months. This necessitates special legal provision for payments in the case of monthly contracts to be made proportional to the length of the month concerned. Moreover, it requires legal definition for the duration of a "month" from any given date. Thus we understand that a month beginning on any day of the last week of a long month (containing 35 days) will close on the last day of the following month. At least, this is the interpretation which, after careful thought, we have placed upon the following interesting example of parliamentary draughtsmanship:

"8. In calculating monthly periods the following rule shall apply: In any period beginning in a long month and ending in a short month, the last day of the short month shall be held to be the corresponding day to any of the days in the last week of the long month."

If this interpretation be correct, a month may mean any period from 28 to 35 days in length. Surely the clause comes perilously near to a *reductio ad absurdum* to the whole scheme. We can imagine the following simple problem: "A domestic servant is engaged on March 32 at £22 a year. What is the amount of the first monthly payment, and when will it fall due?" We are utterly at a loss to solve the question, and suggest it for the consideration of the framers of the Fixed Calendar Bill.

The fundamental feature common to both the bills alluded to is the use of the *dies non.* Mr. Alexander Philip, who was responsible for reviving the idea of this fiction and advocating its practical convenience, appears to have become impressed with the extent of the opposition likely to be encountered before it can be adopted. Accordingly, in a paper before the section of Economic Science and Statistics, read at the recent meeting of the British Association, and in a pamphlet with which we have been favored, he seems to have abandoned those who are seeking to give legislative form to his ideas, and to advance a totally different suggestion. This requires that February shall gain two days, that July and October shall each lose one day, and that the extra day in leap year shall be placed at the end of June. Then in each quarter (now containing three calendar months) a period of twelve weeks (always beginning on a Sunday) can be found, two such successive periods being separated by a week. The idea is that public engagements can be more conveniently fixed by reference to the proposed twelve-week period, while the correspondence between this reckoning and the ordinary calendar can be very simply exhibited by a "perpetual adjustable" arrangement. But this practically means that we should have two calendars side by side, and no further criticism seems to be necessary.

It is fairly evident that the group of people who are bent on introducing a change in our present calendar are not agreed as to the precise form which that change should take. In the meantime it is probable that public opinion in this country is not ripe for any reform. It would welcome a fixed Easter, but it is more than likely that any radical alteration of the calendar would be resented. Since the reformers adhere to the yearly divisions of the Gregorian system, no scientific question is involved at any point, and the public convenience and public feeling are alone concerned with the issue.—H. C. P. in *Nature.*

SCIENTIFIC BOOKS

Methode der Ethnologie. By F. Graebner. Kulturgeschichtliche Bibliothek, Herausgegeben von W. Foy. Serie I., Ethnologische Bibliothek, Vol. I., Heidelberg, Carl Winter, 1911.

Mr. Graebner is one of the serious and broad-minded students who are not satisfied with an accumulation of facts, but who are carrying through their own investigations according to a well-considered plan, and who try to contribute to science in a certain well-defined line of research and look for results that have a definite bearing upon the whole field of their inquiries. In the present book Mr. Graebner gives us a statement of the method that he is following and which will interest all ethnologists. If, however, Mr. Graebner calls his method *the* method of ethnology, we can not agree with him. He must not expect that all ethnologists will limit the field of their researches in the way set forth

in these "Methods." It appears from Mr. Foy's, the editor's, preface, that in this respect his own views and Graebner's coincide; in fact, in outlining the program of the whole series, Mr. Foy excludes expressly "alle geschichtsphilosophischen und völkerpsychologischen Betrachtungen" (p. v). This exclusion of the psychological field seems to me to give to the whole "Method" a mechanical character, and to be the essential cause of differences of opinion between the author and myself which I shall briefly characterize in the following pages.

The book is divided into three chapters: critique of sources, interpretation of data and combination of data. I do not quite share Mr. Graebner's unfavorable view in regard to the lack of critique of all writers on ethnological subjects, and in regard to the feeling that we are confronted by an appalling lack of all method; a feeling that, according to the author, the historian experiences who takes up the study of ethnology. It is true that much that has been written is based on inadequate evidence, and that particularly the so-called "comparative" ethnologists do not weigh their evidence well. Spencer, Frazer and Westermarck, not to mention others, have been criticized again and again by experts from this point of view. However, the whole modern method of ethnology, at least as developed in the United States, is a continuous struggle for gaining a critical view-point in regard to data collected by earlier authors who did not understand the objects and problems of modern anthropology. We believe that a safe interpretation of the older observed data must be based on careful archeological, ethnological and somatological field work. While I see a perfectly sound tendency in these studies, sounder than Mr. Graebner believes it to be, I still recognize the usefulness of the first chapter in which the author expresses the experiences of the historian in a form interesting and important to the unexperienced ethnologist. On the whole, the training given nowadays to students in universities and museums will impress upon them the safeguards on which the author insists, and which are too often forgotten by the amateur.

Our interest centers in the following two chapters: Interpretation and Combination of Data. The fundamental difference of opinion between the author and myself appears in the chapter on Interpretation. He defines interpretation as the determination of the purpose, meaning and significance of ethnic phenomena (p. 55); but he does not devote a single word to the question, how these are to be discovered. He accepts, without any attempt at a methodical investigation, myths as interpretations of celestial phenomena (pp. 56, 57), as, for instance, the Jona theme as signifying the temporary disappearance of a heavenly body; a conclusion which I for one am not by any means ready to accept. At this place the complete omission of all psychological considerations makes itself keenly felt. The significance of an ethnic phenomenon is not by any means identical with its distribution in space and time, and with its more or less regular associations with other ethnic phenomena. Its historical source may perhaps be determined by geographic-historical considerations, but its gradual development and ethnic significance in a psychological sense, as it occurs in each area, must be studied by means of psychological investigations in which the different interpretations and attitudes of the people themselves toward the phenomenon present the principal material. In the case of mythology, by means of which Mr. Graebner exemplifies his considerations, I should demand first of all an investigation of the question: why, and in how far are tales explanatory or related to ritualistic forms? The very existence of these questions and the possibility of approaching them has been entirely overlooked by the author. On the whole, he seems to assume that the psychological interpretation is self-evident in most cases, but that by migrations and by dissemination combinations may be brought about which may lead to misinterpretations in so far as several groups that were originally distinct may be considered as one by origin (p. 64).

Related to this disregard of the psychological problem is Mr. Graebner's claim, that no objective criteria have been found that can prove relations other than those due to historical connection; that the evolutionary investigation can do no more than answer the question: "How can I best and with the least number of contradictions imagine the course of human development in accordance with my general, fundamental views?" (p. 82). Against this method he claims that transfer has been proved to exist everywhere, while the presence of parallel development can not be proved by objective criteria (p. 107). I think, we must say, that certain types of changes due to internal forces have been observed everywhere, and that, therefore, the question of similar or dissimilar evolution through internal forces does not rest on a more hypothetical basis than changes due to transmission.

Another fundamental difference of opinion between Graebner and myself relates to the phenomenon of "convergence," and here again the conclusions reached by the author seem to me due to a narrow, mechanical definition of the term "convergence." He ascribes this idea to Thilenius and Ehrenreich. I may, perhaps, point out that I have raised the essential point in an essay "The Limitations of the Comparative Method of Anthropology,"[1] and again in my essay "The Mind of Primitive Man."[2] Graebner's first error in regard to this phenomenon is one which he shares with almost all other students of anthropogeography. I quote from p. 94: "Gleichartige Erscheinungen können auch durch Angleichung ursprünglich verschiedener Erscheinungen unter dem Einfluss gleicher Natur- oder Kulturumgebung zustande kommen. Da eine spezifisch gleiche Kulturumgebung ausser durch Kulturverwandtschaft aber ihrerseits nur als durch gleiche Naturumgebung hervorgerufen denkbar ist, bleibt diese allein als primäre Ursache von Konvergenzen übrig." This presupposes an existence of a mankind without any individual differences, or an absolute identity of the psychical conditions that are affected by geographical environment. As soon as the psychic basis is distinct, even the most absolute identity of environment can not be assumed to lead to the same result. It is a curious view that is so often held, that when we speak of the influence of environment upon the human mind, only the environment need be considered. Is not in every problem of interaction the character of each of the interacting phenomena of equal importance? In the particular case here discussed we may say that our whole experience does not exhibit a single case in which two distinct tribal groups are so much alike in their mental characteristics that, when they are subjected to the same modifying causes, these mental differences could be disregarded, and it is an entirely hypothetical and improbable assumption that in earlier periods absolute mental uniformity ever existed in distinct groups.

The idea that in cases of independent origin of the same cultural phenomena identity of environment can give the only satisfactory explanation is deeply rooted in Mr. Graebner's mind, for he repeats, on p. 112: "Gleiche Kulturbedingungen bei selbständiger Entstehung können ihrerseits wieder nur auf die Naturbedingungen zurückgehen."

The phenomenon of convergence is next considered as non-existent for two reasons: a theoretical one and an empirical one. The former is based on the consideration that convergence can occur only under identical cultural conditions, and that, therefore, heterogeneous cultural conditions such as are found in cultures not genetically related, can not possibly lead to the same result. The empirical argument is based on a consideration of conditions found in Europe (pp. 113–114). A consideration of the same data leads me to results diametrically opposed to those observed by Graebner. The very fact that in modern civilization a new idea is frequently discovered independently by several individuals seems to me a proof of parallel lines of thought; and Mr. Graebner's statement that

[1] Science, N. S., Vol. IV., pp. 901–8, 1896.

[2] *Journal of American Folk-Lore*, Vol. XIV., pp. 1–11, 1901.

the thought of only one man becomes socially active, *i. e.,* is adopted, seems to me to demonstrate just the reverse from what he claims. For an idea expressed at a time that is not ready for it remains barren of results; pronounced at a period when many think on similar, convergent lines, it is fruitful and may revolutionize human thought. May I point out that Graebner's own book may be taken as an example of this tendency? For it expresses the same fundamental idea that is so potent at present in all lines of biological research, that of the permanence of unit characters. An idea may become effective whenever the ethnic conditions are favorable to its adoption and development, no matter what the historical origin of the present general status may have been.

The questions of independent origin and convergence can not be entirely separated, and some of the previous remarks may perhaps rather relate to the probability of independent origin which Graebner practically denies. One aspect of the theory of convergence relates more specifically to the question whether two ethnic groups that are genetically distinct, which are confronted by the same problem, will solve it in a similar manner. The theory of convergence claims that similar ways *may* (not *must*) be found. This would be a truism, if there existed only one way of solving this problem, and convergence is obviously the more probable the fewer the possible solutions of the problem. This, however, is not what we ordinarily understand under convergence. Ethnic phenomena are, on the whole, exceedingly complex, and apparently similar ones may embrace quite distinct complexes of ideas and may be due to distinct causes. To take a definite example: Taboos may be arbitrarily forbidden actions; they may be actions that are not performed because they are not customary, or those that are not performed because associated with religious or other concepts. Thus a trail may be forbidden because the owner does not allow trespassing, or it may have a sacred character, or it may be feared. All ethnic units, separated from their cultural setting, are artificial units, and we always omit in our comparisons certain groups of distinctive characteristics—no matter whether the comparisons are made from the point of view of cultural transmission, or of evolutionary series. Thus, in our case, the forbidden action stands out clearly as a unit, that of the taboo, although its psychological sources are entirely distinct—and this is one of the essential features of convergence. Nobody claims that convergence means an absolute identity of phenomena derived from heterogeneous sources; but we think we have ample proof to show that the most diverse ethnic phenomena, when subject to similar psychical conditions, or when referring to similar activities, will give similar results (not equal results), which we group naturally under the same category when viewed not from a historical standpoint, but from that of psychology, technology or other similar standpoints. The problem of convergence lies in the correct interpretation of the significance of ethnic phenomena that are apparently identical, but in many respects distinct; and also in the tendency of distinct phenomena to become psychologically similar, due to the shifting of some of their concomitant elements—as when the reason for a taboo shifts from the ground of religious avoidance to that of mere custom.

In the foregoing remarks I have tried to show why Mr. Graebner's negative critique of parallelism and convergence does not seem to me conclusive. Just as little convincing appear to me the arguments on which he bases his method of determining cultural relationships. Here, also, the fundamental error seems to me based on the complete disregard of mental phenomena. Mr. Graebner lays down the following methodological principle: Two or more phenomena are comparable, and the one may be used to interpret the other, if it can be shown that they belong, if not to the same local cultural complex, at least to the same cultural group" (p. 64). It seems to me an entirely arbitrary hypothesis to assume *a priori* the homogeneity of similar phenomena belonging to the same cultural group. Mr. Graebner explains his standpoint by the example of the discussion of agricul-

tural rites in Frazer's "Golden Bough," and accepts the discussion on account of the homogeneity of the cultural groups of Europe and western Asia, from which the examples have been taken. This part of Frazer's deductions seems to me just as unmethodical as the others which are based on examples taken from a wider series of cultural groups. The concepts of comparability and homogeneity, as I understand them, have to deal not only with historical relationship, but to a much higher degree with psychological similarity, for only as elements of the mental makeup of society do ideas or actions become potent and determining elements of further development. To give an instance of what I mean: If the aged are killed by one people for economic reasons, by another to insure them a happy future life, then the two customs are not comparable, even if they should have their origin in the same historical sources. Graebner's idea appears clearly in the following statement: "If in different parts of the earth peoples are found that are closely related in their ways of thinking and feeling, evidently the same question arises, that has been treated before in regard to cultural forms, viz., whether these similarities are not based on community of descent or on early cultural contact" (p. 112). Such a view can be maintained only if we disregard the action of inner forces, that may lead two people of like cultural possessions after their separation to entirely distinct conditions. In short it is based on the view of a very limited action of internal forces.

Through the restriction of comparability and interpretation exclusively to the phenomena of transmission and original unity—a definition that I do not find given, but that is everywhere implied—and by the hypothesis, that ethnic phenomena that occur in two areas due to transmission or to original unity will always remain comparable and can be mutually interpreted, the author is necessarily led to his conclusions, which are merely a restatement of his incomplete definitions and of his hypothesis; for, if we call comparable exclusively phenomena that are historically related, naturally then there can be no other kind of comparability, and psychological ethnology does not exist.

Exactly the same criticism must be made against the sense in which the term "causal connection" is used. Here also the psychological connections are intentionally excluded, because the psychological argument, its method and validity, are not congenial to the author; and "causal connection" is simply identified with historical connection. On this basis only can I understand the statement that in literary tradition causal relations are directly given (p. 73). This is not meant to refer to modern historical science, but to the literary sources of Asia and Europe. Is not literary tradition on the whole proof of the misunderstanding of causal relations, rather than the reverse—provided we understand under causal nexus not the simple mechanical aspect of transmission, but the complex social conditions that admit transmission and that bring about internal changes.

A correlate of the assumption that ethnic elements that are genetically related remain always comparable plays a most important part in Mr. Graebner's method of proving cultural relations: "Whenever a phenomenon appears as an inorganic element in its ethnic surroundings, its presence is due to transmission." This might be true if primitive cultures were homogeneous units; which, however, is not the case. The more we learn of primitive culture, the clearer it becomes that not only is the participation of each individual in the culture of his tribe of an individual character, or determined by the social grouping of the tribe, but that also in the same mind the most heterogeneous complexes of habits, thoughts and actions may lie side by side, without ever coming into conflict. The opinion expressed by Mr. Graebner seems to me so little true, that I rather incline toward the reverse opinion. It seems at least plausible, although it has never been proved, that on the whole only such ethnic features are transmitted that in some way conform to the character of some feature of the life of the people that adopt them. The criterion in question seems to me, therefore, not accept-

able, until it can be sustained by observed facts.

This idea is probably related to the author's conception of the transmission of cultural elements in the form of complexes. He says: "A migration of single cultural elements, also of tales, over wide distances, without the spread of other cultural possessions at the same time, may be designated without hesitation as a 'Kulturgeschichtliches Nonsens'" (p. 116). I should like to see the proof of this daring proposition. It is, of course, not the question whether one cultural group owes much or little to another one, but whether cultural elements are necessarily transmitted in groups. To take only a few examples. Is not the gradual introduction of cultivated plants and domesticated animals a case in kind? Does not the irregular distribution of tales show that they are carried from tribe to tribe without relation to other transmissions? It seems to me that the more the problem of cultural contact is studied, the more amazing becomes the independence of far-reaching influences in one respect, from the spread of other cultural possessions. The example of language used by Mr. Graebner (p. 111) presents facts entirely different from those which he imagines. Thus we find phonetic influences without corresponding lexical or morphological influences and *vice versa*. The serious defect of the "Method" is here clearly seen. Instead of operating with the purely mechanical concepts of transmission and conservatism relating to the most ancient types of culture, we must investigate the innumerable cases of transmission that happen under our very eyes and try to understand how transmission is brought about and what are the conditions that favor the grouping of certain new elements of an older culture.

I think I have shown that not only the psychological and evolutionary standpoint contain hypothetical elements that must be subject to a rigid criticism, but that the restriction of all ethnic happenings to mechanical transmission or preservation contains many hypotheses the validity of which is open to most serious doubt. Mr. Graebner has failed in his attempt, because he does not apply the same rigorous standard to his own favorite views, that he applies so successfully to a discussion of the evolutionary theory (pp. 77 et seq.). Here he is at his best, and his criticism of the many hypothetical assumptions contained in all theories of the evolution of culture are well taken and should be read and minded by all students of ethnology. In a few cases, particularly in the discussion of correlated ethnic phenomena, he does not seem to do quite justice to the force of the argument, because he prefers a spacial interpretation of these correlations to a sequential one; but both are certainly equally possible and probable.

It is, however, curious to note that, notwithstanding his uncompromising negative position, the author tacitly re-introduces some of the most fundamental concepts of cultural evolution. Thus he speaks on p. 63 of the "well-known tendency of degeneration and disintegration, according to which myths become legends and fairy-tales, significant institutions formal traits"; and again on p. 152: "Undoubtedly sound points of view are, that the beginnings of every phenomenon must be simple and in a way naturally grow, and that the development must be intelligible by the most simple psychological process." My criticism of these assumptions would be much more far reaching than that of Mr. Graebner.

Thus it seems to me that the methods of Mr. Graebner are subject to the same strictures as those of the other schools, and the "Ferninterpretation," "Kulturkreise" and "Kulturschichten" must be considered as no less hypothetical than the "Stufenbau" of Breysig or the sequences of Lamprecht.

In the development of science it is, however, useful to carry through a hypothesis to its limits and to investigate the ultimate conclusions to which it will lead. From this point of view pages 104–151, in which the principle of conservatism and transmission are strained to the utmost with an absolute disregard of all other possibilities, will be helpful for a gradual clearing of our views. Perhaps even more helpful is the actual application that Mr.

Graebner has made of these principles in his chosen field of Melanesia in its relations to the whole rest of the world.

My own opinions in regard to the value of a single evolutionary series, the importance of very old cultural elements that survive in many parts of the world, and the occurrence of transmission over enormous areas coincide to a great extent with those of Mr. Graebner. I also hold the opinion that the discovery of a really new idea is much more difficult than is generally admitted, and therefore a manifold spontaneous origin quite unlikely. Nevertheless, I can not acknowledge that he has given us *any* safe criterion that would enable us to tell that in any given case transmission can be definitely proved against independent origin, and I am just as skeptical as before reading his book in regard to the advisability of accepting Ratzel's "Ferninterpretation." I rather repeat once more the warning that I have given again and again for twenty years: to be rather overcautious in admitting transmission as the cause of analogies in cases of the sporadic occurrence of similar phenomena, than to operate with the concept of lost links of a chain of cultural intercourse.

That through the exaggerated application of a single principle, when several must be admitted as acting, new viewpoints may be discovered—that much I willingly admit, and I enjoy to follow the daring generalizations to which Mr. Graebner is led. I may, however, be pardoned if I can not accept this as the method of ethnology. I see safe progress essentially in the patient unravelling of the mental processes that may be observed among primitive and civilized peoples, and that express the actual conditions under which cultural forms develop. When we begin to know these, we shall also be able to proceed gradually to the more difficult problems of the cultural relations between isolated areas that exhibit peculiar similarities.

Franz Boas

México, D. F.

Phytogeographic Survey of North America. By John W. Harshberger. Being Vol. XIII. of Engler and Drude's "Die Vegetation der Erde." Leipzig, Wilhelm Engelmann. 8vo. Pp. 790, with 1 map, 18 plates and 32 text figures. 1911. 52 M.

The series of monographs issued by Professors Engler and Drude under the title of "Die Vegetation der Erde" reaches the thirteenth number in the stately volume before us. Among preceding volumes are Radde's "Pflanzenverbreitung in der Kaukasusländer," Drude's "Hercynische Florenbezirk," Diels's "Pflanzenwelt von West-Australien," Engler's "Pflanzenwelt Afrikas," etc., all of which have been received with favor by botanists the world over and this prejudices us in favor of this one from the hand of Professor Harshberger.

Unlike the preceding this volume is given in English, which indeed was quite proper in view of its American authorship, and the additional fact that it will be much more available to ordinary students and readers. And it may be said here that I know of no book on scientific botany which is more likely to be read by non-botanical readers than this one. As one reads it he is constantly impressed with the importance to a great number of men of just such knowledge as is brought out here. One wishes it were possible to give as clear pictures to the intelligent layman as are here given to the systematic botanist.

The plan of the work may be stated as follows:

After an English explanatory preface by the author, and a short German summary by Dr. Drude, the book is divided into four great parts, the first of which (92 pp.) is historical and bibliographical. This is followed by Part Second (of 77 pp.) which is devoted to geographic, climatic and statistical considerations. Part Third takes up (in 175 pp.) the geological evolution of the North American continent and its flora, while Part Four (of 358 pp.), which is the body of the book, takes up the phytogeographic regions, formations and associations. The whole is followed by a voluminous Index of Plants (of 85 pp.) which includes helpful synonyms.