Correspondence re: "Development of the theory of multiple glaciation in North America". 1924-1933

Thwaites, F. T. (Fredrik Turville), 1883-1961
[s.l.]: [s.n.], 1924-1933

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Special

multiple glaciation file

10-2-28.

414 Harvard Street, S.E.,
Minneapolis, Minn.,
October 1, 1928.

Prof. F.T.Thwaites,
University of Wis.,
Madison.

My dear Thwaites:-

If it was your intention to draw out into print, by your Development of the Theory of Multiple Glaciation in North America, some things that you might not care to say yourself, you are to be congratulated now. I refer to "Multiple Glaciation" by Dr Charles Keyes in PanAmerican Geologist, Vol. 50, page 131 (Sept. 1928) Presumably you have read it ? For the rest I observe that "The "named company of collaborators determines".

Dr Keyes's handling of the "collaborators" strikes me as very interesting and most of his criticisms are such as I might myself make along that line. It may be interesting to see now whether the "collaborators" will fly to your defense or whether they will merely add a few objections to your work for themselves and let you be "the goat" ?

The reader of Dr Keyes's article need not sympathize in the matter of personalities that are directed at you as author, as seems to me, first because every worker in glacial field makes false interpretations that must be evaluated later and that might have been his sufficient defensive against your criticism of him; and second because he purposely agrandizes you as spokesman for Wisconsin, et al., while making ridicule of what you represent. That seems to me to make a fairly even score as to personalities ?

The statement that "Keyes attacks Kay" may not be your invention nor your fiction since I had heard the same idea before elsewhere and seemingly, to me, the "collaborators determine" in that item :

In fact I am right glad that you wrote what you did as you did for I have great satisfaction in your drawing out Keyes as has been done. How else could we have got all that information ? We had been deceived long enough : You did me a favor anyway.

Very truly yours,

F.W. Sardeson.

March 20, 1928.

Mr. F. W. Sardeson,
414 Harvard Street, SE.,
Minneapolis, Minnesota.

Dear Mr. Sardeson:

I am sending you under separate cover another copy of Multiple Glaciation. I have never seen any bentonite clays in the Shakopee although I thoroughly realize that they may occur. These clay horizons are so persistently covered that it is very hard to get samples from them.

I have seen a clay in the base of the Black River which swells and soon fills a drill hole. I thought this quality shows that this is bentonite, but as I am not an expert with a microscope, have made no attempt at an opinion.

Yours very truly,

FTT-M

Geologist

414 Harvard Street, S.E.,
Minneapolis, Minn.
March 19th 1928.

Professor F.T.Thwaites;-
University of Wisconsin,
Madison, Wis.

My dear Thwaites:-

Will you be so kind as to send me another copy of your "Multiple Glaciation of North America". I have lost the the one already sent me. If possible for you to do so it will be a great favor. I counted on it for handy reference to many data on glacial geology. But, a friend to whom I spoke about your review borrowed my copy and now says that he can not return it, because he lost it.

He lost it, I think, because he lent it to another geologist. Knowing that such a work exists, I can not well be happy without it,- and anyway sometime in the future I may be writing something on which I must cite you for authority. Those two can out-swear me or lick me, together, and from experience in my native town of Argyle, Wis., I know it is much easier to beg a friend than to dispute with two possible enemies ?

Another thing: I see that Clarence S. Ross refers to the bentonitic shale in the Stones River formation as the oldest known of that clay type,- bentonite. I have myself seen bentonite clay in the Shakopee formation in Minnesota, although I have no samples at hand now and probably shall never trouble to go after it or to write about it. Chamberlin, Geology of Wisconsin, refers to clay in the Lower Magnesian limestone, and perhaps that is the bentonite ? Older than anything else known ?

Are you interested in bentonite ?

Very truly yours,

F. W. Sardeson,

414 Harvard Street, S.E.,
Minneapolis, Minn.
2 - 28 - 28.

Dr F.T.Thwaites;- Geological Survey,
Madison, Wis.

My dear Thwaites:-

Your work on "The Development of the Theory of Multiple Glaciation in North America" came in this morning's mail but as I happened to be busy I did not see it for two hours. I hasten to thank you for the copy. I am reading it now page by page after having glanced through it to see that it is very interesting.

You have the same difficulty reading N.H.Winchell as I had. He had several county reports in hand at a time and published them in the Annual Reports first and then later as county reports in the Final Geology Report. He expects us to have in mind all as we read one ?

Vol.1, Geology of Minn., is however a simultaneous issue of County Reports, rewritten or at least revised to the one date 1884. I note that you do not review that one, although it is the key to Winchell -'s thought on glacial stages. I use it primarily.

If you turn to Vol.1, Geology of Minn., (1884) page 385, to the title, "The drift" you find that he says (of Freeborn county) that the hills are "moraines that mark the boundary of the ice-sheet in the last glacial epoch". He says not later but "last". Now that "last" drift does not occur in Mower and Fillmore counties.

In description of the drift of Fillmore county (Op.cit.page 313) Winchell describes the "great drift sheet" as lying there with an "older" drift. "An 'interglacial epoch' separated them". To Winchell at that time there are clearly three drift sheets and glacial epoch -s,- the "older", the implied old or "great drift sheet" and the younger or "last".

That was also his idea as he wrote the Fillmore county report in 1876. There are but two drifts in that County, the "older" and the old and those are described by him while having in mind the younger or "last" drift elsewhere as in Minneapolis, (- Chamberlin's Kettle Moraine, in short). Note that he does not say old drift but "older" drift for that which he found out in the Driftless Area (1876) that is "older" than the "great drift" which is in contrast in turn to lesser drift of Chamberlin.

The plain fact is that in 1884 (Geology of Minn. Vol.1,) Winchell is talking about three glacial epochs while Chamberlin at that time (Geology of Wisconsin Vol. 1, page 261,) speaks of two only. I did correctly assign to N.W.Winchell the discovery of three drift sheet -s in Southeastern Minnesota. He knew of those three as early as 1876,- but got no encouragement from Chamberlin and Salisbury.

Winchell should have been more explicit, of course.

Truly yours,
F.W. Sardeson

Ann Arbor, Mich. March 19 1924.

Dear Mr. Thwaites:-

I have read the manuscript of your compilation of the "Development of the theory of multiple glaciation in North America" with much interest. It seems to me a very fair presentation of the views of the various writers. In some cases there might be some expansion of papers that are distinct contributions to the subject, while others that are opposed to the theory of multiple glaciation might be given less space, especially where paper after paper by such authors as G.F. Wright and Warren Upham make no contribution of any significance. This, however, is a matter of an author's individual taste, and you may see more merit in such papers than I do.

In a few cases you seem to have failed to see the force or significance of an investigation. Thus in the case of Bain(61, p.33) you infer that he merely made theoretical calculations, and excluded effects of glacial drainage, etc. I was in correspondence with Bain at that time and I understood that he made selections of valleys in the Kansan that were not affected by the flow of water from the border of the Wisconsin icesheet, their courses of drainage being northward into the Des Moines valley. It is my recollection that he found these fully 15 times as large as valleys cut under similar gradients and similar deposits in the Wisconsin drift. Any climatic changes, such as periods favorable to loess deposition, would be likely to have less rainfall than the post-Wisconsin rainfall, and would thus take more time rather than less for the excavation of the valleys in the Kansan drift. I am, therfore, citing Bain's study to my classes as of importance in showing that post Kansan time is at least 15 times as long as post Wisconsin time. I also have them inspect the maps and judge for themselves how much older the Kansan drift must be than the Wisconsin.

Another case is that of Hershey(95, p.51) where you state that he did not consider the factors which control the rate of erosion under varying conditions. It seems to me his description of the contours of the post-Kansan valleys and of post-Illinoian valleys is clear ewen if brief, and shows that the former must be very much older than the latter. He also pointed out the advantage the Illinoian district has over the Kansan in being distributed along both sides the main drainage course, the Mississippi, and yet it shows less erosion than the part of the Kansan drift area more remote from the Mississippi. This paper of Hershey's also discusses the relative amounts of stream work in post-Kansan and in post-Ozarkian time. This suggests your omission of a summary of Hershey's paper on the Ozarkian stage, as part of the Pleistocene. You refer to it in the summary of Calvin's paper No.84, p.47. I think it ought to be summarized.

Your discussion of Alden's paper No 122. on pp.66-67 seems to commit you to the view that there is no post Illinoian drift in the region where I had reported the Iowan to occur. Yet your analysis of Leighton's paper on p.97 brings out the occurrence of such a drift, classed by Leighton as Early Wisconsin instead of Iowan. Leighton also tries to excuse Alden's oversight by stating that most of Alden's work was outside the area in question. The studies by Leighton thus confirm what Hershey and I had found. The idea that unleached drift outcrops on slopes was not first brought out by Alden. It is a long recognized condition, dating back before my time, for I had it in use when I began studies as far back as 1883. I may have got it from McGee or Chamberlin.

No154

I note that you call Carman's paper on northwestern Iowa "a very sane and conservative report" (p.80). It has impressed me as somewhat insane. I cannot go into the whole matter here, so will merely ask you to read Macbrides discussion of the features of aggradation on Mill Creek (Iowa Geol. Survey, vol.12, pp311-312), Carman's statements on p.325 as to constructional features superposed on old erosion slopes, and p.335 where he notes the fresh appearance of some of the drift. Also Meinzers discussion of drainage features in O'Brien County (Iowa Geol. Survey, vol 21, p.1059). Carman seems to have made a flunk by not being able to trace the boundary for this post-Kansan drift. It is really definite enough so that I can locate it all the way from the Minnesota line to where it passes under the Wisconsin moraine near Odebolt, Iowa, as well as in its course across south western Minnesota, and eastern South Dakota to where it passes under the Wisconsin drift east of Florence S.D. I have shown its position in a map accompanying my paper in Bull. G.S.A. vol.33, p.103. I there label this post-Kansan drift "Iowan?" thus implying that I am in doubt as to its Iowan age. It may turn out to be Illinoian drift of the Keewatin icesheet. It is a much more definite deposit than the Iowan of northeastern Iowa. On this map you will note that I have left undetermined whether the drift on north side of the Driftless Area is Illinoian or Iowan. My study last summer has led me to class it pretty positively as Illinoian, and I had previously been inclined to regard that which lies outside the Wisconsin drift in Washington and Dakota counties, Minn. as Illinoian. Its border passes under the Wisconsin drift in Dakota County. I am now inclined to think that it may be this drift which comes to view outside the Wisconsin in northwestern Iowa.

I note also your statement on p.140 that the testimony of Lakes Bonneville and Lahontan is a most powerful bit of evidence in favor of essential duality of the Pleistocene glaciation. In my opinion the older of the two glaciations of the Sierra, and of the Wasatch and Uinta Mountains cannot be earlier than the Illinoian stage, for the moraines are preserved on rather steep mountain slopes, and have suffered no more erosion than is consistent with Illinoian age. It seems unlikely that any moraines of Kansan or Nebraskan age would be so nearly intact as these are. So I am teaching my classes that the two high stages of these western lakes seem likely to correlate with Illinoian and Wisconsin glaciations in the neighboring mountains. The Cerro drift of the north base of the San Juan Mts. appears to be much older than the two moraine bordered drifts of that region. It is preserved only in patches, like the Jerseyan drift, or like the drift from the Rocky Mts. east of Glacier National Park described by Alden and Stebinger (No.13, p.136 of your manuscript).

I find there are other geologists who published evidences that the Ice Age was interrupted by warm periods some years before Winchell made the publication which you cite as the probable earliest reference to an interglacial stage (No.1, p.3). In one case, that of Prof. Edward Orton, it is distinctly stated that a warm period seems to have come between two cold ones, and the word interglacial is used. See Geol. of Ohio, vol.1, p.430. This was written in 1872. But in 1870 Orton had written a paper for the American Journal of Science (Silliman's Journal) calling attention to a buried peat deposit near Germantown, Ohio. This is reprinted in the Rept. for 1869 of the Ohio Geol. Survey, Printed in 1871. See pp.165-167. Buried soil in Highland County is described in the Rept. for 1870, p.266-7. Soil below loess is discussed in Geol. Ohio, vol.1, p.440, and pre-Illinoian soil on p.443. All these antedate Winchell's first discussion.

In the Geol.of Indiana,Rept.for 1869,a buried soil beneath the glacial gravel of the Wabash bottoms is noted on p.140,and evidence that two drifts of unlike rock constituents occur near there,see p.141. Buried soil in Franklin County is noted on p.185. In the Indiana Report for 1875 a pre-Illinoian buried soil is noted near Quincy,Indiana,see p.319-320. There may be other cases scattered through the Indiana reports. I think it may be advisable to cite these even though nothing is said by the authors as to multiple glaciation,for they show how early such evidences were being brought to notice.

This leads me to suggest that the cases of buried soil cited by Worthen in the early volumes of the Illinois Geol.Surgey have similar value. Thus in Vol.3,published in 1868,buried soils are mentioned on pp.75,87. In Vol.4,1870,a soil that now appears to lie between the Kansan and Nebraskan drifts was struck in a coal shaft in 1859,seepp.46-47. A soil at Bloomington,between bowlder clays,is discussed on pp.178-179. Reference to buried muck in several counties is made on p.206. Buried loess on Mercer Co.is noted on p.302 . A buried peat over blue till is noted on pp.336-337. In vol.5 1873,buried soil at Peoria,p.236. Soil under loess,and at a lower horizon, p.307. There are others in the later volumes,and I may not have caught all in these early volumes all of which were written before Winchell made his report.

I am interested in noting Orton's statement, Geol of Ohio Vol.I,p.430,that the interglacial stage is coming to be clearly recognized in both Europe and America. It shows that the subject is one on which he had done considerable reading and given it mature deliberation. The three-fold division of Glacial time given on the same page is a very advanced statement for that early date. The section given on p.427 is now known to embrace the Wisconsin gravel deposits above the buried soil. The deposits below are likely to be Illinoian . This makes the soil of Sangamon age. Some of the buried soils farther west,like that near Quincy,Indiana are under Illinoian drift. I am in hopes to have opportunity to study their relations in the field the coming season.

I think Mr.Alden would be glad to see this manuscript,and he might be able to suggest further changes. He might also be able to advise you better than I can as to its publication. It seems to me that for class reference it needs to be as full as you now have it,but for publication for use of scientific men it boiled down and given a more philosophical treatment. If you can bring out clearly who seems to be responsible for each of the advance steps made,and cut out all the writings by mossbacks like W.&U. cited above it would serve a useful purpose to scientific men in various lines .

Very truly yours,

Frank Leverett

I am mailing the manuscript to you today in separate package.

Notes by Wm. C. Alden on
The development of the theory of multiple glaciation in North America.
By
F. T. Thwaites, 1925.

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read - but deposits not called "interglacial"

P. 3-a. You seem not to have noted Orton's first references to interglacial beds cited in Leverett's letter.

P. 30. Cut out part of reference to Upham as of little value.

P. 32. Slight change in reference to Buell's four stages.

P. 66. Change statement in regard to Alden's paper (122) as indicated. In regard to Leverett's comment, see my "conclusions" on page 160 of Professional Paper 106. The supposed "early Wisconsin dirft", described by Leighton in Jour. Geol. 1923, is mostly in those parts of Boone, Ogle, and Lee counties, which I did not examine carefully. Leverett suggested to me that there might be early Wisconsin drift in part of the area east of Rock River, and I later passed on the suggestion to Leighton when one of his manuscripts was sent to me for examination.

It looks as though there may be a post-Illinoian drift east of Rock River and south of Kishwaukee River, and possibly a narrow strip extending from Belvidere northeast past Sharon, Wis. I am not prepared to deny this, but I do not think there is post-Illinoian drift in most of the area mapped by Leverett as "Iowan" north of a line passing from Freeport through Rockford to Belvidere. Leighton agreed with me in regard to this part as a result of his own studies, as you will see by reference to his paper.

This does not agree with Dr Kay's statement

P. 68. Chamberlin(124). Take out the suggestion concerning reverting to the old nomenclature, as Chamberlin realized it was not feasible and did not intend to do it, I think.

P. 69. Change wording concerning Calvin (129).

P. 73. Note change in Leighton's (136).

P. 74. See insert under Tilton (138). I understand that Tilton now recognizes that his "Dallas deposits" belong with Kay's "gumbotil."

Pp.74 & 75. Cut out most of Upham's (140)?

P. 75. See insert under Leverett (142).

P. 80. Carman seems to me to have made a careful study in northwestern Iowa. There are some puzzling things about the area as compared with the Kansan area farther south. It may not be possible to fully explain these without a broad regional study. I myself am holding judgment suspended while Doctor Kay's unpublished studies proceed.

Pp.81-84. Slight changes in Alden and Leighton (155).

P. 85. Insert under Kay (158).

Pp.86 & 87. Substitute following for last two sentences under Kay(168):

It was shown that although the gravels in the vicinity of Afton Junction and Thayer can not be used to establish the presence of two drifts, there is other evidence in the region of Afton Junction, such as gumbotil and peat beds, which makes it clear that the two oldest drift sheets are present and that they are separated in age by a very long interglacial epoch.

P. 92. Insert under Kay (171):

There is, however, grave question raised by other geologists as to whether all the types of animals represented in the "Aftonian fauna" could have lived close to the front of the great Kansan ice sheet.

P. 92. See Lee's (172) insert.

P. 94. See Kay (178). Change wording.

P. 94. Leverett (179). Insert (?)

P. 95. Cut out most of Upham's. I can not make anything out of these later papers of his. They are hardly worth citing.

P. 96. The two papers by Keyes are hardly worth citing.

P. 97. Insert in Leighton (186):

The latter drift is said to lie almost wholly east of the area examined by Wm. C. Alden (122) and (159, pp. 154-160).

Pp.108-109. Coleman (23). Cut out the sentence: "Coleman stated that Aftonian existed in Ohio but did not give any authority." It is possible "Ohio" may be a misprint for "Iowa".

Pp.135 et seq. See changes on manuscript.

P. 137. Alden (15). I sent the paper to Toronto but did not go there and present it.

P. 140. Meinzer (3). Is the conclusion yours or Meinzer's? It is not clear to the reader.

P. 141. See note on "Glacial deposits east of Cody". These are not glacial.

Later papers should be noted, among them Baker's Life of the Pleistocene, also O.P. Hays' Pleistocene of North America and its vertebrated animals, Pubs. 322 and 322-A Carnegie Institution of Washington. These are valuable compendiums, even though one may disagree with numerous statements in them.

UNITED STATES
DEPARTMENT OF THE INTERIOR
GEOLOGICAL SURVEY
WASHINGTON

Dr. F. T. Thwaites,
Wisconsin Geological Survey,
University of Wisconsin,
Madison, Wisconsin.

February 16, 1925.

Dear Doctor Thwaites:

I have gone over your manuscript on "The development of the theory of multiple glaciation in North America," together with Mr. Leverett's comments, and I am returning the manuscript under separate cover with my own suggestions. I have not corrected spelling or edited the English except as indicated.

I note that you state you have "approached the subject with the attitude of a skeptic." I recall some of your skepticism and I think that is perhaps in part due to my own skeptical attitude, at the time we were working together, concerning some of the drift which had been mapped as pre-Wisconsin. It is also perhaps in part due to the difficulty of clearly differentiating more than one pre-Wisconsin drift in Wisconsin. I may say, however, that as I have had the opportunity since that time of extending my field studies more widely over pre-Wisconsin drift sheets, any skepticism I may have had concerning there being more than one pre-Wisconsin stage of glaciation has disappeared.

I went into northeastern Iowa in 1914 with considerable skepticism as to the "Iowan" being a distinct drift sheet. Although there are yet some things about that area which I do not understand, I was, after two seasons' work, forced to the conclusion stated in our report in Vol. 26, Iowa Geological Survey. I note that you have seen a copy of a recent letter of mine to Doctor Kay concerning both northeastern and northwestern Iowa. The latter area is also puzzling.

Any idea that we think the Pleistocene problems are fully solved should be given up at once and the efforts should be directed toward constructive field work rather than destructive criticism.

Of course the most striking break is between the Kansan and the Wisconsin drift, and it early became clear that there were at least two distinct epochs, or stages, of glaciation. The most experienced field students of glaciology have, however, gone far beyond that stage so that, with the possible exception of the Iowan, the Pleistocene classification is not likely to be changed much as regards its main stages. It would be quite erroneous and unfortunate to leave the

reader of your paper with any such idea. I think there can be no doubt that the Illinoian drift is considerably younger than the Kansan. The difference in degree of dissection by streams is very markedly brought out on the new topographic maps which are being issued. Nearly all of that erosion on both till sheets occurred prior to deposition of the main deposit of loess.

There is also good evidence of both the Aftonian interglacial stage and the Nebraskan drift as distinct from the Kansan, entirely aside from the interpretation based on the so-called "Aftonian gravels" at Afton Junction and Thayer. I have repeatedly told Doctor Kay that he is giving many the impression that the Aftonian interglacial stage is being discredited by his recent papers. (See my comments in Bull. G.S.A., vol. 35, pp. 73 and 74, 1924). These short papers by Doctor Kay are in the nature of reports of progress in an uncompleted restudy of the older drifts of Iowa. Much of his data are as yet unpublished. Some of the papers are little more than abstracts. Citations from these should therefore be brought up to date, and should be made with caution. Dr. O.P.Hay takes strong exception to any idea that all those types of animals represented in the "Aftonian fauna" could have lived up close to an ice sheet. Each year a little more is accomplished, but the conditions under which most of us work do not permit rushing through to well supported conclusions in a short time.

I am sending you with the manuscript a copy of my recent preliminary paper on the Physiographic Development of the Northern Great Plains. You will find some new interpretations in this. It may be some time before the final paper is issued. I am now extending the studies southward to Colorado and hope soon to reach the Lake Bonneville basin and see if the lake and glacial stages can not be more closely correlated. No such study has yet been made, so far as I know. Any failure to differentiate four or five distinct stages of mountain glaciation, however, does not militate against there having been that many stages of continental glaciation, for conditions in the mountains do not favor the preservation of distinctly recognizable evidence of so many. Atwood has found evidence of three stages in the San Juan Mountains, and Blackwelder in the Wind River Mountains. I saw the latter last summer and in 1923 for the first time.

On the whole, I think your summary of the papers is fair, but I am inclined to think you leave the reader with the impression that the evidence of four or five distinct stages of glaciation is much weaker than one who has studied the older drifts in the field knows it to be. Some of the papers cited as opposed to the idea are much less supported by observed facts than are those favoring the idea. Most of the readers will not be sufficiently well informed on the subject to discriminate the weak papers from those of value. Those who have studied the drifts most <u>in the field</u> are convinced

that there were four, if not five, distinct stages of glaciation separated by notable interglacial stages. The evidence is not everywhere such, however, that a skeptic can be convinced without extended regional study.

If properly fixed up, even from your point of view, the paper should be of interest and sufficiently useful to warrant publication. It ought to be brought up to date of issue and there should be a good unbiased summary, even if brief. If the Wisconsin Survey or University can not publish it, perhaps it might be offered to the Smithsonian Institution, as was C.E.P. Brooks' paper on "The correlation of the Quaternary deposits of the British Isles with those of the continent of Europe," Smith. Rept. 1917, pp. 277-375 (1919). Should you offer it to the Smithsonian, and should I be asked by the Secretary for my opinion as to its publication, I would have to reserve judgment until I saw how you had met the suggestions which I am sending you.

I hope you will pardon my long delay in attending to this matter.

With kindest regards, I am,

Very truly yours,

W. C. Alden

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include

1-21-28

Jan 20 '28

Dr. R. T. Thwaites:
University of Wisconsin

Dear Thwaites:--

Just got your paper on Multiple Glaciation and couldn't lay it down till I had finished it. It is a fine job! and the area is a very valuable contribution. You have spread things out in a very happy way. You have given just what we want and haven't complicated it with details to make it hard reading. I am delighted to get it & thank you not only for sending me a copy but also for doing this fine piece of work for all of us "Pleistocenes".

The old interglacial soils, upon which I am working now, are driving my mind toward very long times for the Pleistocene interglacial periods. If the ice didn't melt entirely off the continent it certainly stayed out of Illinois for a long, long

time during these interyl. intervals.

Let me thank you again

Very cordially

P. MacQuiloch.

IOWA STATE TEACHERS COLLEGE
CEDAR FALLS, IOWA

DEPARTMENT OF NATURAL SCIENCE: E. J. Cable, Ph. D., Head

A. E. Aitchison, M. S., Geography
Marguerite Uttley, M. S., Geography
Vera Rigdon, M. A., Geography
B. L. Swalwell, M. S., Hygiene & Sanitation
W. S. Scott, Ph. D., Agriculture
C. W. Lantz, Ph. D., Botany
Roy L. Abbott, Ph. D., Biology
H. Earl Rath, M. S., Agriculture
O. R. Clark, M. A., Botany
J. S. Hodges, Taxidermist
Winifred Gilbert, M. S., Nature Study

Jan. 22 '28.

Professor F. T. Thwaites,
University of Wisconsin
Madison Wisconsin.
My dear Professor Thwaites:

Just received your article on "The Development of the Theory of Multiple Glaciation in North America" for which except my thanks. I am sure it is a valuable contribution to the phase of the work.

Sincerely Yours.
E. J. Cable

UNIVERSITY OF KENTUCKY

LEXINGTON

January 31, 1928.

COLLEGE OF ARTS AND SCIENCES
DEPARTMENT OF GEOLOGY

Mr. F. T. Thwaites,
Science Hall,
University of Wisconsin,
Madison, Wisconsin.

Dear Mr. Thwaites:

We were very pleased to get your reprint on the subject of multiple glaciation. I think such a summary has been needed and it's very nicely done.

We continue very busy and I suppose always will be busy. The second semester starts tomorrow. Among other things I'll have again the course in topographic mapping. It's an easier course to give here than at Madison as we can get into the field so much sooner.

We received an announcement of Miss Loft's marriage today. It came pretty much as a surprise.

Keep me on your mailing list. I'm still much interested in glaciation.

Regards from both of us.

Sincerely,

Raymond E. Murphy.

1-27-28.

Thwaites

LIBRARY OF CONGRESS
DIVISION OF MAPS
WASHINGTON

Jan. 24, 1928

Dear Fred:

Your book on the development of the theory of multiple glaciation in North America is at hand and I have just finished reading it. You have done splendidly a long and tedious piece of work which was well worth doing. I am sorry you did not make a Ph. D thesis of it. It is much better than most theses. Hereafter anyone who writes on glacial correlation will have to consult your work.

With warm regards

Very sincerely yours,

Lawrence Martin

Mr Fred T. Thwaites
Madison, Wisconsin.

P.S. I suppose Bean and I, as well as Wright, are responsible for some of the "expressions of skepticism" which you recorded on page 42.

All in all. I think you have been very fair.

Thwaites—
Theory of multiple Glaciation
in N.

UNITED STATES
DEPARTMENT OF THE INTERIOR
GEOLOGICAL SURVEY

Ann Arbor, Mich January 25—1928.

Dear Mr. Thwaites:

Your reprint of "The development of the Theory of Multiple Glaciation in North America" has been received, and I have given it a hasty reading. I think you have done valuable service & those of us working in this branch of geology in preparing this summary. The moderate suggestions you make in the line of criticism of certain papers also seem helpful. I wish, therefore, to express my thanks and gratification for what you have done—

Very truly yours,
Isaac Loverett.

INDIANA UNIVERSITY
BLOOMINGTON, INDIANA

DEPARTMENT OF GEOLOGY

E. R. CUMINGS, GEOLOGY AND PALEONTOLOGY
W. N. LOGAN, ECONOMIC GEOLOGY
C. A. MALOTT, PHYSIOGRAPHY
S. S. VISHER, GEOGRAPHY
W. M. TUCKER, PHYSIOGRAPHY
J. E. SWITZER, GEOGRAPHY
A. R. ADDINGTON, PHYSIOGRAPHY
R. E. ESAREY, ECONOMIC GEOLOGY

Jan. 28, 1928.

Mr. F. T. Thwaites,
University of Wisconsin,
Madison, Wisconsin.

Dear Mr. Thwaites:

I wish to thank you for the copy of your recent paper "The Development of the Theory of Multiple Glaciation in North America". I have read it through with much interest. In this publication you have gotten together an excellent synopis and bibliography of the literature dealing with a very interesting phase of glacial geology. It is somewhat noteworthy that the preponderance of papers which present data bearing on the problem in the central area are are from Iowa or an adjacent state. Indiana, Michigan and Ohio have little to offer. It may be noted also that Iowa is a great producer of glacialists. Work is still being carried on vigorously in Iowa and adjacent states. Indiana and Ohio have produced little in the past with respect to the solving of glacial problems and little or nothing is being carried on by men within these states at the present time.

The problem of glacial stages in Indiana has been touched upon in but few papers. Leverett discussed it in Monogr. 38, 1899, p. 109, and Fuller in the Ditney Folio # 84, 1902, p. 3. In the Patoka Folio, # 165, 1908, the latter author practically repudiated his suggestions of a pre-Illinoian glaciation in southern Indiana. These suggestions of a glacial stage in southern Indiana were alluded to in the "Handbook of Indiana Geology", 1922, p. 141. I have myself seriously considered the evidence of a pre-Illinoin glacial stage in southern Indiana, and on pages 143-144 of the "Handbook of Indiana Geology" have suggested that certain drift features may possibly represent an earlier glacial stage than the Illinoian. Since the time of the appearance of the "Handbook" I have made a more thorough study of the attenuated Illinoian drift border, and am now convinced that no substantial case may be made in favor of a pre-Illinoian glacial stage in southern Indiana. This conclusion is stated in my recent paper on the "Glacial Boundary in Indiana", Ind. Acad. of Science for 1925, pp. 100 and 106.

I do not have the reference at hand, and perhaps you know of it anyway, but I may remark that Von Engeln in his revision of Tarr's Physical Geography a couple of years ago stated that there have been but two ice invasions in the United States, though in the opinion of a number of geologists there have been more than two. I could not help but wonder why the author of such a statement could so ignore the positive evidence of more than two glacial stages.

In your summary of the central district you raise a number of interesting questions some of which I regard important. Two of them are the attention which you have called to the ice lobes reaching far into the zone of ice wastage and to the question of

of wind direction, whether dominantly toward or away from the ice sheets (p.123). The latter would have much to do with the character of the climatein the areas invaded by the ice sheets. I have always felt that the ice sheets came out of a region of glacial climate where the greater mass of snow and ice accumulated, and that they then entered a region of non-glacial climate where the ice sheets wasted away. Certainly weighty evidence could be brought together which would warrant such a conclusion, yet I do not know of any particular paper which attempts this. Do you know of any? It does not appear that paleontological evidence may be very conclusive with respect to the problem. It would rather be supplementary or would not bear directly, yet the references to the problem have chiefly come from paleontology. We point to the great glacial plains in the northeastern part of the United States. They were built from the material which has been brought there by great ice sheets. They were made by ice sheets which melted away, which had advanced into a region of non-glacial climate. To the north of the great glacial plains lies a great region from which much of the material came gatheredby ice sheets which accumulated in a region of glacial climate. May we not speak of the two greatly contrasting regions with some assurance that they really existed, though they were alternately progressive and retrogressive during the several ice stages? In support of the two existing areas of contrasting climate we cite the Driftless area and the lack of local glaciation in the Appalachians south of the glacial boundary. You refer to these. In respect to the Driftless Area I am inclined to the opinion that its very existence is dependent upon the fact that the region south of the Great Lakes was chiefly a region of non-glacial climate. Of course I would recognize the existence of the two other factors which are the usual ones cited. Further, the suggestion comes to me that the ice did not advance very far to the south in Montana and on the Columbian Plateau partly (if not chiefly) because of the presence of Chinook winds in these regions. Possibly Bretz's "Spokane Flood" may be related to some such winds which for some reason were unusually prolonged. To my mind the problem seems to be a fundamental one, one which in its conception offers a framework or background of vital importance. Certainly it is an interesting problem. Why has it not been attacked and all available evidence brought to be upon it?

Very truly yours,

Clyde A. Malott

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF GEOLOGY

E. R. CUMINGS, GEOLOGY AND PALEONTOLOGY
W. N. LOGAN, ECONOMIC GEOLOGY
C. A. MALOTT, PHYSIOGRAPHY
S. S. VISHER, GEOGRAPHY
W. M. TUCKER, PHYSIOGRAPHY
J. E. SWITZER, GEOGRAPHY
A. R. ADDINGTON, PHYSIOGRAPHY
R. E. ESAREY, ECONOMIC GEOLOGY

DEPARTMENT OF
REGISTRATION AND EDUCATION
A.M. SHELTON, DIRECTOR
SPRINGFIELD

STATE OF ILLINOIS
STATE GEOLOGICAL SURVEY DIVISION
M. M. LEIGHTON, CHIEF

SECTION OF SUBSURFACE DATA
L. E. WORKMAN, ASSOCIATE GEOLOGIST

URBANA

January 24, 1928

1-25-28,

Mr. F. T. Thwaites
Geological and Natural History Survey
Madison, Wisconsin

Dear Mr. Thwaites:

I have read parts of your paper on the "Development of the theory of multiple glaciation in North America" with great interest and would like to have a copy for further reading and reference. Will you be so kind as to have one sent to me, informing me of the charges for it.

Thanking you, I am

Very truly yours,

L. E. Workman

Sent 1-25-28

RAYMOND C. MOORE
CHESLEY J. POSEY
WALTER H. SCHOEWE
KENNETH K. LANDES
GAROLD L. KNIGHT
DAVID M. DELO
JOHN M. JEWETT

THE UNIVERSITY OF KANSAS
DEPARTMENT OF GEOLOGY
LAWRENCE

Feb. 4, 1928.

Mr. Fred Thwaites,
Science Hall,
Madison, Wisconsin.

Dear Fred :

Thanks very much for the copy of your paper on " The Development of the Theory of Multiple Glaciation in North America ". I think you have made a real contribution to the branch of Glacial Geology. Your paper contains a world of information and an excellent bibliography on glacial papers. Your paper contains very many things that most glacial geologists wanted to know and thus you have saved them considerable work and effort. So again, I want to thank you personally, and also as one representing the branch of glacial geology.

I am sorry that I did not get to hear your paper at Cleveland. You were just about to appear on the program when I had to leave. I hope some time we can get our papers, either glacial or physiographic, scheduled before the last morning.

Cordially yours,

Walter H Schoewe

2-10-28

GEOLOGY AND MINERALOGY

ALFRED C. LANE
HOUSE PHONE UNIVERSITY

BARNUM MUSEUM, TUFTS COLLEGE
BOSTON POSTAL DIST. No. 57, MASS.

Feb 8. 1928

Glenrock paper

My Dear Dr Thwaites:— Many thanks for the valuable report No. 13. I hope perhaps later we can spot the Centronia zone just above the St. Peter; perhaps some of the Black Shale p. 17.

I chuckle at your "aerial" geologic map. Some people might go right up in the air!

I send under separate cover a copy of my Ca Cl2 paper for your Medelian.

Yours as ever

A C Lane

2-10-28

UNIVERSITY OF ILLINOIS
DEPARTMENT OF GEOLOGY
URBANA, ILLINOIS

February 9, 1928

Professor F. T. Thwaites
University of Wisconsin
Madison, Wisconsin

Dear Professor Thwaites:

I have just received a copy of your bulletin dealing with underground water in Illinois and the general stratigraphy in that part of Illinois which borders Wisconsin. I am much interested in your discussion on water quality and problems and I am very much pleased to have a copy of this contribution.

With best wishes,

Yours sincerely,

T. C. Chamberlin

TTQ:GS

STATE UNIVERSITY OF IOWA
IOWA CITY, IOWA

THE UNIVERSITY LIBRARIES

JOHN BOYNTON KAISER
DIRECTOR

Referred to
Mr. F. T. Thwaites

February 23, 1928

Wisconsin Academy of
Science, Arts and Letters
Madison, Wisconsin

Gentlemen:

We wish to order for this Library the publication noted below.

Please bill on the enclosed voucher form, and address both book and bill to the undersigned in order to insure safe delivery.

Very truly yours,

Grace Worner

ACTING DIRECTOR

GW:MB

6 Thwaites: The Development of the Theory of Multiple Glaciation in North America - 1928 Wis. acad. Sci, arts + letters Jan. 1928

To be sent 2-29-28 Vouchers to be returned with bill.

Thwaites

UNIVERSITY OF ILLINOIS
DEPARTMENT OF GEOLOGY
URBANA, ILLINOIS

February 16, 1928

Dr. F. T. Thwaites
Wisconsin Geological Survey
Madison, Wisconsin

My dear Dr. Thwaites:

I have just learned of the publication of your paper on "The Development of the Theory of Multiple Glaciation in North America". Inasmuch as the Wisconsin Transactions are not readily accessible here I would appreciate very much a separate of this paper if you can spare one.

Yours very truly,

Arthur Bevan

Sent 2-18-28

ACB: GS

UNIVERSITY OF ILLINOIS
DEPARTMENT OF GEOLOGY
URBANA, ILLINOIS

February 22, 1928

Dr. F. T. Thwaites
Wisconsin Geological Survey
Madison, Wisconsin

My dear Mr. Thwaites:

I thank you for the copy of "The Development of the Theory of Multiple Glaciation in North America" which arrived a few days ago.

Yours very truly,

Arthur Bevan

AB:S

THE STATE UNIVERSITY OF IOWA
IOWA CITY

DEPARTMENT OF GEOLOGY

GEORGE F. KAY
ARTHUR C. TROWBRIDGE
ABRAM O. THOMAS
JOSEPH J. RUNNER
ALLEN C. TESTER

February 29, 1928.

Mr. F. T. Thwaites,
Science Hall,
Madison, Wisconsin.

Dear Mr. Thwaites:

Some time ago I received from you the copy of your article on "The Development of the Theory of Multiple Glaciation in North America." I have gone through this article and find that it has a great deal of very good material in it. It is an excellent source book. Would it be possible to have three or four more copies of this article which we could have bound and retained in our Library for student reference? If so, I shall be very glad to hear from you.

With best regards, I remain

Yours very truly,

Allen C. Tester

ACT:RG

2-29-28
6 copies sent at request of librarian

TUFTS COLLEGE
DEPARTMENT OF
GEOLOGY AND MINERALOGY
ALFRED C. LANE

Multiple glaciation

BARNUM MUSEUM
TUFTS COLLEGE, MASSACHUSETTS

February 29, 1928.

Dr. F. F. Thwaites
University of Wisconsin
Madison, Wisconsin

My dear Dr. Thwaites:

Thank you for your two papers, and I can see your usual sane attitude coming out, especially in the one regarding multiple glaciation. I can see also that my paper would interest you with regard to your sentence that "one went so far as to virtually deny the existence of such a thing as out-wash" on page 163.

Very truly yours,

Alfred C. Lane

ACL:RFS

THE UNIVERSITY OF ROCHESTER

ROCHESTER, NEW YORK

DEPARTMENT OF GEOLOGY

HERMAN LEROY FAIRCHILD
PROFESSOR EMERITUS
HAROLD L. ALLING
~~ALFRED C. HAWKINS~~
J. EDWARD HOFFMEISTER
FLORENCE WHITBECK

May 4, 1928,

Dear Professor Thwaites:

Sometime ago you kindly sent me your book on "... theory of multiple glaciation ---" It is a fine piece of work, and an excellent summary of the glacial history. Your "General Conclusions" is the best and wisest statement ever written.

I yet feel that the Toronto beds and fossils may not require complete deglaciation. In the final removal of the ice-sheet from New York I find only one break - between the first and the second Vanuxem - and this seems only an ice-front oscillation in New York. Any considerable deglaciation must have preceded our Wisconsin, according to theory. But, why no undisputed interglacial phenomena in New England, New York and Penn.?

Could not the warm-climate

(over)

organisms at Toronto have been carried in from some southward origin? The question deserves study.

I have an idea that the Toronto and James Bay fossils belong somewhere in the later glacial lake succession of the closing phase of Wisconsin. But I have not yet envisioned the physical events.

Late in the summer I hope to send you my book on the Genesee Country.

Sincerely

H. L. Fairchild

I hope that you know Ira Edwards, of the Milwaukee Museum, one of "my boys".

4-27-28

UNITED STATES
DEPARTMENT OF THE INTERIOR
GEOLOGICAL SURVEY
WASHINGTON

Apr 25, 1928

Dear Dr Thwaites, -

I wish to thank you for the copy paper on Multiple glaciation I have been much interested in going over this again and find it very convenient for reference.

I often think of the enjoyable seasons spent in tramping and driving over S.E. Wis. and would like to see more of The State. I hope you will go on with mapping the rest of the State in detail.

Very truly yours

W. C. Alden

Geology Department,
State University of Iowa,
Iowa City, Iowa,
March 25, 1929.

F.T. Thwaites,
University of Wisconsin,
Madison, Wiscomsin.

My dear Doctor Thwaites;

Dean Kay of the State University of Iowa, in his course in Pleistocene Geology has referred me to your publication on "The Developement of the Theory of Multiple Glaciation in North America." My master's thesis is on a glacial problem and for these reasons I would like to obtain a copy of this publication, if it is still available.

Sincerely,

Doris M Wilbur

THE STATE UNIVERSITY OF IOWA
Museum of Natural History
IOWA CITY

HOMER R. DILL, DIRECTOR

12 Natural Science Bldg.

March 25, 1929

Dr. F. T. Twaites,

Madison, Wis.

Dear Sir:

I am writing a thesis on the Pleistocene fauna of Iowa here under the direction of Dr. Thomas and Dean Kay. Would be very glad if you will send me your recent publication , " The Development of the Theory of Multiple Glaciation in North America.

Yours very truly,

Geo. M. Clement,

Geo. M. Clement

March 27, 1929

Mr. Geo. M. Clement,

12 Natural Science Bldg.,

University of Iowa,

Iowa City, Iowa

Dear Sir: In reply to yours of the 25 my supply of separates on "The Development of the Theory of Multiple Glaciation in North America" is now so low that I can not spare more of them unless the need for them is known to be permanent and not temporary, Last year and recently I sent several copies to students at your University but regret that I can do so no longer.

Very truly yours,

F. T. T. Geologist

Same to Doris M. Wilbuxxr,
Orvob C. Brown

Iwm

Geology Department,
State University of Iowa,
Iowa City, Iowa,
March 25, 1929.

Professor F.T. Thwaites,
Geology Department,
State University of Wisconsin,
Madison, Wisconsin.

Dear Professor Thwaites;

I am a member of Dean George F. Kay's Class in Pleistocene Geology in which he has spoken very highly of your paper, The Development of the Theory of Multiple Glaciation in North America, regarding its usability with reference to Pleistocene literature.

If I am not presuming too much, I would be very pleased to have you send me a copy of this paper, or advise me how I might obtain one. Thanking you in advance for this favor, I am

Yours very sincerely,

Irvin C. Brown

SEVENTH ANNUAL FIELD CONFERENCE
OF THE
KANSAS GEOLOGICAL SOCIETY

412 UNION NAT'L BANK BLDG., WICHITA, KANS.

SEPTEMBER 3, 4, 5, 6, 7, 1933

June 14, 1933

FIRST ANNOUNCEMENT

The members of the Kansas Geological Society and participants of former Field Conferences have indicated a desire for our Seventh Field Trip to be in the Ozark Region of Missouri, Arkansas, and Oklahoma to study the pre-Pennsylvanian rocks.

We plan to assemble at Joplin, Missouri, Saturday evening September 2, 1933, for registration and start early Sunday morning on a trip eastward going down the geological section from Mississippian to pre-Cambrian, then southwestward back up thru the section, ending Thursday noon near Spavinaw, Oklahoma.

Dr. H. A. Buehler is taking charge of the trip thru Missouri, Dr. Branner thru Arkansas, and Ira Cram in Oklahoma. These men will be supported by leaders and assistants well acquainted with the rock exposures to be studied. The itinerary for the trip is as follows:

On Sunday, September 3rd, going eastward from Joplin, we will study exposures of the Mississippian. From Springfield eastward to Northview, we will examine exposures of Kinderhook shales and sandstones. Between Northview and Rolla, we will encounter the Canadian and Ozarkian formations. A side trip will be made to Decaturville to permit an examination of the pegmatite dike and the pronounced structural features of that area. This locality presents some of the most interesting structural and stratigraphic problems of the entire Ozark region.

On Monday, September 4th, the trip will be extended from Rolla thru Salem and Eminence to West Plains. The Salem Plateau with exposures of the Canadian dolomites and sandstones will be crossed. Opportunity will be given to study initial dips, the igneous rocks, and the Ozarkian succession near Eminence. In part, this trip is very scenic. Unusual topographic features will also be seen.

On Tuesday, September 5th, we go from West Plains, Missouri, thru Mammoth Spring, Arkansas, Melbourne, Sylamore, and Yellville to Harrison, studying the succession from the Ordovician (Jefferson City) to the Mississippian (Boone). The section is crossed seven times. A fine series of exposures of the Calico Rock and St. Peter sandstones are visible between Melbourne and Sylamore on a recently opened road. A new series of outcrops have been opened up on State Highway 14 south of Yellville.

On Wednesday, September 6th, from Harrison thru Eureka Springs to Rogers and Fayetteville, we cross the section from Ordovician to Mississippian four times and study especially the Mississippian near Fayetteville.

On Thursday morning, September 7th, we travel from Fayetteville over Mississippian to the Illinois river area northeast from Tahlequah where the Ordovician rocks found in the subsurface of Oklahoma and Kansas are excellently exposed. Almost every bed of the eastern Oklahoma Ordovician is readily visible and the formational contacts are clear. From the Illinois river area the trip extends over Mississippian northward to the Spavinaw area where the granite and Arbuckle are well exposed.

Each evening a meeting will be held and the leaders will briefly review the geology seen that day and outline the geology to be seen the following day. A program will follow with geological papers and discussions.

This makes a 4½ day trip, two days of which are business holidays, Monday being Labor Day. The trip has been planned to reduce as much as possible the personal expense of those attending. Hotel rooms and meals will be arranged for in advance, usually at reduced prices. The route from Joplin to Spavinaw is about 600 miles over highways and good roads. In Missouri we will be escorted by the State Highway Patrol. Registration for the trip including the guide book will not exceed ten dollars. We will appreciate your bringing a car, especially the five passenger models. If you cannot do this, come anyway and we will see that you get transportation.

Guide books will be furnished to all attending the trip, giving a detailed road log with a clear cut picture of the geology in the form of many columnar sections, maps, and explanatory notes. This detailed pre-Pennsylvanian information will be a very valuable reference, especially to those studying the subsurface stratigraphy of Oklahoma and Kansas. Extra copies of this book may be purchased at a price not exceeding five dollars each.

Kindly fill in and mail the enclosed postcard so that we may know how many guide books to have printed and be able to plan for those attending and make arrangements for transportation.

FIELD CONFERENCE COMMITTEE

Thos. H. Allan
E. P. Philbrick
J. L. Garlough