covers only the current literature. The older publications should not be neglected, but should be cataloged in a series of monographs on special subjects.

The annotation of books by experts, advocated for many years with rare enthusiasm by Mr. George Iles, or at least the indexing and condensing of authoritative book reviews, is another work that would naturally come within the scope of a Central Cataloguing Bureau.

And finally, the bibliographical interests need an organ of their own where problems can be discussed and results made known.

It is clear that all these various undertakings, if carried out simultaneously, would result in a great deal of unnecessary duplication were there no central organization to guide and supervise the whole, and, if no provision were made for the utilization in many places of any title entry needed in several catalogues, without the necessity of setting up the matter anew for every time. As long ago as 1850, Professor Charles C. Jewett proposed that stereotype plates be made of the titles of all the books in American libraries, these to be kept at the Smithsonian Institution and to be utilized for the printing of catalogues of any library desiring it. The cost was too large then and the proposition too new. What was then looked upon as the visionary, though interesting, dream of an enthusiast, is now a reality, proved to be of economic value. The experience of the John Crerar Library with electrotype plates for title entries, used for printing of catalogues in book form as well as on cards, has been that the cost of making these plates and of their care and handling is less than that of printing the same matter over again from newly set type.

The purpose of these lines has been to call attention to the need of an Institute for Bibliographical Research where all the bibliographical and library interests of the country would center, and I hope that they may reach some one who might be able and willing to endow such an institute.

Aksel G. S. Josephson.
The John Crerar Library, Chicago,
Sept. 10, 1901.

DISCORD.

To the Editor of Science: Permit me to respond briefly to Mr. W. Le Conte Stevens's remarks on 'Discord and Psychology' in the issue of Science for September 20. (1) How Mr. Stevens found out that I had not read the investigations of Professor Mayer on this subject is an enigma to me. His intimation that they were unknown to me is based solely on the fact that I do not swear by the authority of these investigations, made 25 years ago (1875). I have known them for many years. But I know also many more recent investigations which do not agree with those of Mayer. Physicists who are interested in psychological theories and discuss them in their text-books may keep up with current literature by looking once a year at the 'Psychological Index,' published annually by the *Psychological Review*, or at any other psychological bibliography. (2) With Mr. Stevens's request to criticize from the psychological standpoint the sentences quoted by him, I shall gladly comply by quoting a few sentences from a physicist who was unusually familiar with psychological literature, namely the late Professor Melde, who says (Winkelmann's 'Handbuch der Physik,' I., p. 789): 'Eine Hauptstörung eines regelmässigen Tones oder eines consonirenden Zusammenklanges bilden die Stösse oder Schwebungen. Sie können durchs Zusammenwirken der primären Töne, also durch Interferenz entstehen, oder es können auch auf rein mechanische Weise solche Stösse erzeugt werden. Ihre Wirkung muss in beiden Fällen mehr oder weniger die einer Beimischung eines Rasselgeräusches sein. Zur Erklärung des inneren Wesens einer Con oder Dissonanz können aber nach des Verfassers Ueberzeugung auch nicht die Stösse (*viz.*, auch nicht Nebentöne) herangezogen werden, denn *eine sogenannte Dissonanz besteht sicherlich auch ohne jedes Verhandensein von Stössen.*" Let me further quote from Stumpf, 'Konsonanz und Dissonanz' (1898), p. 5, where the author discusses the very investigations of Professor Mayer which I am supposed to have ignored: "Wir können Intermissionen in *beliebiger Frequenz* auch bei zwei *konsonanten* Tönen künstlich herstellen, *ohne dass sie dissonant würden.*"

When there are beats, the psychologist speaks

of beats; when the beats are comparatively frequent, he speaks of 'roughness'; but the psychologist does not arbitrarily call roughness 'discord.' Upon the *cause* of discord the psychologists have *not* agreed; it is as yet unknown —at least to the psychologists.

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A CORRECTION.

IN SCIENCE for September 27, 1901, I called attention to a signature of a work entitled 'Florula Lexingtoniensis,' which I then supposed to be a work of C. S. Rafinesque. There is now no doubt that the signature in question is part of a work with the same title which appeared in the *Transylvania Journal of Medicine*, under the authorship of C. W. Short. The signature had been repaged, and does not have the appearance of a journal extract.

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CURRENT NOTES ON PHYSIOGRAPHY.

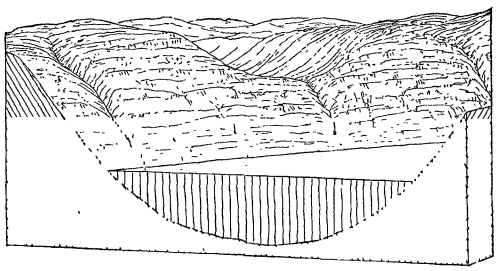
MT. KTAADN.

TWO visits to Mt. Ktaadn (5,150′) in northern Maine and four ascents have convinced Tarr that even the summit of the mountain has been glaciated, for fragments of schist, argillite and sandstone were found on its granite peaks ('Glaciation of Mt. Ktaadn, Maine,' *Bull. Geol. Soc. Amer.*, XI., 1900, pp. 433–448, 10 pl.). The greater part of the top is occupied by a 'tableland' surmounted by the several summits and gnawed into by huge basin headed valleys or corries, whose smooth and precipitous walls can hardly be scaled. Little talus lies in the basins, but a number of rock-basin lakes and terminal moraines were found on the valley floors. Where the basins come close together they are separated by sharp ridges, whose ruggedness Tarr accounts for by the moderate destructive action of the upper part of the ice sheet, as well as by postglacial weathering. He suggests that large local glaciers radiated from Ktaadn after the time of general glaciation.

Following the views of Richter, de Martonne and Matthes, recently noted in these columns, and the still earlier views of Johnson, the steep walls and sharp dividing ridges between the Ktaadn corries would be ascribed to the retrogressive erosion of their local glaciers, aided by the excessive frost action of the Bergschrund belt; and the 'tableland' would be regarded as a residual of a larger preglacial dome.

NORWEGIAN FIORDS.

THE year-book of the Norwegian geological survey for 1900 ('Norges geologiske Undersögelse,' No. 32, Aarbog for 1900, Kristiania, 1901, p. 263, many sketches and an English summary), contains an account of two important landslips in postglacial clays and a general discussion of the relief of certain typical areas. The highlands are regarded as presenting traces of two cycles of erosion; the older appears in the lofty snow-covered plateaus, more or less mountainous; the younger in the broad, open, high-level valleys among the high plateaus. The deeper valleys, whose deepest distal portions contain the fiords, are of later origin, after a great upheaval of the land, and are probably the work of water and ice in several interglacial and glacial epochs. Regarding the relative proportions of ice and water work, Reusch appeals to certain fiorded valleys, in whose walls a number of ravines have been produced by ordinary subaerial erosion. In such cases, the valleys must have been, Reusch thinks, worn nearly to their present depth before the ravines could have been formed. Hence "the glaciers enlarged the main valley and partly destroyed the side valleys, but they cannot be said to have made the main valley." But this conclusion leaves the problem in a quandary; for if the ravines indicate the preglacial depth of the main valley, it is difficult to understand why certain



hanging lateral valleys, whose streams are much larger than those in the ravines, were not also