cian equator inclined 70 degrees with respect to the present one, the Ordovician tropics having passed through what are now Ecuador, Panama, Newfoundland, central Russia, and East Pakistan. South America and Africa would thus have to have been widely separated, in contrast to the later Gondwana configuration accepted by many, and Fell's equator would pass through the position of the Ordovician Gondwana Pole as recently computed by Fairbridge. Why didn't Fell discuss his subject in the light of polar wandering curve superpositions, analysis of diversity gradients such as those Stehli studies, or even concepts of sea floor spreading available when his article was written? The article is virtually nonquantitative in spite of abundant and suitable data. Kühne's paper is marred by muddy philosophy of systematics, violations of the Code of Zoological Nomenclature (*nomina nuda*), and various inadequately documented assertions (trends to increase the number of cheek teeth in several supposed lineages, reference to upper cheek-tooth formulas of *Paulchoffatia*, and others). Kühne's figure 3 appears to be of a single specimen, not three. These are matters that should have been rectified by the editor if not by the author.

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Complexities of a Provincial Scientist

John Dalton and the Progress of Science. Papers presented to a conference, Manchester, England, 1966, to mark the bicentenary of Dalton's birth. D. S. L. CARDWELL, Ed. Manchester University Press, Manchester; Barnes and Noble, New York, 1968. xxii + 352 pp., illus. $9.50.

The bicentenary of John Dalton's birth was celebrated in Manchester in September 1966 by two conferences. One was intended primarily for specialists in the history of science; the other was more general and aimed at the public. The papers from both these conferences make up the present volume.

The essays gathered together here are most successful in destroying what I think may be called the myth of John Dalton. This is the Dalton one usually meets in science textbooks or popular histories — the simple, self-educated Quaker drawing his inspiration and most of his knowledge from Newton's *Principia* and *Optics*, developing his epoch-making theory almost in an intellectual vacuum and failing to understand the full implications of what he had created; retaining his simplicity even at Manchester as a schoolteacher, and dying as the scientific world swept by him. The Dalton one meets in this volume is a much more complicated and scientifically sophisticated person, and both his milieu and his understanding of it are shown to be much more complicated than the myth would have it. His appreciation of the implications of what he had done and his struggles to avoid having his atomic doctrine swallowed up in others' theories are also delineated with skill and sensitivity.

The general level of the articles is very high indeed, which makes it impracticable to single out individual authors and titles for praise. Let me just strongly urge all historians of science and others interested in Dalton and the origin of the atomic doctrine to purchase the book and enjoy themselves reading it.

The one serious criticism I would make is of the quality of the illustrations. This really affects only one contribution, that by Kathleen R. Farrar on "Dalton's scientific apparatus." This worthy article, which successfully destroys the myth of Dalton as a founding member of the "ink bottle, beeswax, and string" school of British science, is accompanied by some of the muddiest figures and plates I have ever seen. Details are impossible to discern in the figures, and plate IVa is simply a jumble of flasks and barometers and what have you. Plate Va, to which reference is made in the text (p. 180), simply does not exist. Plate V itself appears opposite page 171, and for 150 pages I thought that it had been mistakenly included from another volume published by the Manchester University Press. It is of "The internal structure of a segment of a receptor from the retina of the rhesus monkey as seen under the electron microscope." It was not until page 322 that the plate was linked up to Dalton's colorblindness.

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Biological Variability in Man

La Diversité Humaine en Afrique Subsaharienne. Recherches Biologiques, Etudes Ethnologiques. JEAN HIERNAUX. Editions de l'Institut de Sociologie, Université Libre de Bruxelles, Brussels, Belgium, 1968. 261 pp., illus.

In this interesting monograph, Jean Hiernaux assesses the distribution of a number of monogenic and polygenic features in African peoples that inhabit regions south of 22°N latitude. He uses sophisticated statistical techniques to express the patterns of human variability and the taxonomic distance between populations and to test correlations between certain phenotypic and genetic traits and selected climatic factors.

The features chosen for detailed study include 10 gene frequencies (I^A, I^B, I^O, R_0, R_1, R_2, $r + R_0{}^u$, L^M, Hb^S, and Hb^C), the frequencies of arches, loops, and whorls on the fingers, and 18 traditional anthropometric dimensions. The biometric features are considered to be multifactorially determined while the biomolecular traits are controlled by simpler genetic mechanisms.

Hiernaux computes the coefficients of correlation between the mean values of the gene frequencies and morphological dimensions and between these features and each of six climatic factors—mean annual rainfall, mean value of humidity in the wettest month, mean value of humidity in the driest month, mean daily temperature in the hottest month, mean daily temperature in the coldest month, and mean altitude. Further, he uses partial coefficients of correlation strategically to test independently the possible effects of each climatic factor.

In general, Hiernaux found that the biometric traits exhibit higher correlations with mean annual rainfall and maximum temperature in the hottest month than with other climatic factors. By contrast, the biomolecular and dermatoglyphic traits generally do not evidence such notable correlations with major climatic factors. Thus, in explaining ABO and abnormal hemoglobin distributions, Hiernaux places primary emphasis on disease vectors and other microclimatic factors instead of on gross climatic factors. He believes that human phenotypes are notably subjected to selective forces of the environment and that even in instances where rather recent admixture has occurred, the descendant populations may reflect,

to some extent, selective biases of the environment on the new gene pool.

But historical factors are not always considered to be of secondary importance by Hiernaux. He suggests contacts with peoples of Arabia (instead of correlations with altitude) to explain the distribution of R_0 and L^M frequencies and certain biometrical features in the peoples of the Ethiopian highlands.

A particularly interesting approach used by Hiernaux is to compare independently the constituents of classical anthropometric indices with climatic variables. His results indicate that previous authors have been too simplistic in interpreting the meaning of these indices. For example, the two fundamental dimensions of the nose appear to be responsive to quite different climatic factors. Nasal breadth is highly correlated with mean annual rainfall and mean temperature in the hottest month, whereas nasal height is relatively unaffected by these factors. By contrast, nasal height is correlated notably with mean humidity in the coldest month and is relatively independent of the climatic factors that are highly correlated with nasal breadth.

In the final section, Hiernaux discusses the need to base classifications of human variability solely on biological information (as opposed to cultural and linguistic criteria). He offers a model of a modern biomathematical approach and gives examples of how results of his sophisticated statistical studies contrast with the opinions of several authors who have dealt previously (often in a typological manner) with sub-Saharan peoples. Hiernaux judiciously avoids erecting a "new" classification on the basis of his extensive, though admittedly still incomplete, data.

A work of such scope carries with it from the outset many difficulties that decrease the reliability of its results and conclusions. Hiernaux relies in great part on the published mensurations of many other scientists from different schools. Moreover, many characteristics, such as skin, eye, and hair coloration, pilosity, subcutaneous fat distribution, ear form, and other features commonly used in studies on "race" are omitted owing to the paucity of quantitative data in the literature. But Hiernaux generally discusses freely the inadequacies of conclusions based on available data.

The major value of Hiernaux's approach lies in its objectivity and emphasis on the quantitative expression of variability. It is to be hoped that members of the International Biological Programme and other research teams will soon develop and adopt uniform techniques that will not only reduce biases of human biologists in collecting data but also express quantitatively those features that could not be included in Hiernaux's pilot study.

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Scoring the University

The Closed Corporation. American Universities in Crisis. JAMES RIDGEWAY. Random House, New York, 1968. xii + 276 pp., $5.95.

Time and again during the series of crises at Berkeley, as I've listened to various accounts of what the "real" trouble is, I've been moved to ask myself the question of the old spiritual, "Were you there?" Like the blind men in the old tale who went to see the elephant, each one of us seems to have concentrated on one feature of the situation and to have attempted to amplify his picture into an image of the whole system. And this is not too difficult a task to perform, especially if you are angry. Nothing can shake the conviction of a Reagan or a Rafferty that the whole business of student violence is the work of a few highly trained revolutionaries of the red race. Nor can the facts in the least disturb the professional mind that has fixed the blame on a plot against academic freedom.

Now James Ridgeway gives us another image, in which the university of America is depicted as a very shoddy institution. Its professors meddle in politics and business in ways that range from the naive to the immoral. Its administrators are not merely indifferent to the problems of the community surrounding the university; some, as at Columbia and Chicago, are actively engaged in trying to rid their places of "undesirable" blacks, and could not care less where the good riddance goes. Perhaps shoddiest of all are the trustees, giants in the business world but hopelessly inept and dangerous in the world of education. The poor undergraduate is kept in a "holding pen" until fat enough for the market.

As I read this indictment, I couldn't help feeling again that the simple story was largely irrelevant. I agree wholeheartedly that the make-up of the Regents of the University of California is very unhealthy for the university, and the method of appointment and tenure is extremely bad. But even supposing, as Ridgeway suggests, that the Regents were elected, with the help of the students, for reasonable terms, and even supposing the professors kept out of foolish entanglements with other institutions, and even supposing the administrators refused to serve on industrial boards of directors, what then? It seems to me nonsense to assume that making universities less shoddy in these respects will make them better institutions of learning.

My own biased viewpoint on the "crisis" is that universities have lost their philosophical basis. I don't merely mean that philosophy is no longer being taught by philosophy departments, though this is no doubt true. I mean that none of the leaders, faculty, student, or administration, seems able to say anything very significant about the meaning and purpose of higher education. Consider, for example, the "no nonsense, hard-line" policy of some politicians and some educators (Hayakawa is a recent example). These men tell us that there is a vast "silent majority" who merely wish to be educated. Educated for what? The point is well illustrated by Ridgeway's book. "The principle that should govern higher education," he says, "is surely simple enough: Since educational institutions are generally regarded as serving a public function, and financed to a large extent by the general citizenry, they ought to be responsible to the public." Which is true and trite if "responsible to the public" means "serving the true needs of those who deserve to be educated," or else false and insidious if the phrase means "performing in a manner which pleases the majority of the electorate."

Of course it would be unfair to criticize Ridgeway's book for its lack of philosophy, because he is frankly writing as a journalist. Indeed, the great success of journalistic indictments of the auto industry, the drug industry, the air travel industry, and now the educational industry is partly to be explained by the shallowness of the philosophical base of our culture. But in the case of universities there must surely be some hope that a community of scholars and students can turn its attention to acquiring a deeper understanding of why such a community should exist.