other country in intellectual interests. But America has a special reason for paying her respects to the genius of Helmholtz, since Helmholtz in his seventy-second year paid his tribute of respect to the genius of America. One year before his death he crossed the ocean to study and to enjoy the scientific institutions of this country from the Atlantic to the Rocky Mountains, certainly the most famous European who has visited America for many years, and nobody who saw his noble personality in New York or Boston or Baltimore, in Philadelphia or Washington or Chicago, will ever forget him.

The American members of the International Committee are Dr. Wolcott Gibbs, President of the National Academy of Sciences; Dr. Herman Knapp, Professor of Columbia College; and Dr. Hugo Münsterberg, Professor of Harvard University.

Contributions may be sent before May 25th to the undersigned Secretary and Treasurer of the American Committee. The lists of contributors will be published weekly in SCIENCE.

HUGO MÜNSTERBERG.

38 QUINCY STREET, CAMBRIDGE, MASS.

SCIENTIFIC LITERATURE.

Manual of Geology. By JAMES D. DANA. Fourth Edition. American Book Co. 1895.

The announcement, a few months ago, of a new edition of Dana's Manual filled geologists with liveliest expectations. It is needless to say that these expectations are more than realized. The Manual is so well known that a full account is wholly unnecessary—geologists need no urging to buy it. They simply must have it; they cannot do without it. I write this, therefore, not to call attention to the book; but partly because I am glad to have this opportunity to express my unstinted admiration for the author and for the book; and partly because I wish to draw attention to the author's position on some important questions which have come into prominence since the last edition.

1. Every geologist will be gratified to see that the author now comes out frankly for evolution; not, indeed, evolution in a materialistic sense, but in a reverent, theistic sense. In a certain Agassizian sense he has always been an evolutionist, but he has been often quoted by the *opponents* of evolution as now understood (*i. e.*, 'origin of organic forms by descent with modifications') as sustaining their position. In this edition his utterances are not to be any longer mistaken; although he is, perhaps, more nearly Lamarckian than Darwinian, or, at least, than Neo-Darwinian. Surely such plasticity and open receptiveness of mind retained even to the very last is a noble evidence of the true scientific spirit.

2. In this edition he separates the Palæozoic into two primary divisions with *Eo-Palæozoic*, including the Cambrian and Lower Silurian, and the *Neo-Palæozoic*, including the Upper Silurian, Devonian and Carbonic. Thus he makes the greatest break occur between the Lower and Upper Silurian. If this be so, would it not be better to use Lapworth's term 'Ordovician' for Lower Silurian, retaining the term Silurian for the Upper Silurian alone? Probably this would violate the priority-rule of nomenclature; but, perhaps in this, as in many other cases, rules too strictly interpreted stand in the way of a rational classification.

3. He accepts the probability of a Permian glaciation, especially in the Southern Hemisphere; and of an elevation and enlargement of an Antarctic continent and its connection with the southern points of South America, South Africa and Australia as a cause of such glaciation. These great changes of physical geography and climate, and consequent wide migrations of faunas and floras, would go far to account for the

enormous and apparently sudden changes in organic forms which took place during and at the end of the Permian period.

4. In connection with the last he accepts also the idea of a land-connection (Gondwanaland) between *India and South Africa*, and perhaps indirectly through the enlarged Antarctic continent—with Australia—in Permian and Triassic times, as evidenced by the great similarity of the plants and the reptiles of that time in these now widely separated countries. It is true that there is very deep sea between these points now; but it is possible that the idea of the permanence of deep sea basins, originated by Dana, may have been pushed a trifle too far by Wallace as a means of separating faunas and floras.

5. He does not accept Algonkian as a system of rocks coördinate with Palæozoic and Mesozoic, but regards these pre-Cambrian strata as the upper part of the Archæan, *i. e.*, as Huronian and upper Laurentian. Perhaps the time is not yet come to settle this question definitely.

6. He accepts as probable the existence in Quaternary times of a greatly elevated and enlarged Antarctic continent, connecting with and connecting together the southern parts of South America, South Africa and Australia similar to that of Permian times, as evidenced by the faunas, and as accounting for the Quaternary glaciation of these regions.

7. He agrees with Hilgard in thinking that the LaFayette formation (many geologists seem to forget that we owe this name to Hilgard) is a torrential *river* deposit of the early Quaternary and not a *marine* deposit of the Pliocene times as maintained by McGee, and that therefore it indicates elevation and not depression of the continent.

8. He does not accept Croll's theory of the cause of the glacial climate; but, along with most American geologists, regards it as mainly due to elevation of northern land. This would not only directly increase the cold in high latitude regions, but would indirectly increase the ice-accumulation by connecting America and Europe in these regions and thus limiting the northward extension of the Gulf Stream, which, circulating around the Atlantic in mid-latitude regions, would furnish abundant warm vapors to be condensed as snow on the elevated northern land.

9. As might have been expected, his discussion of mountain-making is masterly. But one is interested, though not surprised, to observe that he does not accept the recent theories of Reade, Dutten and others as to the cause of mountain formation, but still regards the *contraction-theory* in some form as more probable.

But a reviewer is 'nothing if not critical.' I must vindicate my character as reviewer by finding some faults, even though they be trifling.

10. This edition, we observe, drops out the graphic illustrations of the distribution, in time, of families, orders and classes of animals, which constitutes so conspicuous, and, we may add, so attractive a feature of previous editions. We observe also that the index of authors quoted and of those from whom figures are taken is omitted. This is to be regretted in a work which will be so constantly referred to.

11. We observe also a few errors of oversight or of misunderstanding of authors quoted. On page 359, and again on page 380, he gives, on King's authority, the whole thickness of Wahsatch sediments, from the Cambrian to the Laramie inclusive, as 31,000 feet. In fact, King gives between 31,000 and 32,000 for *the Palæozoic alone*, page 122; and in addition 3,800 feet for Jura-Trias, page 537, and 12,000 feet for the Cretaceous, page 539 (49th parallel, Vol. 1).

Again, he states on page 520 that the oldest known insect—*Protocimex*—is found in the

upper part of the *lower* Silurian; but on page 566 he says that the oldest known insect is the *Palæoblattina* of the *upper* Silurian.

We might mention others, but they are all trifling. In fact, the accuracy of the book is extraordinary.

In conclusion, we must heartily and most gratefully welcome the new edition. It is hard to say what American would be without Dana's Manual. Its encyclopedic fullness and yet extreme conciseness makes it hard reading for those who come to it without serious purpose. The word '*Manual*' exactly expresses its purposes and uses. It must be in the hands of every special student; it must lie on the table of every teacher of Geology to be consulted on every subject of doubt.

I had just finished this notice when the sad news of Dana's death was flashed across the continent. All recognized that this event could not be long delayed; but none the less it came as a shock to every man of science in the country. We are thankful that he lived to finish this new edition, for it is indeed the only fitting monument. No monument is worthy of a man of science except that which he erects for himself.

Joseph Le Conte.
University of California.

A Handbook of Systematic Botany. By Dr. E. Warming. Translated and edited by M. C. Potter. 8vo. pp. 620, fig. 610. London, Swan, Sonnenschein & Co. New York, Macmillan & Co. 1895.

This excellent English translation of Professor Warming's important work will be welcomed by all students and it cannot fail to have a wide use as a text-book. The descriptions of the groups are clear, concise and complete, the illustrations capital and many of them original, and the press-work leaves nothing to be desired.

The arrangement of groups is from simple to complex—the only arrangement compatible with our present knowledge. The special application of this principle may be best stated in Dr. Warming's own words as printed in the preface:

"Each form which, on comparative morphological considerations, is clearly less simple, or can be shown to have arisen by reduction or through abortion of another type having the same fundamental structure, or in which a further differentiation and division of labor is found, will be regarded as younger, and as far as possible, and so far as other considerations will admit, will be reviewed later than the 'simpler,' more complete or richer forms. For instance, to serve as an illustration: Epigyny and Perigyny are less simple than Hypogyny; the Epigynous *Sympetalæ*, *Choripetalæ*, *Monocotyledones* are, therefore, treated last; the *Hydrocharitaceæ* are considered last under the *Helobieæ*, etc. Zygomorphy is younger than Actinomorphy; the *Scitamineæ* and *Gynandræ*, therefore, follow after the *Lillifloræ*, the *Scrophulariaceæ* after the *Solanaceæ*, *Linaria* after *Verbascum*, etc. Forms with united leaves indicate younger types than those with free leaves; hence the *Sympetalæ* come after the *Choripetalæ*, the *Sileneæ* after the *Alsineæ*, the *Malvaceæ* after the *Sterculiaceæ* and *Tiliaceæ*, etc.

"Acyclic (spiral-leaved) flowers are older than cyclic (verticillate-leaved) with a definite number, comparing, of course, only those with the same fundamental structure. The *Veronica*-Type must be considered as younger, for example, than *Digitalis* and *Antirrhinum;* these again as younger than *Scrophularia; Verbascum*, on the contrary, is the least reduced, and, therefore, considered as the oldest form. Similarly the one-seeded, nut-fruited *Ranunculaceæ* are considered as a later type (with evident abortion) than the many-seeded, follicular forms of the order; the *Paronychieæ* and *Chenopodiaceæ* as reduced forms of the *Alsineæ* type; and the occurrence of few seeds in an ovary as generally arising through reduction of the many-seeded forms. The *Cyperaceæ* are regarded as a form derived from the *Juncaceæ* through reduction, and associated with this, as is so often the case, there is a complication of the inflorescence; the *Dipsacaceæ* are again regarded as a form proceeding from the *Valerianaceæ* by a similar reduction, and those in their turn as an off-shoot from the *Caprifoliaceæ*, etc. Of course these principles of systematic arrangement could only be applied very generally; for teaching purposes they have often required modification."

While there is wide difference of opinion among botanists as to the relative degree of complexity of some of the families, and the sequence adopted by Engler and Prantl in