of force of which the human mind has hitherto formed the least definite idea." Unfortunately, this could also be the lament of the historian concerning the state of understanding of the history of chemistry during the first half of the 19th century. This book is but a modest contribution to this understanding.

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Salvage Archeology

Introduction to Middle Missouri Archeology. DONALD J. LEHMER. National Park Service, Washington, D.C., 1971 (available from the Superintendent of Documents, Washington). xiv, 206 pp., illus. $3.75. Anthropological Papers, 1.

To many young North American archeologists the accomplishments of the large-scale archeological salvage programs of the 1940's, '50's, and '60's have seemed questionable. Now we have a detailed summary of some of the work and its results by an archeologist intimately involved in both the field and the laboratory research. The Missouri River trough in North and South Dakota was the focus of the most extensive of these programs, and the administrative structure, the funding, and the organization of personnel and resources, together with the physical and intellectual accomplishments of this salvage effort, are the subject of Lehmer's *Introduction to Middle Missouri Archeology*. The volume should dispel some of the myths about the salvage program and make some of its inadequacies understandable.

The first part of the book deals with the history of the research and the second summarizes the results. The contents of the first section, however, avoid (and perhaps rightly) many of the emotion-laden issues raised by salvage. Lehmer notes the lack of problem orientation and the inadequacies of preexcavation survey and site evaluation but does not attempt to explain them. For example, the chapters on the origin of the program, its organizational, personnel, and funding problems (pp. 1–21, 35–38), provide a sketch of who did what and where and when, but there is little in the way of an analysis of the powerful social, political, and economic forces that relentlessly propelled the dam building program and made salvage a necessary nightmare. The book does not detail the mad scramble for money, equipment, and bureaucratic power typical of the world of salvage, a world in which the scientific merit of archeological research was measured by dam construction schedules and cubic yards of excavated dirt and debris and in which systematic research was an act of heroism in the face of overriding political necessity.

In the second, or synthetic, portion of his treatise, Lehmer uses a modified version of the Willey and Phillips phase, tradition, horizon system to "pigeonhole" (p. 25) the masses of data produced by salvage. In Lehmer's modification (i) the Willey and Phillips tradition is subdivided by introducing a new taxon, the variant, (ii) the horizon is considered an inappropriate integrative device and is discarded, and (iii) phase identifications, though deemed desirable, are restricted to the terminal end of the archeological sequence. Hence the variant is the taxon of greatest use. According to Lehmer a variant is "a unique and reasonably uniform expression of a cultural tradition which has a greater order of magnitude than a phase, and which is distinguished from other variants of the same tradition by its geographic distribution, age, and/or cultural content" (p. 32). This modification makes good taxonomic sense; it does not violate the logical structure of the Willey and Phillips formulation, and through its use Lehmer provides a lucid and useful diachronic comparison of artifact complexes, settlement patterns, and village types.

In sum, Lehmer's attempt to create taxonomic order from the extant data is the best one currently available. Nevertheless, this accomplishment must be viewed in the perspective of broader research aims and goals. The comparison of artifact inventories, settlement patterns, and village types and the construction of taxonomic orders do not in and of themselves provide testable propositions about the social phenomena with which they purport to deal. Nor do they serve to provide incentives for developing new research strategies. I do not mean to imply that rigorous artifact comparisons or well-designed taxonomic orders are not important—they most certainly are, but as one of several alternative means to the end of explicating prehistoric human behavior. Lehmer has provided a useful beginning from which we can proceed—it is to be hoped in a systematic manner.

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Plants in the Tropics

Introduction à la Phytogéographie des Pays Tropicaux. Les Problèmes Généraux. R. SCHNELL. 2 vols. Vol. 1, Les Flores, les Structures. xvi pp. + pp. 1–500. 180 F. Vol. 2, Les Milieux, les Groupements Végétaux. viii pp. + pp. 501–952. 165 F. Gauthier-Villars, Paris, 1971. Géobiologie, Ecologie, Aménagement.

For many years, major contributions to the understanding of West African ecology have been made by French biologists, particularly through the agency of the Institut Français d'Afrique Noire. The names of August Chevalier, Theodore Monod, André Aubréville, Paul Jaeger, and Raymond Schnell come to mind readily. *La Forêt Dense* by the last author has been particularly influential on thinking about rain forests in that area. Now Schnell, who began his studies of tropical botany 31 years ago, has taken on a larger task. In two volumes—almost a thousand pages—he reviews the phytogeography of tropical lands in general. The book was 19 years in the writing (1950–69) and is based on a course given by the author at the Sorbonne. It is no mere account of the distributions of the plants; the treatment is a synthesis of geographical material with detailed morphological, environmental, vegetational, and historical information (together with a few physiological data), so that it merits the adjective ecological.

The first volume contains two parts, devoted respectively to the historical geography of the tropics (11 chapters) and the structures and "biology" (autecology) of tropical plants (12 chapters). The second volume's first part deals with interactions between tropical plants and their environments (8 chapters) and its second is concerned with types of tropical vegetation and their dynamics (11 chapters).

The importance of this work is accentuated by our desperate need for information on which to base the com-