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ART. XXIII.—*The Diversity of the Glacial Period*; by
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IN the November number of this Journal, there appeared an article entitled "The Unity of the Glacial Epoch," by Prof. G. Frederick Wright, that seems to call for a rejoinder, partly to set right the personal views of some whose positions are opposed, and partly to state with a greater approximation to correctness the leading facts bearing on the question and the necessary inferences from them. There are many questions relating to the problem that are legitimate subjects of difference of opinion. Were the article confined to these, it would be foreign to my habit to reply to it. Even as it is, if this were the first or the second or the third discussion by the author that has failed to correctly represent personal views and scientific determinations, I should doubt whether I were justified in writing an article that must necessarily embrace a large controversial factor.

The article is not of the nature of a constructive advocacy of the unity of the Glacial epoch, as its title seems to imply, but an attempt at destructive criticism of the evidences advanced in support of the duality or diversity of the Glacial period, and an attack upon the individual positions of some of the advocates of these views. The insufficiency of all arguments advanced for duality or diversity, even if demonstrated, would, without positive arguments in proof of unity, only put the question back where it was two decades ago. It would make it simply a matter of doubt, with only conservatism on the side of simplicity. That is an effort of doubtful utility at

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the present time. The advocates of unity should present direct evidences of indivisibility and of the persistence of like conditions throughout the period if they are to merit attention. Destructive arguments have their place in such discussions, but they have an inferior value, unless they are attendants of affirmative arguments.

Prof. Wright's introductory statement of the question seems to me to need radical reconstruction to be even approximately representative of the present attitude of glacialists. If we set aside the views of those who hold glacio-natant theories of the origin of the drift, in whole or in part, there will remain at least four classes of views, with an ulterior fifth class. These may be designated, (1) the primitive views of unity, (2) the later views of unity, (3) the several views of duality, (4) the several views of plurality or diversity, and (5) ulterior synthetic views based on exhaustive analysis.

1. *Primitive views of unity.*—The old views of unity recognize but a single comparatively short ice invasion (modified by oscillations of the margin), involving but one stage of land elevation followed by one stage of depression, the Champlain. Local glaciation was held to be an incident of the retreat, and an elevation was thought to accompany the succeeding Terrace epoch. The stages of elevation and depression were quite generally held to have a genetic relationship to glaciation, but that was not universal, nor is it essential to the classification. It was the view of some that local glaciation accompanied the elevation of the Terrace epoch, thus constituting a species of second glaciation, but this is immaterial so far as the interpretation of the great body of drift in the United States is concerned.

2. *Later views of unity.*—The later views of unity depart somewhat radically from the old ones in postulating a long depression, or series of depressions, in the earlier stages of glaciation (perhaps preceded by a stage of elevation), followed by a prolonged stage or series of stages of elevation followed again by a stage of depression, this last being the Champlain depression in the strict sense of the term. This view differs from the old doctrine of unity in the important and very necessary feature of recognizing at least one early stage of depression, of which several separate episodes of glaciation have already been determined. It also parts company with the old view in entertaining a radically different conception of the extent and complexity of the period and of the importance of its constituent episodes. The working methods of those who hold this view are radically different from those of the old school, as well as their habits of interpretation, in that they are analytical and discriminative in respect to structural,

genetic and historical factors. In these respects, this view does not very essentially differ from the following views. It only diverges from them in recognizing the whole complex series of glacial movements as connected and bound together across the episodes of retreat and advance by conditions that are to be interpreted as signifying continuity rather than discontinuity.

3. *Duality.*—The several views of duality differ in detail, but they agree in recognizing one, and only one, epoch of deglaciation of such nature and importance as to justify a division of the period into epochs by means of it. Each of the two epochs is held to have been marked by episodes of ice advance and retreat and other changes, but these are thought to be subordinate to a bipartite division. The separation of the epochs of lake-formation in the Great Basin region by an interval of aridity, the separation of the epochs of glaciation of the interior by an interval of extensive and high-gradient erosion and weathering, and the intervention of a temperate fauna and flora between glaciated deposits in Europe, are typical illustrations of the class of data to which these views appeal. It is not essential to prove or to hold that the ice completely left the continent, any more than it is necessary to prove or to hold that the sea retreated from the land between the recognized epochs of the Niagara period. The question is not a question of two glacial *periods* but of two glacial *epochs*, as these terms have come to be commonly used in America and England. This does not make the distinction merely a choice of terms. It is a distinction of ideas, and of not unimportant ideas. The dualist holds that he has grounds for an important subdivision of glacial history comparable in significance to that which divides the periods preceding. There is, it is true, an element of judgment here, as in all similar cases, but back of that there is an essential distinctness of ideas.

4. *Plurality or diversity.*—Some of the most experienced glacialists of Europe and of this country who have held to the last class of views, have come to feel their inadequacy in the interpretation of the vast growing mass of evidence, and have enlarged their views in the direction of still greater diversity. These have come either to recognize three or more glacial epochs, or to feel that the diversity is so great as to make a simple bi-partite division unsatisfactory. One of the latest and most notable expressions of this class of views is to be found in a recent paper by Dr. James Geikie, "On the Glacial Succession in Europe" (Trans. Roy. Soc. Edinburgh, May 16th, 1892) in which five glacial epochs are recognized, and a map of the distribution of the ice in the second and fourth of these epochs is given.

1. Doubtless nearly or quite all the older students of drift, of the glacial school, once entertained views that should be placed under the first class. Such views linger with some whose later attention has been turned in other directions chiefly, and with some whose studies have been confined to areas presenting only one time-phase of the drift conspicuously, and some hold these views for reasons less felicitously explained. I should class Professor Wright's views in the primitive group. He is doubtless in advance of many phases of the older views and his papers show some progress, but he has failed to definitely recognize and accept two stages of depression separated by an important stage of elevation. This is best illustrated by his position respecting the formations of the Delaware region, which will be noted further on. His impression of the extent and complexity of the ice age is of the earlier rather than of the later order. He does indeed use terms that by themselves would signify great extension and complexity, but he employs arguments and interpretations which show that they carry a significance very different from that which is given them by those who take the newer and enlarged views of unity. For instance, he objects to the reference of the erosion of the lower gorge of the Allegheny and Upper Ohio Rivers to an interglacial epoch on the ground of the length of time required by the erosion, and speaks of such views as "making unnecessary demands on the forces of nature." (*Man and the Glacial Epoch*, p. 218.) His descriptions of the Ice age in his two books and elsewhere seem to me to convey an archaic and bedwarfed impression both of the extent and complexity of the period. I cannot, therefore, think that his views of the glacial period have any such extension as would entitle them to be classified under the newer views of unity. The most serious phase of the matter is that neither analytical methods nor their results seem to find even an approximately accurate reading, much less exposition and discussion, at his hands.

2. The later views of unity seem to have few, if any, declared advocates, due, doubtless, to the fact that as soon as a glacialist comes to realize the nature of the phenomena involved to an extent sufficient to cause him to dismiss the primary views, he quickly passes on into the third or fourth class, or else takes an attitude of conservatism and reserve, and awaits further evidence before engaging in a definite advocacy. These views are, however, entitled to a distinct place and to full recognition, because they not only represent the attitude, whether transient or otherwise, of a considerable body of glacial students, but they are still kept among the working hypotheses of many who employ the method of multiple working

hypotheses. That method requires that all hypotheses not absolutely excluded by evidence should be retained in the working group. If any doctrine that can properly be designated a doctrine of unity shall ultimately prevail, it will be one that will be developed along the lines of effort which characterize this class and distinguish it from the first class.

So far as the attitude of those who entertain this class of views concerns analytical methods of working, it stands, so far as it is typical, in sharp antagonism to that of the advocates of the first class, because the ultimate establishment of their views depends upon the clear discrimination of the glacial deposits that were formed in the apparent intervals appealed to by the advocates of the third and fourth classes and upon the demonstration that such episodes of glaciation occupied these intervals. This involves not only the clear discrimination of the intervals but the determination of the special deposits that bridge them. This demands the utmost resources and highest refinements of analytical and discriminative methods. Success will not lie in ignoring or belittling the extension of glacial time or the greatness of the intervals now discovered; but, if attained at all, it will be through ability to fill in the gaps with undiscovered deposits and to bridge over the unquestionable changes of surface attitude with gradational stages and concurrent glaciation, and to demonstrate that the variations in the character of glacial action took place by such gradual steps as to bind the whole into inseparable unity. This requires an attitude toward discriminative work precisely opposite that assumed in Professor Wright's discussion of the subject.

3. It is difficult to name, and moreover unprofitable to attempt to name, the glacialists who at the present hour advocate the doctrine of duality as distinguished from plurality or diversity, since so many of those who some time ago felt compelled by evidence to believe in at least one interglacial epoch, but who were not convinced that it was necessary to recognize more than one of the major type, have in recent years passed on to, or at least toward, the recognition of two or more such intervals under the impress of the accumulation of evidence. No one can have followed conscientiously the onward movement of opinion during the last few years and have failed to note the strong drift from simple duality toward either a strongly diversified duality or toward plurality. In the midst of this movement, it is idle, as well as harmful, to attempt to define the positions of individual glacialists in so far as they have not recently defined them for themselves.

4. What has just been said applies to the lower limit of the fourth class, but there are some whose declared opinions make

this a distinct and important group. The very able paper by Dr. James Geikie, already referred to, is a notable expression of advanced opinion of this kind. Incidentally it indicates a similar attitude on the part of some other European glacialists of large and varied experience and of unquestioned ability, and some American glacialists have indicated at least a hospitable attitude to similar views. A comparison of the actual position of Dr. Geikie with that assigned him by Professor Wright, and a comparison of the array of evidence which Dr. Geikie advances with the treatment it received in the article in the November number of this Journal is one of several illustrations of the ground of my protest.

5. There is an ulterior synthetic view to be based on a previous exhaustive analysis to which every cautious student of the subject looks forward as the ultimate interpretation. This is an ulterior view because it is impossible to take such a view at the present time, except in a crude prophetic sense. The method may now be clearly seen but the data for its realization are not at hand. The first step toward it is a thoroughgoing analysis of the glacial complex into its constituent deposits and the definite delineation of these both in plot and in section involving the tracing out of the connections and the correlations of the constituent sheets, the determination of the intervals that occur at different horizons and in different sections of the country (for they are not the same everywhere), and the evaluation of the nature and length of the intervals and their climatic, orographic and other characteristics. Then will come the final test of the unity views, in the demonstration, or the failure to demonstrate, that episodes of glaciation fill in the intervals and bind the whole into indivisible unity. Then will come, also, the final test of the doctrines of duality or diversity in the establishment, or non-establishment, of intervals which prolonged research has failed to bridge, and which temperate faunas and floras show to be necessarily interglacial intervals, unless there be brought into the series the distant polar connections which are presumed to exist in any case. It is in the interest of each of the classes, unless it be the archaic one, to press on analytical and discriminative studies and to give to them and to their results full recognition.

Immediately following his general statement of the question, Professor Wright remarks, "In approaching the subject, it is important to notice the fact that Professor Chamberlin inaugurated his induction as director of the glacial division of the United States Survey by publishing a monograph of "The Terminal Moraine of the Second Glacial Epoch" [correct title "Preliminary Paper on the Terminal Moraine of the Second

Glacial Epoch], thus assuming the truth of his theory in the title." I am puzzled to see how the title "The Terminal Moraine of the Second Glacial Epoch" any more assumes the truth of a theory than the title "The Unity of the Glacial Epoch." If the purpose of the sentence was to convey the impression that I commenced my consideration of the subject by assuming the truth of the duality theory, it is precisely antipodal to the fact. This paper was my seventh discussion of the moraine or some part or phase of it. The first of these was prepared while I entertained primitive and inherited views closely similar to those which Professor Wright now advocates. These views I gradually abandoned as my information increased and my series of papers show a progressive change of opinion. It was only at the close of my official work on the Wisconsin Survey, when called upon to sum up and interpret finally the results reached, that I definitely announced an abandonment of the old view and an acceptance of the dual view, assigning reasons therefor.* If anything relative to the history of my personal views is of any importance in the matter (which is not my assumption), it is this statement of change of view at the close of several years of consecutive study, a statement which had more of a final than of an inaugural character.

The effort of Professor Wright to lay the groundwork of presumption that my whole interpretation of the facts bearing on the duality of the epoch is possibly a mistake by reason of a change in my mapping in Illinois would have been without force had he made a fair statement of the case. He states, "in this preliminary monograph (see pp. 322-326) the moraine is made to correspond with the kettle moraine of Wisconsin, and to hug the southern shore of Lake Michigan, but in the Seventh Annual Report of the U. S. G. Survey the later glacial drift is carried down to Bloomington more than one hundred miles farther south, while at the latest date Mr. Leverett (*Am. Geol.*, July, 1892, p. 23) specially deputed by Professor Chamberlin to look after the moraines, draws his later moraine line one hundred miles still farther south, through Litchfield, Hillsboro," etc. (p. 353.) There is here a complete omission of all reference to the provisional lines which were marked with dots on the map in my earlier paper, and concerning which the following was said in the text:†

"There may be no more fitting place to make a qualifying remark in regard to the whole region between the moraine above traced and that adjacent to Lake Michigan. The drift of this area bears undoubted evidence of being recent, and, though this is in considerable part due, superficially, to aqueous

* *Wis. Geol. Sur.*, vol. i, pp. 271, 272.

† *Third Ann. Rep. U. S. Geol. Sur.*, 1883, p. 331.

agencies, it seems to me probable that the region will prove to have been largely, possibly completely, covered by ice in the earliest stage of the second glacial epoch. It is not, however, traversed by conspicuous moraines, at least not by any as well developed as those above outlined. Low ridged belts of subdued morainic aspect have been observed at numerous points, but their relations have not yet been traced out.

"A similar qualifying remark may be here made concerning a considerable area in Northern Illinois, outside the moraine described in this paper. The freshness of its drift, and the unsculptured contour of its surface, bear evidence of recent origin. Some portions of this area seem clearly to be of lacustrine and fluvial origin, at least superficially, and I have at times supposed that all might be due to waters marginal to the adjacent glacier, since there is no conspicuous bordering morainic ridge; but the tendency of recent evidence, gathered in a special study of this class of deposits, seems to favor the hypothesis of more extensive glacial occupancy, even where the evidence of it in obvious moraines is feeble or wanting. This questionable region is now under investigation. The dotted lines on the map indicate some of my working hypotheses."

Prof. Wright neglects to say that my later mapping relates to this area, thus announced to be under investigation, and that its morainic lines were such as the preliminary map had foreshadowed. He further failed to say that this later map was one constructed merely to exhibit the location and direction of striae in connection with my paper on "Rock Scorings of the Great Ice Invasions."* I have not yet re-discussed the region nor the moraines in question. The more recent investigations bring out into clearer definition and justification the ground of doubt which lay in my mind at the time of the first mapping, for they show that there are two groups of moraines representing two important episodes of glacial history, and that both mappings will be retained, when corrected and perfected, as factors in an ultimate differential map of the drift of the region. To demonstrate their exact correlations east and west still remains difficult, because the later lines override the earlier at large angles and conceal their connections, and because the moraines of both epochs bunch themselves in the reëntrant angles between the ice lobes in such a way as to make their demonstrative disentanglement a work of extreme difficulty. On this account I have left the provisional correlations set forth in the earlier paper standing without re-discussion, that the work of later correlation might be the freer.

* 7th Ann. Rep. U. S. Geological Survey, pp. 147-248.

Having made it appear by such omissions that I had changed my mapping to the amount of a hundred miles, Professor Wright states that Mr. Leverett draws "his later moraine line" one hundred miles still farther south. If by this expression it is meant that Mr. Leverett referred this moraine line to my second glacial epoch, I think a reading of Mr. Leverett's language will fail to show warrant for it. Mr. Leverett distinctly recognized two groups of moraines coincident with two orographic attitudes. He did not discuss the relations of either of them to my correlations. If it is meant simply that this moraine was announced "later" than the others, it has no pertinence, unless it is assumed that any moraine of any sort is to be referred to my later epoch, which is not Mr. Leverett's view nor mine. As to still more southerly moraines, Mr. Leverett and myself are quite aware of the hilly belt at Greenville (whose course it may be remarked in passing makes a high angle with the drift border) and also of other more or less definable but obscure tracts of thickened drift of a terminal or quasi-terminal kind. It is a part of our hope and expectation to be able at length to trace out obscure sub-marginal thickenings of the drift sheets, and other quasi-marginal markings of one kind or another, over the whole drift field, old as well as new, where the more pronounced terminal moraines are absent. This has formed a feature of my plans for many years and considerable preliminary data have been gathered and some detailed tracing has been done in pursuit of it, in connection with work on the more important and significant moraines which mark advances of the ice and define glacial episodes if not epochs. Some of these moraines of minor significance have been mapped, and others are yet to be added. Some of them will lie not far from the border of the drift at some points, and it is to be hoped that the more obscure older drift will yield marginal indices in all districts sufficient to show its history with approximate accuracy. With this in view, I have developed new criteria of discrimination and have proposed additional distinctions between marginal moraines and have sought other marginal and submarginal markings suited to the requirements of this more critical work. Of course a map showing all these moraines and marginal lines without indicating their differences, gives a more or less anti-divisional impression, much as does the diagrammatic geological column of the text-books and dictionaries, and of course it is possible to lump these all together as "moraines of recession," or "receding moraines," just as all the sedimentary strata south of the Archæan terrane may be disposed of as deposits of sea-recession. These moraines are moraines of ice-retreat in much the same sense as the Paleozoic sediments are

beds of sea-retreat. There was a general retreat in both cases, but there were advances in both cases, and the recognition of these constitutes the essence of the science in both cases.

Besides the implications involved in the attempt to make the older moraines—even undiscovered ones—do duty as correlates of what others regard as obviously much younger ones of different character, there is a specific erroneous impression conveyed by the statement that closer observation is not unlikely to carry the exterior moraine farther south still which would be “all that is necessary to make the extreme boundary stand related to the moraine there as it does throughout the eastern part of the Mississippi Valley.” This carries the implication that there is a constant relation between the extreme boundary and the outer terminal moraine in the eastern part of the Mississippi Valley. This is not the case. The fresh stout moraine that lies near the border in the upper Ohio region departs from it in going westward and disposes itself in a strongly looped fashion that is not coincident with that of the border. It is precisely because of this lack of coincidence and because of the marked differences in the character of the drifts that a distinction between them is urged. In Illinois there is nothing within a hundred miles of the border that presents any rational grounds for correlation with the outer terminal moraine in Western Pennsylvania. Professor Wright claims to have spent five seasons on the border between Pennsylvania and the Mississippi River,* and yet he has not found in Western Ohio, Indiana and Illinois any such moraine-and-“fringe” in any such relationship as he has so often described in Western Pennsylvania. One would think that five seasons, work would give a better suggestion of what is likely to be found true respecting a moraine-and-“fringe” border, than the one offered to the effect that Mr. Leverett or some one else is not unlikely yet to find a moraine of some sort somewhere near the border. In Bulletin 58 U. S. Geol. Survey (p. 75) Professor Wright says, “In Illinois nothing like a moraine was encountered in any portion of the State which I traversed, which included Randolph, Perry, Franklin, Jackson, Williamson, Saline, Gallatin and White counties”

Mr. Leverett recognizes the following stages applicable to the region between its limit of drift in southern Illinois and the the head of Lake Michigan.

1st stage. Sheet of drift averaging perhaps 20 feet in thickness. Intervals of slight or partial deglaciation shown by occasional presence of soil between sheets of till.

2nd stage. Interval of deglaciation of great length. Surface of old drift sheet deeply oxidized, leached and much eroded. Thick and widespread soil.

* The Dial, Dec. 16, 1892, p. 380 ; Jan. 1, 1893, p. 7.

3d stage. Deposition of main body of loess and associated silts. Apparently very low altitude and slack drainage.

4th stage. Long interval of deglaciation. Large valleys cut in loess.

5th stage. Formation of thick drift sheet with morainic border 75 to 100 feet deep. Ice reached Shelbyville (not Litchfield) at maximum advance. Later sub-stages of glaciation marked by moraines or thickenings of drift at the margin of the sheets (40 to 50 feet). Intervals between successive moraines probably short since evidences of oxidization, leaching, erosion and soil accumulations are scant. Elevation about as at present when Shelbyville moraine was formed. Drainage less vigorous when later moraines of this stage were formed.

6th stage. Interval during which ice lobes and ice currents were shifted. Length probably considerable, though decisive evidence is wanting.

7th stage. Latest series of ice lobes south of Lake Michigan formed. Elevations greater, drainage freer. Minor moraines marking later sub-stages. Followed by the lake deposits of the region.

Mr. Leverett is inclined to correlate the outer moraine in Eastern Ohio and Northwestern Pennsylvania (there called the terminal moraine in a double sense by some writers), with the outer member of this last group, which is about 240 miles from the extreme limit of drift in Illinois, but owing to the fact that the moraines in the intervening region became bunched together in the angles between the ice lobes, an entirely demonstrative correlation by tracing is difficult, and he withholds a final judgment. But leaving the doubt as wide open as the phenomena will admit, he regards a correlation of the outer moraine of Western Pennsylvania with anything within 120 miles of the southern limit in Illinois as opposed to all evidence.

In his elaborate monograph on "The Pleistocene History of Northeastern Iowa,"* Mr. McGee defines and illustrates, with great fullness, the following series:

1. A lower till ranging in thickness from a mere veneering to 200 feet, averaging perhaps 50 feet, characterized by greenstone erratics (in contrast with the granitic erratics which characterize the upper till); nowhere displaying a moraine-like peripheral thickening, but everywhere attenuated toward its margin. General facies ancient, ferrugination and oxidation greater than that of upper till.

2. A forest bed found in 40 per cent of the well-sections, 20 to 25 per cent of them showing a definite bed. Old soil

* 11th Annual Report U. S. Geological Survey.

thicker than that of to-day. Pine, oak, elm, sumac, walnut, willow, ash, hickory, and tamarac recognized.

3. An upper till, ranging in thickness from a veneering to probably 100 feet, averaging perhaps 20 feet. Erratics generally granitic. Grades horizontally at the south into loam, and vertically either grades upward into lœss, or is overlain with a certain unconformity by lœss. Only rarely and locally do terminal moraines mark the margin.

4. A lœss mantle, which in part grades into the upper till, and in part overlies it with a definite dividing plane between. Regarded as a deposit contemporaneous and continuous with the upper till.

These formations are older than the moraine-bordered tongue of till which occupies the north central part of the State and they pass under it. They are separated from it by great general erosion, oxidation, leaching, etc.

Back of this lie the complicated moraine-bordered deposits of Minnesota. We have here, therefore, a series comparable to that on the east side of the Mississippi. The upper till of Mr. McGee was deposited at a time of slack drainage. The moraine-bordered tongue of Central Iowa was deposited at a higher gradient, as shown by the freer drainage from its edge.

It thus appears that the latest studies of these two experienced investigators, the fullness and detail of whose work in this region is unsurpassed, bring into clearer and sharper definition than ever before, the distinction between these drift sheets, and magnify the importance of the intervals.

While the question of one or more epochs is not necessarily dependent upon that of the origin of glaciation, there is so intimate a connection between them, that whatever bears upon the one, is not without application to the other. If the great intervals of deglaciation and the great advances of the ice were due to astronomical causes of the type advocated by Croll, every great change, being due to a reversal of the action of the controlling agency, is entitled to distinct recognition. If the chief cause of glaciation is northern elevation, any evidence which shows a depression far to the northward carries with it as a logical conclusion the assumption of general deglaciation. Prolonged glacial action at low altitudes puts the theory to a severe strain. Halts and advances at low altitudes impose still greater difficulties, the more severe the greater the interval. Even if it is granted that just after the ice reached its maximum extension, a depression took place because of the loading of the ice and that low gradient deposits attended the retreat of the ice, it is evident that there could be no considerable readvance without a restoration of high altitude. For instance, the ice could not rationally be presumed to retreat to

the northward and remain long enough to permit the accumulation of a soil equal to that of the present and then advance again while the country still lay at a low altitude. It certainly could not be supposed that the ice would retreat so that low-gradient deposits should take place six or seven degrees of latitude back from the edge of the ice without general deglaciation also taking place. Now, in the Mississippi Valley we have silt deposits spreading over many thousands of square miles and connecting themselves with the second main drift sheet of the series given above. They overlies the oldest drift throughout large areas but are separated from it by a soil horizon. (The older drift seems to have its own silt-appendices in some regions also.) These silts occur on the highlands adjoining the main rivers throughout the older glaciated regions from Ohio to Nebraska, and they reach northward to the western part of Wisconsin, the southeastern part of Minnesota, the southwestern part of Minnesota, the southeastern part of Dakota and the northern part of Nebraska, and from these tracts they extend without detectable interruption to the gulf region. In other words, they reach seven degrees north of the extreme drift limit in the Mississippi region and six degrees in the Missouri region. To account for the phenomena in harmony with the hypothesis of glaciation by elevation, it appears to be necessary to suppose that the northern region was elevated to a height sufficient to cause an ice sheet to creep down to a point south of the latitude of 38 degrees on a plain of great extent; that this was followed by a depression sufficient to cause the withdrawal of the ice for an unknown distance and to remain withdrawn long enough for a soil and for peat and forest beds to accumulate to thicknesses fully comparable to those of the present; that there was then an advance sufficient to form the second till which overlies this forest bed, followed by a retreat under such conditions as to permit silts to gather over the wide areas indicated for six or seven degrees northward and at considerable heights above the present streams, spreading well back on the uplands. Now a depression that would permit a deposit of these silts throughout this great extent and at these heights, carries with it the presumption of the removal of the ice from the whole northern region, because the depression is assumed by the hypothesis to have been caused by the ice itself, and to have been progressively greater to the north, and so, unless an entirely inconsistent supposition be interpolated, the northern region must be presumed to have been lower than it is to-day. The great extent of silts, therefore, in the Mississippi region carries with it the presumption of complete deglaciation of the northern region, if elevation be assumed to be the chief cause

of the Ice age. This, it seems to me, holds good under any form of the aqueous hypothesis of these silt formations that is consistent with the phenomena. If they are attributed to wind action, the aridity and the combination of conditions necessary for the accumulation of the silts make the presumption of disappearance of the ice equally great.

Subsequent to this period, on the theory of elevation, there must have been another uplift at the north sufficient to cause the ice to readvance to points as far south as the first advance in certain regions, and within 300 to 500 miles of the first advance in others.

Now an elevation sufficient to produce glaciation as far south as 38° , followed by a depression sufficient to permit silts to accumulate 7° north of this, followed by another elevation sufficiently great to cause ice to advance to like degrees of latitude, in the main, seems to me a sufficient change in the great agency of the time and a sufficient orographic movement to justify the distinction of separate epochs. So far as I can see, nothing less than these extraordinary oscillations are sufficient to explain the phenomena, and to these must be added minor oscillations of very considerable moment. For myself the phenomena of low altitude deposition seem so great and so completely demonstrable as to be fatal to the hypothesis. But if not, the very multiplication of overlapping sheets and marginal moraines signifying halts and advances, that is appealed to in the endeavor to weaken the evidence of two or more epochs, bears in precisely the opposite direction when the demonstrable conditions of such halts and advances are duly taken into consideration.

In the matter of the "fringe," Professor Wright's statement, "The fact that the oldest part of the glaciated region 'is not bordered by a definite terminal moraine, but ends in an attenuated border' is only another way of stating the fact which Lewis and I began to urge upon the attention of the public ten years ago during the early part of our investigations," is unwarranted and tends to confusion historically and scientifically. The chief questions regarding these phenomena were squarely and broadly before scientific readers before Professors Lewis and Wright urged their single view of one variety of the phenomena. But that is a trivial matter. On the contrary, the confounding of the phenomena designated and interpreted as a "fringe" with the phenomena of the attenuated border is a serious source of error. The phenomena are not only not equivalents, but they stand in some measure as antitheses. There are four classes of facts to be distinguished.

1. A drift sheet bordered by a terminal moraine attended by scattered drift on its outside formed at the same time as the moraine by minor incidental action. This scattered drift may not inaptly be called a "fringe." It is a trivial phenomenon, a mere incident of the main action.

2. A drift sheet that, in contrast with the above, thins out gradually to an attenuated edge. A specific example of this, described in detail, may be found in the Sixth Annual Rep. U. S. Geol. Surv., p. 265. There may be scattered incidental drift on the border of this as well as of the moraine.

3. A bordering tract of scattered boulders, sometimes reaching out many miles beyond any continuous drift. The origin of this is not certainly known, but it is probably due to temporary bordering lakes or glacial floods. It is well developed at some points in Dakota and Montana.

4. A bordering tract of scattered pebbles attended by silt. This is probably due to glacial waters acting at low gradients in conjunction with detached ice, perhaps local, perhaps glacier derived. An example of this is described in detail and discussed in the paper above cited (p. 271.)

To confound all these and to merge them under a single term is to push science backward. It is especially unfortunate to create confusion respecting the vital distinction between a moraine-bordered sheet with incidental drift scattered outside, and a thin-edged sheet that may be attended by similar scattered drift. The scattered bordering drift is trivial in significance in either case. This is especially so when the terminal action is strong and vigorous, expressing itself in a definite moraine and when the outer drainage is free. The attenuated edge, as distinguished from the moraine-thickened edge, has a radical significance, because it expresses a vital distinction in mode of action. If the term "fringe" had been confined to the first class of phenomena, and the phenomena discriminated with reasonable accuracy, its introduction might prove a convenience. But the attempt to make it a synonym with the "attenuated border" which happens to be the form the old drift sheets assume, and the failure to distinguish the two phenomena has made its introduction most unfortunate and renders the propriety of its further use very questionable. The more important phenomena outside the Pennsylvania terminal moraine to which the term "fringe" has been applied I think has been demonstrated to be the border of an older drift sheet, and not at all a "fringe" of the terminal moraine with which it has no genetic connection.

Turning to the phenomena of the Delaware Valley, it will assist to a clear understanding of the question if it be recalled that in the original mapping of the drift of the region by the

geologists of New Jersey and Pennsylvania, the terminal moraine which crosses the river at Belvidere, and which may be spoken of conveniently as the Belvidere moraine, was represented as the boundary of the glacial drift. This was not rigidly affirmed by the geologists of New Jersey, and from personal statements made to me by Professor Cook at the Paris International Geological Congress in 1878, soon after the appearance of his report on that region, and from recent statements of Professor Smock, it appears that a knowledge of some drift outside of the Belvidere moraine was possessed by them, and that the moraine was regarded as representing only the approximate border. Professors Lewis and Wright insisted much more rigidly upon the limital character of the moraine;* and Professor Lewis published a paper subsequent to his final report on the terminal moraine, in which he attempted to show that all the supposed drift outside of the moraine in Pennsylvania except obvious valley wash was illusive.† In recent years several geologists, notably Professor Salisbury, have observed drift on the uplands, extending to a distance of at least 25 miles south of the moraine. Rare occurrences of glacialized boulders have been found fully 60 miles south of the moraine. Professor Salisbury refers a portion of this extra-morainic drift extending in places at least 12 miles beyond the moraine, to direct glacial action at a time much antecedent to the formation of the Belvidere moraine. Respecting other and generally more distant deposits he has reserved opinion.

It is fair to those geologists who in an early day overlooked this outer drift, or failed to see its full meaning, to note that in its character and expression it is markedly different from the drift of the moraine and from that within it, and that this is ample excuse for the oversight at that stage of work. But even then it did not escape Professor Smock, although we are not aware that he has anywhere published statements concerning it. The oversight is less pardonable if the truth be, as insisted by Professor Wright, that this outer drift is of the same age and kind as that of the terminal moraine.

Notwithstanding his own oversight, Professor Wright insists that those who saw at once the essential facts upon their first visits failed to see what ordinary competency required. In particular, he asserts that Professor Salisbury is in error in referring to a glacial origin the deposits at High Bridge and Pattenburg, which lie about 12 miles distant from the nearest point of the moraine. He claims to be able to demonstrate this. One essential part of his demonstration is the assertion

* Second Geol. Sur. Penn., Rep. Z, p. 45.

† On supposed Glaciation in Pennsylvania South of the Terminal Moraine. This Journal, Oct. 1884, pp. 276-285.

that "There is no foreign material in the cuts, at least, Professor A. A. Wright and I could not find any"; (p. 307) and a second is that drift from the Medina sandstone is absent from these localities, and that its absence is decisive.

1. The absence of any single lithological element is not demonstrative of the absence of drift of glacial origin. Only a short distance to the north, the whole group of crystalline erratics is reported by Professor White to be absent from a large part of Susquehanna and Wayne counties.* No one questions the glaciation of that region because of their absence. Most experienced glacialists I think must have encountered similar instances of the local absence of a particular rock constituent. Such apparent absences over considerable areas have been encountered by Mr. Buell in a specially careful tracing of boulder trains from the isolated crystalline knobs of Wisconsin, and for a time such absence seemed to indicate a limitation of that particular kind of drift; but further search has shown that beyond this barren area the particular erratics reappear and extend onward for considerable distances.

2. It so happens that Mr. Kummel of the New Jersey geological survey, without a knowledge that the localities were the subjects, or were likely to be the subjects of special question, made collections of rock specimens at both High Bridge and Pattenburg, simply with a view to rendering them as complete and representative as possible. Upon request, these collections have been sent to Chicago and examined, and Professor Smock has also kindly given his opinion of them. Referring to a group of specimens belonging to the collection, Professor Smock expresses the opinion that "they belong to or have come from the ledges of Cambrian, Green Pond Mountain and Potsdam, in New Jersey and New York. Of course it would be possible to duplicate the lot in the Green Pond Mountain range in the Kittatinny Mountain series, and in the Potsdam." These all lie at distant points. Among the specimens are pebbles of sandstone so closely resembling the Medina sandstone that only an expert familiar with the special character of the formation in New Jersey could give an opinion of any value at all. Professor Smock, after a careful examination, does not feel justified in giving a positive opinion. In the presence of these specimens, the alleged absence stands upon a very slender basis.

The collections contain specimens of gneiss, sandstone of at least two distinct types, quartzite, shale of two kinds, and chert. These appear to represent certainly five different

* *Sec. Geol. Sur. Penn.*, G 5, 1880, p. 26.

formations. This petrographic variety, taken in connection with Prof. Smock's opinion, seems to be a sufficient answer to the claim that there is no foreign material in the cuts.

As a collateral point, Professor Wright asserts that "South of Musconetcong Mountain stretch the Triassic red shales which cover so much of the central part of New Jersey. But in this portion of the State, there has been absolutely no transportation of northern material out upon the Triassic shales, showing that no movement here ever passed the Musconetcong Mountain" (pp. 365-6). Drift lying upon the Triassic shales is reported to occur at the following points by the following observers :

1. West and N.W. of Pittstown.	A. R. Whitson.
2. Hensfoot.	H. B. Kummel.
3. Pattenburg.	R. D. Salisbury, C. E. Peet and H. B. Kummel.
4. South of White House Station.	A. R. Whitson.
5. North ($1\frac{1}{2}$ or 2 miles) of Somerville.	F. C. Schrader and R. D. Salisbury.
6. New Brunswick.	C. E. Peet, R. D. Salisbury.
7. Liberty Corners.	F. C. Schrader, R. D. Salisbury.
8. Berkley Heights.	A. R. Whitson, R. D. Salisbury.
9. New Vernon.	R. D. Salisbury.
10. Bernardsville.	F. C. Schrader, R. D. Salisbury.
11. Basking Ridge.	R. D. Salisbury, F. C. Schrader.

It is not affirmed that all these are direct glacier deposits, but they are identified as drift and other than obvious valley drift. Of these, the Pattenburg and Hensfoot deposits and the area near Pittstown lie south of Musconetcong Mountain and fall directly within the territory alleged to be barren. The remaining localities lie to the east of this, and while they do not come within the strict limits of Professor Wright's statement they bear on the general question involved. Of these localities, all, or nearly all, lie in such topographical situations as to absolutely exclude the interpretation placed upon the High Bridge and Pattenburg deposits by Professor Wright.

It appears, therefore, that he has based his assumed demonstration, first, upon a general proposition that experience has shown to be entirely inadmissible; secondly, upon the absence of foreign material at High Bridge and Pattenburg when the specimens gathered by a young geologist sent there merely to make a representative collection embrace a large variety of rock specimens unrepresented by formations in the vicinity and referred by Professor Smock to distant terranes; and thirdly, upon an assertion, in the most positive terms, of the absence of drift on the Triassic areas south of Musconetcong Mountain, when such drift is found there by other observers.

Turning now to the interpretation which Professor Wright puts upon the High Bridge and Pattenburg deposits, it is to be noted first, that the one occurs upon a spur as shown by the New Jersey topographical maps and that to reach higher ground a line running along the back of the spur must be followed, rather than one down the maximum slope in the most natural line of slide and creep. No height exceeding 460 feet above the deposit in question occurs in the neighborhood, except such as are separated from it by depression; and to reach even this moderate height, it is necessary to go one mile back from the locality. In the other case, heights 500 feet above the area in question occur within two miles to the north, but they are separated by gentle slopes and a shallow valley. The slope on which the deposit lies is composed of Triassic rock. These facts bear upon the suggestion of Professor Wright that these deposits are due to degradation, slide and creep. Bearing more specifically upon that interpretation is the fact that in these deposits there are boulders and pebbles of rock not now in the adjacent ridges, and these are polished and scratched in a manner precisely similar in every respect to the well-known polishings and scratchings of glacial pebbles. One or two in the collection of Mr. Kummel take rank among the best examples of typical glaciation; and if they were passed through the hands of a hundred glacialists of the greatest experience, not one could be expected to hesitate for a moment to refer them to glacial action. Nothing at all closely simulating them has ever been reported as the demonstrable work of either land slide or creep. Furthermore, the deposits in question are in part distinctly stratified, a feature foreign to the products of landslide and creep. The deposit at High Bridge is about 30 feet in maximum thickness, and that at Pattenburg scarcely less.

To refer to creep or landslide deposits having these considerable thicknesses, containing derivatives from five formations, embracing nine recognizably different varieties of rock, a part of the deposits being stratified, and containing in both localities scratched stones, the glacial origin of which no glacialist would ever independently question, is most extraordinary in what claims to be a demonstration of an illusion on the part of one of our best trained and most critical observers.

As another proof of alleged error on the part of Professor Salisbury, Professor Wright says, p. 364: "The extensive oxidation spoken of by Professor Salisbury in the quotation made from his recent reports on the glacial deposits of New Jersey is clearly of preglacial origin." That extensive oxidation affected the old surface material that became a part of the old drift, here as elsewhere, goes without saying. In a profes-

sional discussion of the character of the drift, it is as much presumed that this factor has been eliminated as, in the discussion of an astronomical question, it is presumed that the error of refraction has been eliminated. To discourse upon the existence of such previously oxidized material, in a serious paper written for professional glacialists, at this date, has an archaic flavor. To presume that Professor Salisbury, who has done some of the most critical work that has ever been done upon residuary products and their contributions to the older drift, overlooked this factor in the study of the Delaware region, is a reflection upon the critic, rather than upon the work he criticised. I have discussed with Professor Salisbury upon the ground the pre-glacial factor in the sum total of oxidation presented. To presume that a geologist cannot distinguish whether the aging of a pebble took place before or after it became a pebble, is to suppose that this department of geology is yet in a primitive condition. To assume that there are no criteria for distinguishing pre-glacial oxidation from post-depositional oxidation and to fail to see the applicability of those criteria to the region under discussion, and make use of them, is to condemn the whole work of whomsoever makes the assumption; for the evidence of post-depositional oxidation, weathering, degradation and aging in its several forms, is so perfectly clear and so completely demonstrative, that it should not fail to impress itself upon any observer who has even a moderate command of the discriminations which such a study necessarily involves. If any critical student will examine the terminal moraine and the drift north of it, and the gravel train that leads from the moraine's outer edge down through the gorge of the Delaware, on the one side, and will examine the higher terraces and their fluvial deposits and the drift scattered over the highlands, on the other, he will find clear and abundant evidence that the one is relatively young and fresh, and that the other is markedly old and that the two deposits cannot, by any rational interpretation, be made contemporaneous.

That Professor Wright is in radical error, respecting the deposits of the Delaware Valley, is evident from the self-contradictory nature of his own interpretations. He refers the slack drainage deposits along the Delaware (Philadelphia Brick-clays, McGee's Columbia) to the Champlain depression.* But he admits, what is incontestable, that after this epoch of slack drainage deposition there was an elevation during which were deposited the Trenton gravels, which he refers to a "time when the ice had melted far back towards the head

* See this Journal, Nov. No., pp. 358, 366, 370, 371 and 372. See also, "Man and the Glacial Epoch," pp. 254-261; also, "Ice Age of N. A.," pp. 522-527.

waters of the Delaware, and after the land had nearly resumed its present relations of level, if indeed, it had not risen northward to a still greater height.”* Now it is incontestable that it was after the ice had retired from the head waters of the Delaware, that the depression occurred during which the marine deposits of Montreal and the Champlain region were laid down. Professor Wright also states that between the deposit of the Philadelphia brick-clay and that of the Trenton gravel, there was a “long interval.”† We, therefore, have Professor Wright referring to the Champlain depression deposits that were laid down long anterior to the Trenton gravels, which, in turn, were deposited at a time of elevation which, in its turn, was demonstrably early than the Champlain deposits. To refer the slackwater deposits of the Delaware and the marine deposits of the Champlain Valley to the same depression, and yet put between them a “long interval” and a period of elevation, is a characteristic instance of the self-destructive interpretations to which the old doctrine of unity naturally invites.

Besides the evident error of confounding two depressions, separated from each other by the acknowledged interval occupied in the erosion of the gorge of the Delaware and by the intervention of an elevation comparable to the present, there is a specific error in Professor Wright's correlations. He makes the well-known gravel terrace at Trenton the correlative of a terrace of less than half its height at Yardville, a few miles above, overlooking the fact that terraces of the same nature and gradually rising altitudes occur at short intervals all the way from Trenton to the Belvidere moraine. There they join the moraine in the characteristic fashion of moraine-headed terraces with which students of the high-gradient terminal moraines have become familiar. That this connection is clear and demonstrative is the judgment of at least five geologists who have studied the formations.

It is altogether probable that a complete analysis of the drift formations of the Delaware will develop some additional factors, and possibly extend its history, but the following features I think may be accepted as demonstrative:

1. That there was an earlier invasion of the ice which reached at least a dozen miles south of the Belvidere moraine. For present purposes it does not matter at all whether it included the High Bridge and Pattenburg deposits or not, nor whether the more distant drift is glacial or glacio-natant.

2. That there is an older fluvial deposit (the Philadelphia brick-clay) which is likewise much older than the Belvidere

* “Man and the Glacial Period,” p. 261.

† “Man and the Glacial Period,” p. 257.

moraine and which for the present is to be associated in age with the old glacial drift, though the two may not prove, upon further examination, to be strictly contemporaneous.

3. That after the formation of this older river deposit, which took place at a low altitude and low gradient, there was an epoch of elevation and of erosion, during which the Delaware cut its channel down to the depth of 200 to 300 feet below the upper old terrace.

4. That there followed a second incursion of ice which formed the Belvidere moraine, the over-wash of which ran down into this previously formed gorge and filled it up to a height of about 100 feet in the immediate vicinity of the moraine, and to progressively less and less heights farther down stream until at Trenton the height had declined to about 40 feet.

If Professor Wright were correct in his correlation of the terrace at Trenton, it would not affect, in any essential degree, the history here given, because the other terraces within the gorge above, some of them in the immediate vicinity of the moraine and in immediate connection with it, constitute the essential part of the evidence. I do not think that any geologist, at all expert in these lines, can entertain any doubt as to contemporaneity of these terraces with the moraine, or that the gorge was formed antecedent to the moraine, or that the higher terraces, capped with the old gravels and clays, were formed earlier than the gorge; and I am surprised that anyone professing familiarity with drift phenomena, should question the markedly superior age of the old drift.

The phenomena on the Delaware, therefore, taken in connection with those of the St. Lawrence and Champlain Valleys, seem to admit of no rational interpretation that does not involve two depressions and an intermediate stage of elevation. The duration of the elevation has as its minimum measure the cutting of the river gorge and the high-gradient glaciation beginning with the Belvidere moraine and embracing several subsequent episodes of undetermined length. I conceive that there may be differences of judgment as to how much divisional value such orographic stages and such alternations of action and such evidences of aging may be entitled to; but that here are tangible divisions of the glacial history of the region that are of fundamental importance in the interpretation of the deposits and in the determination of the glacial relations of supposed art relics, does not seem to me to admit of question.

The phenomena on the Susquehanna are, so far as known, very closely similar to those upon the Delaware, but they have been much less fully worked out and may be passed over.

The phenomena upon the Allegheny, Upper Ohio and adjacent rivers, seem to me to be in precise harmony with those of the Delaware. Before discussing them, however, I need perhaps to say a word respecting Professor Wright's hypothesis regarding the effects of a supposed glacial dam at Cincinnati which, in his view, destroys the force of the data upon which I postulate an interval of deep river erosion between the earlier and later glacial invasions.

In the first place it should be noted that, although nearly a decade has passed since the hypothesis was advanced, no outlet for the great hypothetical lake has been found, though it has been called for as the necessary credential of such an hypothesis. Such an outlet must, in the nature of the case, be a marked phenomenon.

It is to be noted, in the second place, that the decisive facts brought out by Mr. Leverett with reference to the white silts which spread over the drift north of the Ohio throughout the whole reach of the supposed dam, destroy one of the chief arguments for the hypothetical lake above the supposed ice dam, and this holds true when the dam is given the greatest extension assigned it by Professor Wright. It will be borne in mind that Professor Wright appeals to the silts of Beech Flats as evidence of a lake caused by the dam. Mr. Leverett has shown that these are but a part of an extensive sheet stretching westward over the very area occupied by the ice supposed to have formed the dam, reaching even to and beyond its western border, covering wide areas in southwestern Ohio, northern Kentucky and southeastern Indiana. Mr. Leverett's language is clear and covers the whole ground: it is as follows:

"Not only are the clays of these two localities [Beech Flats, Pike county and the till area, Highland county, Ohio] "similar in macroscopic and microscopic aspect, but they form a practically continuous sheet extending from the Beech Flats and adjoining lowlands outside Wright's glacial boundary westward onto the glaciated districts of southwestern Ohio, northern Kentucky and southeastern Indiana, occupying the site of the hypothetical Cincinnati ice-dam and showing as strong development below (west of) the site of the supposed dam as they do above it. The fact that these clays cover a part of the glaciated district proves that their deposition occurred subsequent to the time of maximum glaciation, and their distribution shows that the ice-sheet nowhere reached the Ohio river while they were being deposited. It is evident, therefore, that their deposition cannot be attributed to an ice-dam on the Ohio at Cincinnati or any point below."*

* *Am. Geol.*, vol. x, July, 1892, p. 21.

Prof. Wright strives to elude the force of this by the following statement :

"Mr. Leverett in particular has attempted to correlate some of the clay and loess deposits in southeastern Indiana, with deposits of similar character in Illinois, attributing both to the earlier Glacial period during its slackened drainage. But he does not seem to have duly considered the facts which I have presented making probable an obstruction of the channel of the Ohio near Madison, Indiana, in Jefferson and Ripley counties which might well account for the facts in that part of the State most like those in southern Ohio. (See Bull. U. S. Geol. Surv., 58, pp. 65, 66.) Something more than similar microscopical results must be relied on to demonstrate chronological identity of deposits."*

In Prof. Wright's own language, "A theory driven to such extremities cannot be said to be altogether free from difficulty."†

Prof. Wright thus explains the discovery of these Beech Flat silts : "So confident have I become in the reality of this dam that I have not hesitated to use it as a means of putting myself in the line of discovering other facts which are the natural consequence of this. Many of the facts enumerated in this paper (as, for example, those connected with the headwaters of Brush Creek) were thus discovered. It was reasoned that they must exist from the nature of the supposition ; and upon examination in proper localities it was found that they did exist according to previous calculation. I need not say that such experience is the most convincing proof of a theory."‡

The succession of deposits even in this region on which such large conclusions have been staked, and which I therefore touch incidentally, is very significant. There is here (1) a till sheet with an attenuated edge reaching across the Ohio into Kentucky for a few miles, (2) an interval of deglaciation indicated by a soil horizon at the surface of this sheet, (3) a silt deposit overlying this and indicating a period of slack or still-water deposition, and (4) a till sheet edged by a terminal moraine, from the outer side of which there was free vigorous drainage, as shown by moraine-headed terraces of gravel. At two localities these silts have been seen beneath this later drift, and there is much indirect confirmatory evidence of this relation. These facts indicate an interval between the two during which occurred the change from conditions of silt

* This Journal, Nov., 1892, pp. 369-370.

† Ibid., p. 371. Where this sentence is used by Prof. Wright it has no pertinency whatever, as Mr. Leverett's suggested hypothesis to account for the unusual depths of the rock channels near the border of the drift stands wholly by itself.

‡ Bulletin 58, U. S. Geol. Surv., p. 101.

deposition over the country generally to conditions of free drainage pouring down through valleys cut to considerable depths below the silt horizon (Leverett).

In this region, therefore, between the border of the drift and the outermost moraine there is decisive evidence of a till-depositing period, an interval of soil formation, a silt-depositing period, an interval of radical drainage-change and a moraine-producing advance of the ice. This is a very different collocation of formations from that which seems to be implied by Prof. Wright's language (pp. 353-356 and 357). It is very far from being a simple moraine-and-"fringe" combination.

In Eastern Ohio and Western Pennsylvania the outer moraine runs much nearer the attenuated border of the old drift and the intervening silts are obscure or absent, or confined to the remnants of the old base levels, so that it is not so surprising that the vanishing edge of the old drift should here be mistaken for a dependency of the outer terminal moraine, but that the relationship is really the same as it is demonstrably to the westward, I hold to now be beyond serious question.

These points bear upon the explanation of the glacial gravels that lie upon the high terraces in the Allegheny and adjacent valleys. These terraces I have maintained (Bulletin 58, U. S. Geol. Surv., pp. 20-38) were produced at a time of base level degradation, which in its later stages was contemporaneous with the earlier ice incursion whose waters bore gravels down the Allegheny, Upper Ohio and some adjacent streams, and formed the 40 or 50 feet of capping which lies upon the rock benches that constitute the body of the terraces. I have argued that subsequent to this the land was elevated and the lower newer steep-sided gorges of the Allegheny and neighboring streams were cut to a depth that may be roundly stated as 250 feet, and that subsequent to this the later ice incursion formed the outer moraine of the region. From the outer side of this moraine glacial streams bore their sands and gravels down the Allegheny gorge cut during the interglacial interval. The evidence of this, gathered in a joint study by Mr. Gilbert and myself, may be found in Bulletin 58, U. S. Geol. Surv., pp. 32-36. I therefore argue that between the time when the glacial gravels were deposited on the high terraces and the incursion of the later ice there was a cutting of the gorge to the depth of 250 feet roundly speaking. I regard this gorge-cutting as a *minimum* measure of the interval between the two ice incursions. Prof. Wright does not admit the force of this evidence because he interprets the occurrence of the glacial gravels on the high terraces as the work of ice floating from the edge of the glacier on the surface of the hypothetical lake formed by the supposed Cincinnati ice dam.

He now admits, what I have not understood him to do before, that the terraces, so far as they are rock, are due to base-leveling, but that the glacial gravels deposited upon them are due to floating ice. To the latter part of this view, there seem to me to be two fatal objections. In the first place, I think it is entirely foreign to observation, as well as to *a priori* considerations, to suppose that floating ice would produce, in the abandoned bends of old rivers surrounded by hills, and upon remnant shelves along the sides of the valley, fine stratified gravels bearing every aspect of river deposits. Under the conditions postulated a very different class of deposits should be formed. Besides, the deposit should have been practically uniform over the whole bottom of the supposed lake, and not simply confined to the old terraces. In the second place, these terraces rise in altitude toward the headwaters of the river until some of them are considerably higher than the supposed ice dam, notably those at Warren, Tidioute and Oil City, which are respectively 1395, 1390 and 1270 feet above tide, while the height assigned for the dam is 1000 or 1100 feet. (See Bulletin 58, U. S. Geol. Surv., p. 27.) Aside from these definite and tangible factors, the whole aspect, association and relationship of these formations make them river deposits and exclude them from the distinctive lacustrine class.

In admitting to his hypothesis the factor of base-leveling, Prof. Wright has relinquished the last remnant of occasion for postulating his ice dam, for all of the phenomena appealed to are the natural incidents of base-leveling. Stretches of slack water, which are inevitable when a river reaches the base-level stage and begins to build up its bed, furnish deposits indistinguishable from those of lakes; indeed lakes form in the abandoned channels of the stream. So also base-leveling and the resilience from it involve the transfer of the position of low divides which lie near the upper limit of the base plain, the transfer being from the side of the shorter or easier course toward the longer or more obstructed course. The transferred divide is carried into the bottoms of the stream whose territory is invaded, so that its former flood plain deposits become the new divide; and hence the phenomena of slack water deposits on some of the present divides become, under the hypothesis of base-leveling, precisely what is to be expected. These may occur at altitudes greater than the base plain of the main river. These and coördinate methods of action cover all the phenomena for the explanation of which the supposed ice dam was brought into requisition.

Being left thus without a *raison d'être*, being robbed of all support from the Beech Flat silts, and being entirely without

the necessary support of a recognizable outlet, the hypothetical lake may well be dismissed from the literature of the subject. The whole phenomena fall into perfect consonance with the phenomena of the Delaware and Susquehanna.

As one of the strong, and as it seems to me, unanswerable arguments in favor of a considerable interval between an earlier and a later drift formation, I appeal to the cutting of the Delaware gorge 200 feet or more, which was demonstrably later than the high terraces bearing glacial silts and gravels and demonstrably earlier than the Belvidere moraine from which a gravel stream was poured down into the gorge cut during the interglacial interval. I make a like appeal to a similar class of phenomena on the Susquehanna, and I repeat the appeal in respect to the precisely analogous phenomena of the Allegheny and other tributaries of the Upper Ohio, and the upper part of the river itself. I make a similar appeal to the erosion of the lower Mississippi valley and several of its branches, the erosion here however sustaining a different relation to the old erosion plane. Doubtless the appeal could be made to all branches if the import of the phenomena were equally clear in all cases, or had been equally studied. The form which the valley erosion took and the material eroded varied with the antecedent and concurrent conditions which were not the same in all valleys, nor the same in all parts of the same valley, but a correspondent erosion occurs on all branches that have been carefully studied so far as I am informed. All this class of phenomena repeating itself over and over from the Atlantic to the western plains, carries a force from which I think there will be found no escape when the phenomena are critically and judicially investigated.

But the case does not rest upon the interglacial cutting of these river channels alone. It is supported by concurrent erosion over the whole surface of the old drift. If there were any error in the interpretation of the river channels it would be shown in the facies of the old drift. But its topography shows an amount and kind of erosion that indicates similar antiquity. The phenomena are so wide spread that special enumeration is wholly impracticable. In a paper on the Driftless Area, Prof. Salisbury and myself attempted a careful description of a part of these erosion features and drew a comparison between them and those of the new drift. There, it is to be noted, the two borders were subject to the same conditions save that of age.

One of the best fields for a comparison between the fresh little-eroded topography of the later drift and the much-eroded topography of the earlier drift, is found on the borders of the tongue of the later drift which terminates at the capital of

Iowa. The surface within the moraine loop has suffered very slight erosion, except in the vicinity of the streams. Even the channelings of these are sharp-sided and ditch-like. Over the surface shallow sags, without outlet, occur in extraordinary abundance. They are to be numbered rather by tens of thousands than by thousands. On the old drift outside of the loop an erosion surface of pronounced type is presented. A complete erosion topography has been developed and has sunk itself deeply below the original surface. To appreciate this contrast let any one make a circuit of one hundred miles north from Des Moines upon the later drift, and he will find, throughout, indisputable evidence of freshness and recency. Let him then make a similar circuit of one hundred miles west, south and east on the area of the old drift and he will find a rolling surface with undulations reaching up to 80 feet and more of perfectly characteristic dendritic erosion topography, and upon examination of the stratified portions of the drift he will readily be convinced, if a topographic geologist, that this erosion supervened upon the formation of the drift. On the summits of this rolling land he will find the loess-like silts that cover so much of the earlier drift. Modern topographic science goes for naught, if evidences of this kind do not signify a prolonged interval between the deposit out of which such an erosion surface has been carved, and the immediately adjacent deposit upon which so little carving has been done.

To this combination of general erosion and of channel cutting, there are to be added evidences of like import from oxidation, ferrugination, decomposition and other characteristic forms of weathering. These, in the hands of an expert competent to eliminate preglacial and englacial factors, bear testimony of great value. The value is certainly almost wholly dependent upon the circumspection, skill and conscientiousness with which the criteria are applied, but the value is none the less real. When these evidences are found, as in this case, concurrent with general surface erosion and with the special erosion of river gorges, the combination has a value much beyond either factor alone. When to this is added the fact that the drift sheets most eroded and most oxidized, are those which were deposited at low-gradients and without erosion advantages at the start, and that in general, they lie in regions that are now lower than the less eroded drift of later date, the case is still further strengthened. When, to the combination, there is also added the fact that the deposition of this low-gradient, now much-eroded, much-weathered deposit, was separated from that of the fresher, little-eroded sheet by an elevation of the surface and an increase of slope which gave a markedly more vigorous drainage, and the still further fact

that the interval is emphasized by notable vegetable accumulations, and supported by the strong analogy of the Great Basin phenomena, the whole combination takes on a strength which seems to me altogether irresistible.

While the evidence from plant and animal remains found in the interglacial beds in America has not yet reached a fullness and definiteness which render it altogether decisive regarding the climate of the interglacial intervals, though it indicates something less than a glacial climate, the evidence of this class in Europe, a summary of which is given by Dr. Geikie in the paper already referred to, appears to me entirely decisive, unless the integrity of the observations can be overthrown. The demonstration of a climate in Northern Europe, comparable to the present, seems to carry with it the necessary conclusion that glacial conditions were essentially as remote then as now. It seems to me idle to cite the fact that temperate faunas and floras now exist in the vicinity of glaciers within the temperate zone, for these are mere local phenomena and do not stand related to the general climate of the region, as the great ice sheets of glacial times must have done. The fossil evidence of the acknowledged glacial epoch shows Arctic conditions. On the same basis of reasoning, a temperate fauna and flora show the absence of glacial conditions.

When, therefore, to the strong cumulative physical evidence in America and the similar physical evidence in Europe, there is added the array of organic evidence that has now grown large, it seems to me the case approaches demonstration, so far as the general fact of a great interval or intervals is concerned. To fail to accept this, at least as a working classificatory basis, is to do violence to the highest interests of science.

The history of this question I apprehend, will be very analogous to that of paleontological research. In the earlier stage, investigators were satisfied with very general delineation and rude groupings of forms. But, with the progress of the science, more and more critical scrutiny of likenesses and differences was found requisite, and closer and closer specific and varietal distinctions were drawn. This tendency at length became excessive and the multiplicity of divisions was pushed beyond the actual phenomena. The enlargement of collections then began to bring together many species by intergradations, and the elimination of false species resulted. But yet certain species stand clearly differentiated from all other species and unquestionably will continue to stand. The gaps between them can only be filled by tracing them far back in geological time. So I think it may be with the divisional effort in the glacial field. We are very likely to push differentiation to excess in the stage we are now approaching, and to recognize

for a time, episodes of glaciation that are not widely separated from their neighboring episodes. But this will be wholesome for the time, just as species-making was. It is, indeed, almost necessary to the ultimate end sought to make the sharpest possible discriminations, even if they result in excessive divisions which shall last only until such time as they may be brought together by interstitial formations.

Although not fully placed before the public, at least four divisions of the drift south of the Great Lakes are now being employed as working hypotheses and are gaining strength under the tests of the field. The interval I have chiefly discussed in this paper is the middle one. I have regarded it as the most important one, possibly the only one that will ultimately stand as possessing an epochal value, but I am in sympathy with the movement led by Dr. Geikie, because it will develop a more precise delineation of the history of glaciation. If the terminal moraines of Ontario have much divisional importance, they will make the present working classification of of this country closely analogous to that advanced by the distinguished Scotch glacialist.

It is the present judgment of the majority of critical workers in both the American and European field, that amid all future tests two or more of these divisions at least will stand as well sanctioned epochs, to whatever grade others may be relegated by the fuller knowledge of the future.