

Why could not the record before us be published in England as well as in America? This seems a very feasible plan, and would doubtless add to the usefulness of the work, inasmuch as English collaborators might be added.

Very different from the American annual is the English year-book, yet it is, we believe, the only "year-book of science" of which we can boast. Outside it resembles a shilling railway novel; inside it is a pleasant gossiping account of odds and ends of science picked up at the Royal Institution. An altogether disproportionate amount of space is devoted to extracts from the papers and addresses of Prof. Tyndall, and the woodcuts on the title-page are taken from the same source. We are glad, however, that the "Year-book of Facts" still remains, notwithstanding the death of its former indefatigable compiler. Mr. Vincent tells us he undertook at short notice to continue the work of the late Mr. Timbs. To compile a year-book under such circumstances can be no light duty, and hence we must be lenient to its shortcomings. So far as the book goes, Mr. Vincent has done his work well, and gives a bill of fare that no doubt will be relished by the *dilettante* scientific public. But it should be clearly understood that the volume is merely a *scrap-book* of popular science, and not in any sense an annual register, such as we hope may soon be issued.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Systems of Consanguinity

IN Sir John Lubbock's *vindication* of his original charge that I seem to have two theories of the facts in my work on Consanguinity (*NATURE*, vol. xii. p. 124), he fails to show that the classificatory system was interpreted by me as "arbitrary, artificial, and intentional." This is one of the theories, and in fact the principal one, which he ascribes to me, and which I repudiate. The other theory, that which I did advocate, is presented both in his address before the Anthropological Institute and in this *vindication* (stated partially and imperfectly), as something that I "admit." "Mr. Morgan admits that systems of relationship have undergone a gradual development, following that of the social system." (Address, p. 4, *NATURE*, vol. xii. p. 125.)

It would require too free a use of your columns to explain at length how, by quotations severed from their connections, and by a use of their phraseology not in accordance with my design, a defence of an unwarranted statement has been put together.

I beg leave to re-state the propositions in my work on Consanguinity, which contain the substance of the views I have advocated, and to which I stand committed; and to request those who may be interested in the subject to read the last chapter in the light of these statements.

In that chapter, entitled "General Results," the facts are discussed under seven propositions, in substance the following:—

Proposition I. That the systems of consanguinity given in the tables may be resolved into two, which are radically distinct, one of which is called *descriptive* and the other *classificatory*. The first is that of the Aryan, Semitic and Uralian families, and the second that of the Malayan, Turanian, and Ganowánian families.

Proposition II. That these systems are to be ranked as domestic institutions.

Proposition III. (in full). "Can the origin of the descriptive system be accounted for and explained, from the nature of descents and upon the principle of natural suggestion, on the assumption of the antecedent existence of marriage between single pairs?" (Con. p. 472.)

The affirmative of this proposition is maintained. "It is the institution of marriage between single pairs which teaches the descriptive system; whilst this form of marriage has been taught by nature through the slow growth of the experience of the ages." (Con. p. 469.)

Proposition IV. (in full). "Can the origin of the classificatory

system be accounted for and explained, from the nature of descents, upon the assumption of the existence of a series of customs and institutions antecedent to a state of marriage between single pairs, of which the Hawaiian custom is one?" (Ib. p. 474.)

The affirmative of this proposition is likewise maintained. Under it the solution of the origin of the Malayan system is given, and also of the Turanian, together with the customs and institutions, fifteen in number, arranged in a sequence, which stand connected with the birth and growth of these systems. Assuming, for example, the intermarriage of brothers and sisters in a group, every relationship in the Malayan system is found to be that which would actually exist; wherefore, the system itself proves the antecedent existence of this form of marriage. The same line of argument and of inference is then applied to the Turanian system. In Propositions III. and IV. I speak of both forms as *natural* in contradistinction to *artificial*, although they are radically different. They are natural in the sense that they are in accordance with descents as they actually existed when each system respectively was formed. This is the main proposition in that chapter, occupying in its discussion nineteen of its forty-three pages. It presents the theory of the author; it is the only place where the origin of the classificatory system is discussed.

Proposition V. This proposition maintains the unity of origin of such tribes of the American aborigines as are found to possess an identical system of consanguinity.

Proposition VI. (in full). "Where two or more families, constituted independently on the basis of such a system of relationship, are found in disconnected areas or upon different continents, can their genealogical connection be legitimately inferred from their joint possession of the same system?" (Ib. p. 498.)

After showing that the people of South India who speak the Tamil, Telugu, and Canarese dialects have a system of consanguinity identical with that of the Seneca-Iroquois of New York, in upwards of 200 relationships, the question is raised, "How shall this identity be explained?" It was my discussion of this question that confounded my distinguished adversary, which he misunderstood at first, and is not sure that he "quite comprehends even now." How his difficulty could have arisen I confess puzzles me. Under Proposition III. the origin of the descriptive system had been discussed, and under Proposition IV. that of the classificatory; but under this (VI.) the question was whether any evidence of the Asiatic origin of the Ganowánian family could be found in this identity of systems. The four hypotheses quoted by him (vol. xii. p. 124) are produced and discussed here. "Spontaneous growth" was referred to and of course rejected as an adequate explanation of this identity of systems.

Proposition VII. relates to inferences that may be drawn from partial identity of systems.

These several propositions show very plainly, I submit, that these systems are not explained in that volume as "arbitrary, artificial, and intentional," and equally plainly that they are explained as growths or results of certain customs and institutions.

Turning now to Sir John Lubbock's *vindication*, his first principal quotation is taken from the discussion of my first proposition, where "natural and spontaneous" is used in opposition to resulting growths from customs and institutions, the cause being unknown in the first case, and known in the second. His second quotation is from the discussion of my sixth proposition, where "spontaneous growth" is used, and in the same sense.

The discussion of the mass of materials accumulated in that volume was confined to forty-three out of five hundred and eighty-three pages. It was a new subject, in which it was necessary to invent, to some extent, a new terminology. I am aware of its great defects, but I deny that two theories of the facts are to be found therein, or that I have explained the classificatory system as "arbitrary, artificial, and intentional," which is the point from which this discussion started.

Rochester, New York, July 20 LEWIS H. MORGAN

Weather on the Atlantic

I HAVE reluctantly come to the conclusion that attempts to forecast the weather on the North Atlantic frequently result in disappointment. A recent passage from New York to this country has enabled me to gather some remarkable data on this subject, so remarkable, indeed, that any one crossing for the first time might reasonably question the action of the barometer. If I had had only one on board, I should certainly have doubted its