exposed. The latest report of the U.S. Commissioner of Education deals with the year ending June 30, 1905. The total income for that year of American institutions of university rank, excluding benefactions, amounted to 8,355,000*l.*, an increase of 289,200*l.* over the preceding year, and of this amount 23·6 per cent. was from State appropriations and 6·9 per cent. from Federal appropriations. That is, more than 2,506,500*l.* was provided from American public funds for higher education during the year with which the report deals. A very generous estimate of the amount provided here from public funds for higher education of every kind, including the Royal Colleges of Science of London and Dublin, the universities and the university colleges, would be to place it at a quarter of a million pounds sterling, so that the British case is in no way improved by importing the question of the amounts provided for higher education.

Sir W. T. Thiselton-Dyer is doubtful only about the greater belief of American statesmen in the need for the introduction of scientific methods in the solution of problems of government, but of the need of scientific ways of thinking on the part of our legislators he is quite convinced, and that is really the important matter.

A. T. S.

The Interpretation of Mendelian Phenomena.

APROPOS of the discussion on the interpretation of Mendelian phenomena, may I seek enlightenment on one or two points from your readers? Mendelian phenomena are possible only when reproduction is bi-parental. They cannot occur, of course, when it is parthenogenetic. I believe I am right in thinking that Mendelian workers suppose or hope that they have found a master key to the problems of heredity. Now, I am able to understand that the study of alternative inheritance may ultimately shed a light on the function of sex, but I find it difficult to conceive how it can shed a light on any other biological problem of importance; for example, the problems of the alleged transmission of acquirements, of the causation of variations, of the retrogression of characters which have lost selection value, and of the mode of development (whether or not by the recapitulation of the phylogeny). All these problems are of at least equal importance to the problem of sex. I have sought information from my Mendelian acquaintances, but I am always told that we must await the accumulation of data—a somewhat Micawber-like attitude, as it seems to me. I hope I make myself clear. The information I seek would be contained in the answer to the following question:—If Mendelism has a bearing on any biological problem save that of sex, what is that problem? If, as I anticipate, no one is able to name another problem, I venture to suggest that Mendelians are engaged in nothing more than the investigation of sex.

Mendelian phenomena have been observed principally in crossed artificial varieties of animals and plants. Crossed natural varieties usually blend their characteristics. This is conspicuously the case with man, the animal who, so far as is known, has crossed more often than any other, and whose hybrids may be observed up to the tenth or twelfth generation in South America and elsewhere. It has been said that "human skin-colour is the only character that is known to blend perfectly"; but this statement is certainly incorrect. With the exception of eye-colour, and possibly one or two other traits, such as the Mongolian eyelid, human hybrids appear to blend every character as perfectly as skin-colour. The transmission of no character is Mendelian. Thus mulattos have the black eye of the negro, and when they breed *inter se* continue to reproduce it indefinitely. There is no segregation. May I mention one other fact which is of considerable interest, but which seems to have escaped the attention of Mendelian workers? Crossed artificial varieties usually reveal latent characters in abundance. I am aware that the correctness of the term latency has been disputed, but it will serve to indicate what I mean. So far as I have been able to ascertain, no single instance of a latent character resulting from the crossing of natural varieties has been recorded. Certainly crossed human varieties reveal no such traits. A very significant passage bearing on this matter may be found in "Animals and Plants" (vol. ii., pp. 24–5). It would appear, then, that characters become latent only under conditions of artificial selection, that is, when mutations are selected. It has been maintained that nature also selects only mutations, but, to say the least, this has not been demonstrated as yet.

Bearing in mind, then, the facts that latent characters appear only when artificial varieties are crossed, and that crossed natural varieties usually blend their characters, the question arises whether Mendelians, so far from investigating even the whole problem of sex, are engaged in anything more than the investigation of those abnormalities of sexual reproduction which occur under conditions of artificial selection.

G. ARCHDALL REID.

Southsea, September 17.

On Correlation and the Methods of Modern Statistics.

PROF. KARL PEARSON'S letter in NATURE of September 19 gives me a welcome opportunity of explaining what was not intelligible in the condensed report of my remarks in the discussion at Leicester, on methods of modern statistics.

Prof. Pearson communicated to the Royal Astronomical Society (Monthly Notices, May, 1906) a paper by Miss Winifred Gibson, giving an account of a research conducted in the statistical laboratory of University College, London. The first part of this paper discussed the relation between parallax and magnitude of the stars. I confessed to some misgivings as to the astronomical value of the results, and raised two questions, first, as to the method, and second, as to the matter.

Prof. Pearson thinks that I am on safer ground in the second than in the first. I will therefore examine first his reply to my second point, which was that the parallax material contained in Newcomb's table (appendix to "The Stars") is quite unsuitable for discussion by a general statistical method, since it relates very largely to stars selected for investigation because of abnormal proper motion.

Prof. Pearson "fancies that astronomers have been guilty of a considerable amount of circular reasoning. They start from the hypothesis that magnitude is very closely related to parallax. . . . The fundamental hypothesis that the brighter stars are much the nearer as yet awaits statistical demonstration. . . . Surely the hypotheses of high relationships between magnitude and parallax and proper motion are of sufficient importance to deserve *proof*, rather than to be taken as axiomatic." In this matter Prof. Pearson is under a misapprehension. Astronomers do not believe that magnitude is very closely related to parallax; very obviously it is not. But they do believe that parallax is somewhat closely related to proper motion. There are seventeen stars in the sky brighter than mag. 1·5, and their parallaxes have been determined with the heliometers at the Cape and at Yale. Here are the results.

Seven have proper motions (on a great circle) less than 0″·1 per annum:—

	P.M.	Parallax	Mag.
	″	″	
Canopus	0·00	0·00	1·0
Deneb	0·00	0·00	1·3
Rigel	0·01	0·00	0·3
Betelguese	0·03	0·02	0·9
Antares	0·03	0·02	1·3
Achernar	0·09	0·04	0·5
β Centauri	0·09	0·05	0·8
Mean ...	0·04	0·02	0·6

Six have proper motions between 0″·1 and 1″·0:—

	P.M.	Parallax	Mag.
	″	″	
Aldebaran	0·19	0·11	1·1
Regulus	0·27	0·02	1·3
Vega	0·36	0·11	0·1
Capella	0·43	0·09	0·2
Pollux	0·64	0·06	1·2
Altair	0·65	0·23	0·9
Mean ...	0·42	0·10	0·8