intellectual justification for racial persecution. But Darlington suggests that its real import is just the opposite and that genetic differentiation of local populations is favourable to evolutionary advance. " The assumption of a genetic basis for race and class differences," he says, " provides the evidence, the only scientific evidence, in favour of racial tolerance and co-operation. . . . The future rests with those genetically diverse groups, whether races or classes, which can practise mutual help and show mutual respect."

These are the hard facts of life. I call them hard for two reasons. They are hard in the sense that they are only a little way removed from solid and careful experiments ; and hard also because they emphasise the limitations which the genetic constitution imposes on human development. But I think there is another group of facts, which I might call soft facts, to which Darlington pays too little attention. They are soft both because their experimental basis is not so clear cut, and because their implications are kinder to our idealism. They are the facts about the social mechanism of transmitting ideas and practices from one generation to the next. Man has in fact developed a completely new type of heredity, which enables him to pass on to later generations the ability, say, to fly, by a means in which genes are not *directly* concerned at all, that is to say by education. This is a process at a higher level of organisation than the genetic mechanisms of animal evolution. It must have its own laws. A new gene persists in evolution if it is favoured by natural selection. What decides whether a new concept or idea will persist? We simply do not know. Certainly it is not its sheer usefulness to mankind, or we should have had universal peace for many centuries by now. But there is here, I think, a whole science waiting to be worked out, as complicated as that of animal evolution, and for human affairs even more important. Particularly if we try to compare human races, it seems impossible to make much progress until we can assess the importance of their different cultural traditions in determining how their genetic potentialities become realised. My own opinion is that up to the present we know next to nothing about the whole subject ; and that all we can usefully say about races is that they almost certainly must be genetically different, that it is probably a good thing that they should be, but that we have no idea how different they are or what the differences consist in.

The vigour of Darlington's thought, and prose, and the combination of a highly individual point of view with a wide field of scientific knowledge, will, I think, cause this book to be quoted for a long time as an important mid-twentieth century opinion about the cultural influence of biology.

C. H. WADDINGTON.

Rh-Hr BLOOD TYPES. Applications in Clinical and Legal Medicine and Anthropology. Selected Articles in Immunohematology. By Alexander S. Wiener, M.D., F.A.C.P., New York : Grune and Stratton. 1954. Pp. 763. $11.50.

In the fifteen years since the discovery of the Rh groups there has been continuous progress in our knowledge of their serology, genetics and applications. The field has grown to be large and complex, so that there must be many people who would welcome an authoritative monograph on the Rh groups. The scope is indicated in the second subtitle : the volume comprises, in fact, a selection of 82 papers published by Dr Wiener and his colleagues and two papers published by Dr Wexler in support of Wiener's contentions, together with notes on technique and a bibliography listing 333 papers published by Wiener in the years 1929 to 1953.

The declared objective of the anthology appears in the preface: "It seemed worthwhile to collect in one volume the author's most representative and important contributions to this subject, thus giving an idea of how knowledge in this field developed."

This bold identification of author with subject sets the tenor of the whole volume. None would wish to deny the importance of Dr Wiener's contributions to the serology of the Rh groups. The discovery of the Rh groups by Landsteiner and the author; the demonstration with Peters of Rh immunisation as a cause of hæmolytic transfusion reactions; the pioneer work on "blocking" or "incomplete" antibodies and their placental permeability; the applications of these findings to hæmolytic disease of the newborn and forensic medicine: writings on all these subjects are included, and they show the power and versatility of Wiener's mind and the competence with which he can handle difficult material.

Nevertheless, it scarcely needs to be pointed out that others have tilled the same field and have reaped results of comparable importance. The reader will seek in vain a reasoned account and criticism of the work of Levine, Fisher, Race and Mourant, not to mention others. Wiener's references to other authorities are usually grudging, sometimes openly hostile. There is evidence throughout the volume of a tendency to rake over the ashes and rekindle the dying embers of a controversy long since grown wearisome. The controversy concerns, of course, the Rh nomenclature; Dr Wiener repeatedly stresses the advantages of his own notation and the drawbacks, even absurdities, of the CDEF notation.

"Controversy" is hardly the word. Dr Wiener has been recognised these many years as the champion of the Rh-Hr notation, and it seems that he is still poised, pen in hand, ready to do battle at any time and place. The protagonists of the CDEF notation, however, have been unwilling to enter the lists. They have relied, rather, on the arts of peace and gentle persuasion, and they have confidently let their notation plead for itself. One is forced to conclude from Dr Wiener's protestations, that the CDEF notation is gaining general support. There must be few geneticists to-day who doubt the soundness of the theoretical foundation of Fisher's and Race's hypothesis and the consistency and essential correctness of the genetical interpretation which has been developed upon it. This construction is a good deal more substantial than Don Quixote's windmills; small wonder that to tilt against it is discomfiting.

Dr Wiener's conduct sometimes seems unchivalrous. The tone of his asides can be judged from this quotation (p. 364): "I.M. Jaundiced, a poet residing at 36 Genotype St., High Titer, R.H., has celebrated the discovery of little f by composing a song entitled, 'C, D, E, F, . . . Gee!'" Moreover, he uses the symbol F for the Duffy antigen, which had previously, and with genetical precision, been designated Fy^a, and he supports an earlier suggestion that the symbol C should refer to an antigen of the ABO complex. The adoption of these terms can hardly have had any purpose other than that of introducing confusion into the CDEF camp.

The arguments for and against each of the two notations need not be recapitulated here. To the pathologist it is of very little practical importance whether there is in the Rh groups a complicated system of multiple allelomorphs or a series of four closely linked factors. What the pathologist requires is a system of nomenclature that is easy to learn and to use. It is, of course, possible to master almost any system of notation, just as it is possible to master the cuneiform script and the Boolean algebra of symbolic logic. However, we are concerned not with intellectual gymnastics but with a system which is rational and easily workable in practice. Even consistency and genetical precision can justifiably be sacrificed : the use of the symbols ABO to designate allelomorphs as well as antigens already defies current genetical practice. To the reviewer it seems that there may be real advantages in the retention of a duplicate system of notation for this purpose. Thus, the Rh notation allows concise designation of the phenotypes of the antigens and also of the corresponding chromosomes. But the CDEF notation undoubtedly simplifies the genetical interpretation, thereby rendering the whole system easier to learn and remember, and it allows concise designation of antibodies. Once a system of nomenclature is laid down it should be adhered to and not tampered with year by year, as Wiener has shown a tendency to do with the Rh notation. The difficult question of a convenient terminology will not be decided here and now, in any case ; it will be resolved only by time and future custom.

On the other hand, to the geneticist and the evolutionist who is interested in the different ways in which particular genetic situations can arise and establish themselves in natural populations, the distinction between linked factors and multiple allelomorphs is of the greatest importance. The utmost possible precision in notation is also essential, and there is little doubt that this is better supplied by the symbols of Fisher and Race than by those of Wiener. The question of priority need not be laboured : in genetics the best notation has precedence over the earliest notation, thereby avoiding the stultifying effects which strict adherence to priority has sometimes produced in taxonomy. Even in the Fisher-Race system of notation, there should be separate and always distinct terms for genes, antigens and antibodies. The letters themselves, ' C, c, . . . " should refer to the genes concerned, the corresponding antigens should be designated " C-antigen, c-antigen . . . . " and the antibody can be conveniently termed, as it always has been, " anti-C " or " anti-c ".

As far as polemic is concerned, the lesson of the past is plain for all to read. The history of science includes a succession of acrimonious disputes, and it can be said with confidence that the reputation of the disputants invariably suffered, the advancement of knowledge often being held up for a generation or longer. Thus, the haggling of Newton and his followers with Leibnitz and his followers over priority in the discovery of the infinitesimal calculus was damaging to the characters of both great men. And the obstinate British were almost barren mathematically during the succeeding century while the more progressive Swiss and French perfected the calculus and made it the simple, easily applied implement of research that Newton's successors should have had the honour of making it. Moreover, Newton's dispute with Huyghens over the nature of light might in happier circumstances have been resolved so as to show that there was right on both sides, thereby opening the way to the unified corpuscular and undulatory theory which was developed only very much later. There is, again, the case of Priestley who adhered to the Phlogiston hypothesis even after his own experiments had made it untenable. Not convinced by the brilliant researches of Lavoisier, Priestley remarked, " I have well considered all that my opponents have advanced, and I feel perfectly confident of the ground I stand upon. . . . Though nearly alone I am under no apprehension of defeat."

Compare these unfortunate incidents with the honourable solution of the question of priority in the recognition of natural selection as an agent bringing about evolutionary changes. Lyell and Hooker sent to the Secretary of the Linnaean Society the following letter, referring to Darwin and Wallace : " These gentlemen having, independently and unknown to one another, conceived the same very ingenious theory to account for the appearance and perpetuation of varieties and of specific forms on our planet, may both fairly claim the merit of being original thinkers in this important line of enquiry ; but neither of them having published their views, though Mr Darwin has for many years past been repeatedly urged by us to do so, and both authors having unreservedly placed their papers in our hands, we think it would best promote the interests of science that a selection of them should be laid before the Linnaean Society."

Again, Pasteur, having shown that optically active organic compounds rotate the plane of polarised light even when in solution, drew the bold, and, as it turned out, correct conclusion that the molecules of such compounds are themselves asymmetrical. This led him into conflict with Biot, who was not without doubts regarding the accuracy of Pasteur's observations. Biot called Pasteur before the Academy, handed to him some racemic tartaric acid, soda and ammonia, and bade him repeat the experiments. The crystals prepared, Biot himself made the solutions and examined them in the polarising apparatus. Then, in Pasteur's own words, " Without even making a measurement, he saw by the appearance of the tints of the two images, ordinary and extraordinary, that there was a strong deviation to the left. Then, very visibly affected, the illustrious old man took me by the arm and said, ' My dear child, I have loved science so much all my life that this makes my heart throb.' "

All things considered, *Rh-Hr Blood Types* is at once an example and a cautionary tale. The example appears in the speed and deftness with which a seemingly trivial observation (that a rabbit antiserum against monkey red cells agglutinates some but not all human cells) was exploited in the brilliant serological discoveries concerning transfusion reactions and *erythroblastosis fœtalis*. The other side appears in Wiener's reluctance to recognise the profound significance of the work of others in the field which he has come to regard as his own preserve. There are no monopolies in science.

ANTHONY ALLISON.

AN Rh-Hr SYLLABUS. The Types and Their Applications. By Alexander S. Wiener, M.D., F.A.C.P., F.C.A.P. New York : Grune and Stratton. 1954. Pp. 82. $3.75.

The purpose of this booklet is to present an up-to-date summary of the Rh groups and their applications to clinical and forensic medicine and anthropology in a compact, easily understandable form. It is intended as an introduction : " for readers not specialising in the field, it contains all the information they require and will make it possible for them to read and understand without difficulty current articles on the subject,