Correspondence re: William S. Cooper, J. M. Hansell thesis, "Glacial geology of northwestern Wisconsin". 1930-1932

Thwaites, F. T. (Fredrik Turville), 1883-1961
[s.l.]: [s.n.], 1930-1932

Bear

UNIVERSITY OF MINNESOTA
COLLEGE OF SCIENCE, LITERATURE, AND THE ARTS
MINNEAPOLIS

DEPARTMENT OF BOTANY

July 22, 1930.

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

Dear Dr. Thwaites:

I am very grateful to you for sending the reprint and also the copy of your lecture notes, which have just arrived. I have read them both with interest and I am very glad to have the references to other literature. I am more than ever convinced that I have several interesting contributions to make to the knowledge of outwash.

I dislike to trouble you further while you are engaged in field work, but I am going to venture to ask one more question. I am planning to work in the region of Grantsburg during August, being very much interested in what the Mississippi was doing during the maximum extension of the Grantsburg Lobe. I am wondering if the Wisconsin men have worked in that region, and if there is anything published or available in manuscript that will save me unnecessary field work.

I am looking forward to discussing my problems with you some time during the Fall.

Sincerely yours,

William S. Cooper

W. S. Cooper, Professor,
Department of Botany

WSC:EM

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I am looking forward to discussing my problems with you some time during the fall.

Sincerely yours,

William S. Cooper

Department of Botany
University of Minnesota
Minneapolis

June 6, 1930

Dr. A. R. Crook,

State Museum,

Springfield, Illinois

Dear Dr. Crook:

I wish to thank you for the two separates recently sent me.

I was in your museum last July with Dr. Leighton but as it was after closing hours we had no chance to call on you. I expect to be in Illinois again this summer working on road materials for the State Geological Survey but I will probably not reach Springfield again.

Very truly yours,

F. T. Thwaites

Brown Hotel,

Fairfield, Illinois,

July 14, 1930

Dr. W. S. Cooper,

Dept. of Botany,

University of Minnesota,

Minneapolis, Minnesota

Dear Dr. Cooper:

Your letter of the second was forwarded to me where I am working for the summer. I will not be in Madison until after the middle of September. I will be most pleased to see you after that date.

Mrs. Thwaites writes me that she has sent you a separate of my paper on pitted outwash and I hope that it may help you. I will write her to also send you the portion of my mimeographed outline of glacial geology which treats of outwash. I also have seen the deposits of Glacier Bay and fully realize the poor quality of most of the published papers on this subject. A bibliography of papers on outwash is included with the outline.

Very truly yours,

Associate Geologist

UNIVERSITY OF MINNESOTA
COLLEGE OF SCIENCE, LITERATURE, AND THE ARTS
MINNEAPOLIS

DEPARTMENT OF BOTANY

July 2, 1930.

Dr. F. T. Thwaites,
Department of Geology,
University of Wisconsin.

Dear Dr. Thwaites:

My friend, Dr. Lawrence Martin, has suggested that you can help me in solving a problem in which I am engaged.

I am working on a study of the post-glacial history of the Mississippi River from Brainerd to Minneapolis. This may seem a strange subject for a botanist. As a matter of fact, however, my field of work lies in the transitional zone between botany and geology. The present study began as an investigation of the vegetation of some local ancient sand dunes. This led to a study of the dunes themselves and the problem has gradually broadened until it now includes the whole history of the post-glacial history of the River valley in both its physiographic and vegetational phases. On the physiographic side, my findings run counter to the published conclusions of the Geological Survey and others. In particular, I am interested in certain extensive outwash deposits that seem to be of rather unusual character, and it is here that you can probably help me.

I need to know all there is to know about outwash; its various types and kinds and its ~~noted region~~ mode of origin. I know of no place in which such information is summarized, and have been rather unsuccessful in discovering any publications that are of value to me. I will be very grateful if you will refer me to any literature that treats of outwash and its formation.

It would be a great pleasure also to have an opportunity to talk over my problems with you, and if you should happen to be near this region to show you the features themselves. It may possibly be convenient for me to stop at Madison sometime during the summer or fall if you are to be there.

I may add that I have seen considerable of modern outwash and associated features, especially in the Glacier Bay region in Alaska.

I will be very grateful for any help which you can give me.

Very truly yours,

William S. Cooper

W. S. Cooper, Professor,
Department of Botany

Am sending Pittsburgh Outwash Pamphlet

WSC:EM

Dec. 22, 1931

Dr. William S. Cooper,
Hotel Pontchartrain,
New Orleans, Louisiana

Dear Dr. Cooper:

Your letter of the 17th arrived just too late to head off my returning the manuscript to Minneapolis. I am sorry that I forgot to look up Martin's statements in Monograph 52 but took his last published work only.

I take it that I am to keep the esker photographs. Thank you very much for them.

Dr. Hansell read the manuscript very carfully. He did some more work in that area last summer. Dr. Aldrich and Mr. Dean did not get time to read it, I understand, although they had it for some time. Therefore, we decided to send it along regardless.

I am sorry that I forgot to include the map and so am sending that enclosed.

The outline of glacial geology or rather text, for it has grown a lot recently, is coming along slowly. I fear a small number of studdents this year. Physiography fell off to 12. But I have strong hopes for a large class in mapping. Meantime we are beset with rumors of salary cuts and high income taxes based on 1930 income! But we will hope for the best and carry on as usual. I will remember you when I get the glacial text finished. Doubtless some will rave when they read some of the new ideas and new correlations but they will have to get over it.

Wishing you a Merry Christmas and a Happy New Year,

Sincerely,

Lecturer in Geology

UNIVERSITY OF MINNESOTA
COLLEGE OF SCIENCE, LITERATURE, AND THE ARTS
MINNEAPOLIS

DEPARTMENT OF BOTANY

Dec. 17, 1931.

Dr. F. T. Thwaites,
Department of Geology,
University of Wisconsin,
Madison, Wisconsin.

Dear Dr. Thwaites:

I thank you for your second letter with the many quotations. I am leaving for the South on Saturday and am taking all the materials and comments relating to the glacial decline paper with me. I will work them over next month and will hope to have the paper ready for publication very soon. Martin has already read the paper, since he is a very good friend of mine. He called my attention to some statements in monograph 52, in which he gave expression very briefly to views very like ours. I will gladly wait until your new outlines are ready and will be very happy to see them then.

I am sending you a fresh copy of the esker photograph. The one that I sent with the paper was imperfect and need not be returned.

If Aldrich has read the manuscript will you kindly return it to me with the map car Hotel Pontchartrain, New Orleans, La.? I will have my headquarters there for at least the next two weeks.

With best wishes,

Sincerely yours,

William S Cooper

W. S. Cooper, Professor,
Department of Botany

WSC:EMF

GLACIAL GEOLOGY OF AN AREA IN THE NORTHWESTERN CORNER OF WISCONSIN

J. M. Hansell, 1930

The report submitted by J. M. Hansell covers the northwestern corner of Wisconsin. It is accomapanied by several topographic maps on which the geology is shown. The main thesis of the report is to prove the existence of a lake in the Barrens of that region.

The topography of the region is described in extreme detail and much of this information has of necessity to be repeated under the heading of Geology. The length of this portion seems excessive.

The main fault of the report is the fact that it is obviously devoted to an attempt to prove a single hypothesis and not to a general discussion of possible explanations. It is clear that over large areas the author could have possessed little detailed knowledge of the drift. Sections exposed in gravel pits and cuts are rarely discussed. The report is sadly lacking in photographs and text diagrams. Such statements as "good beach gravel" or "like-sized gravel" convey little meaning. At no place is the derivation of the lake clay discussed; did it come from the non-calcareous red drift of the Lake Superior region or from the calcareous graydrift of the Grantsburg lobe?

In giving the glacial history is is curious that it is begun backward and the Wisconsin drift is discussed before the pre-Wisconsin. In the same way the descriptions of the better known Lake Duluth basin are for the most part given after those of the Barrens Lake. In some cases, as with the description of topography, this has entailed considerable repetition. No clear discussion of various possibilities or of the controling features which governed lake levels is given. The possiblity that the Barrens were aggraded to the level of the 920 foot terrace at St. Croix Falls after the extinction of the lake but before the abandonment of the Brule outlet is not mentioned. In fact, the entire discussion is quite unsatisfactory although the critical localities have been studied.

The matter of the rough topography in the Barrens is poorly treated. The assignment of the summit flats to lake planation in view of the lack of bowlder pavements and of well defined lake beaches seems preposterous. The possibility that these flats may be either (a) delta tops, or (b) remnants of pitted outwash seems not to have been considered. The prevailing southwest dip of the cross bedding seems to argue for streams flowing southwest but this fact is not mentioned. An especially weak point is the discussion of the stratified materials in Douglas County. Their physical character should show whether beach, stream, or ice-marginal. In general the description of the physical character of the sediments is most inadequate. The reader is left in considerable doubt on many important points in this connection.

The English in which the report is written is in many places involved and obscure. In many places it is said that something is "occasionally marked", "occurs often", or in other ways time and place are hopelessly mixed. In several instances different portions of the same sentence are not of parallel construction.

In present form the report could not be recommended for publication for it is lacking in too many particulars. It does not show a thorough and broad grasp of the problems involved but seems rather to be a record of rather casual observations made in the course of the magnetic survey, many of them by others than the author of whom many seem to have been quite inexperienced geologists. Its seems doubtful if the author has been over enough of the area himself to be in a position to complete a satisfactory report.

Hansell thesis

pp. 9-13 Detailed physiography seems much too detailed for the reader to grasp. Since no interpretaion is given here much has to be repeated later in report
p. 35. The discussion of the drift area between Lake Duluth and the Barrens is incomplete and unsatisfactory. The physical nature of the stratified sediments is not mentioned and this is a vital factor in interpretation. One of the geologists once called one of the townships a "text book example of pitted outwash" but this fact is not mentioned in the report.
p. 50 The topography of the gray drift may be due largely to its high clay content. The "reworked drift" may be pitted outwash. The local occurence of lake clays and silts is no evidence of a large or widespread lake as they are very common almost everywhere having been formed in local ponds in kettles.
p. 44 Suggestion of leaching below a lake seems impossible
p. 50. Flats above sags suggest a pitted plain or pitted deltas. Dexcription of the beach gravels inadequate as no photographs are given.
p. 53 Phrases "good beach gravel", "good terrace" instead of well marked, perfect, etc.
p. 54 Hypothesis of lake planation ignores bowlder pavements which would certainly be produced if knolls of till were leveled. See conditions in Apostle Islands. Similar instances are not common. Theory also not in harmony with weak development of beaches. Influence of floating ice on wave action not mentioned.
p. 57 Phrase "occasionally marked" is a confusion of time and place.
p. 64 Statement as to the less relief of the older drift is not correct since much of this area is exceedingly rough. Why is the history begun backward? Note statement about age of the gray drift.
p. 70 Table here is repeated from p. 43
p. 75 Is the gorge all trap? Does not check my data
p. 76 There is a definite lack of characteristic lake outlet topography at some of the points suggested as outlets. Abrupt recession of lake level on retreat of ice from base of St. Croix moraine is not mentioned nor are the terraces of the Dalles of which the highest is about 920 feet. The fact that glacial drainage must have aggraded the Barrens to the level of the terraces is not mentioned. See topographic map of Burnette County. Since the clays extend west of Grantsburg where was the gray ice border then? Discussion of possible northeastern extension of the lake into area devoid of clays is not sufficient. Derivation of clays seems not to be considered explicitly.
p. 78 Some of the overlying sands are probably of dune origin
p. 80 Survival of the rough topography might be due to short time of covering by lake waters plus survival of ice blocks in kettles
p. 81 The Bluff and Karlsborg Ridge seem much like a terminal moraine. The discussion of the large ice block seems rather obscure. Entire discussion of possible history is not adequate as a number of possibilities are not mentioned.
p. 83 The comments on Leverttts work seem to be well sustained but the exterpolation of the rate and direction of tilting seems hazardous.

Entire report contains much valuable information but leaves a feeling that it is not complete and that it is devoted too much to just proving the existence of a Barrens lake without correlating it with different interpretaions of the glacial history.

Nov. 27, 1931

Prof. W. S. Cooper,
Dept. of Botany,
University of Minnesota,
Minneapolis, Minnesota

Dear Dr. Cooper:

Your letter of the 24th and enclosed manuscript arrived yesterday. I wish to thank you for sending this to me.

The first paper is now in the hands of Dr. Hansell and I hope to be able to return it to you in a few days. He is in the field near here at present.

The expected furor broke forth on schedule with regard to the first paper. I then dug into the literature for references in re stagnation and the other fundamental differences of continental and mountain glaciers and interviewed several of the staff and a number of students from this and other schools. All evidence I could find supported my idea and yours, that stagnation had never been seriously stressed and that too many of the current ideas are based on mountain glaciers. However, I will write more fully on these matters when I return the first paper. Enclosed with it will be some quotations from leading glacialists.

Very truly yours,

Lecturer in Geology

I note that you have some differentinterpretations from those of Hansell. Turning to my CONFIDENTIAL criticism of his doctors thesis I note some of the same things. I said "The main fault of the report is the fact that it is obviously devoted to an attempt to prove a single hypothesis and not to a gneral discussion of possible explanations." "Such statments as 'good beach gravel' or'like-sized gravel' convey little meaning." "The assignment of the summit flats to lake planation in view of the lack of boulder pavements and of well defined lake beaches seems preposterous. The possibility that these flats may be either (a) delta tops, or (b) remnants of pitted outwash seems not to have been considered." "In general the description of the physical character of the sediments is most inadequate." The report "seems to be a record of rather casual observations made in the course of the magnetic survey, many of them by others than the suthor of whom many seem to have been quite inexperienced geologist." The conditions under which the field work was done and the report written were not such as to give the conclusions much weight. Therefore, I am very glad to see an alternative view presented. I have myself seen only a small part of the area in dispute and I was not invited to go on the field conference with Dr. Alden in the spring of 1930.

Nevertheless, I wish to make a few suggestions. I think it is well established that during the Wisconsin the source of ice shifted steadily to the west. In fact a recent paper of Leighton's gives geographic names tothe subdivisions which are based on this fact. The Early Wisconsin (Leverettsubstages 1 and 2) came from well to the east and formed no Green Bay Lobe that is now preserved. The Middle Wisconsin (substage 3, the Gray Drift of eastern Wisconsin) came from farther west and made the recognized Green Bay Lobe. My work in Langlade County for the Wisconsin Road survey had just reached the interlobate angle when Mr. Bean had it discontinued when he left the Highway Commission. (Since then I have worked a little with students to help me.) I there found definite evidence that the Green Bay and Langlade lobes are the same age for northern waters entered the southern area before many ice blocks had melted. The Late Wisconsin (substage 4) is decidedly younger (this is the Red Drift of eastern Wisconsin) for it is separated from the previous drift by the Forest Bed on which Mr. L. R. Wilson is now publishing a report. This drift came from north of Lake Superior although not unlikely farther east than did the Patricia drift of Minnesota. I could not check Leverett in his location of the "moraine" of this substage in Vilas County Wisconsin unless it is superimposed on an older terminal (Winegar Moraine). I have often wondered if possibly your Patrician is not really Substage 4 instead of 3. If so I would run the line of the border of this invasion along the moraine from Hudson northeast to the eastern part of Bayfield County, thence east to where Leverettt has it. The brush country of northern Wisconsin is so different from the plains country that it proved difficult for Leverett to get used to it and it may well be that he is mistaken in some of the conclusions. It is a bad country to work in for anyone. However, this change in substage correlations would not affect your work materially.

However, I have also wondered about Leverett's readvance of the Superior Lobe which he makes equal to the Keewatin advance in time. I have ventured to suggest to Hansell that the Patrician ice might have crossed the Lake Superior basin on top of stagnant Labradorian ice. Might this be ture and might the buried ice have come to life again after being uncovered or should this be regarded like Weidman's idea that the last glacier crossed on a frozen interglacial lake?

On thing that is not new with me for it was suggedted long ago by Hobbs is the limitation of active movement in a continental glacier to a zone near the border. This would explain some of the marginal irregularities which seem to have been determined by very small topographic differences.

Dec. 3, 1931

Dr. W. S. Cooper,
Dept. of Botany,
University of Minnesota,
Minneapolis, Minnesota

Dear Dr. Cooper:

Enclosed please find the first manuscript you sent me together with a letter from Mr. Bean in regard to it and some extracts from the literature in regard to ice stagnation. My personal conclusion from this study is that Russell may have had some ideas of stagnation during wastage of continental glaciers. He certainly had seen the stagnant portions of the Malispina glacier much more thoroughly than anyone else that I know of. If so, he died before the publication of the report which I read and whatever definite ideas he had on the subject perished before getting into print. I recall very well that Martin also mentioned the possibility of stagnation but the best evidence that he did not regard it as a usual thing for continental glaciers or a distinctive character of them is the fact that although I worked with him for several years my 1927 Outline of Glacial Geology is just as vague on the subject as most books of earlier date are. Bean declares that Martin recognized stagnation as an important phenomenon of continental glaciers. It is certainly true that he repeatedly urged that the ice melted back to where it was thickest and that Leverett had failed to take this into account. I certainly would be a good idea to get his reaction on your paper. Address Col. Lawrence Martin, Division of maps, Library of Congress, Washington, D. C. So far as I can recall, however, Alden who was a student of Chamberlin (Martin was a follower of his brother-in-law, Tarr), never mentioned stagnation. I can find no reference to it in his report on Wisconsin. I doubt seriously that his official position on the Government Survey would allow him to express any of his personal ideas. Witness the fact that although he first called my attention to the poor evidence of peneplains in the erosional history of western Wisconsin his final report follows exactly the statments made in earlier U. S. G. S. publications. (Hope I am right in this; Martin also did the same thing and I might have got them mixed after 24 years;)

With regard to the Outline of Glacial Geology. I am entirely out of copies at present and as a new, much revised edition with illustrations is well under way I hate to spend the time to sort out any more sets from the few sheets left particularly as I would have to make new stencils to replace certain missing pages. The new edition will present things in a considerably different way and I would appreciate it if you can wait until spring for a copy. May I trace the esker picture to make a block diagram from it? The new outline will have only line drawings. I am keeping the esker picture and the map to return with the other ms.

Turning to the second part of the ms. I was very glad to read your clear explanations of the glacial history of the Minnesota area. I was especially interested in your lucid disposition of Father Retzik's claims of contemporaneous Patrician and Kewatin ice. Your description inspires confidence and there are only a few suggestions which I can make.

I could find no mention of the fact that Berkey thought that the clays at Granstburg were overlain by till. Leverett still harks back to this old observation which later test pitting by Hansell and Aldrich failed to prove. They did not dig at the designated spot but suggested that Leverett and Weidman mistook stream wash for till.

The point about the source of the clays is well taken. I had made a similar criticism myself. Mr. Wilson who worked on his bachelors thesis near Solon Springs thinks that red lake clays underlie the pitted outwash or pitted delta plain of that region. He recently found more varved clays but I am not clear just what their relation is to the ones he found under the peat in the bottoms of the kettles. I always felt from the character of the Upper St. Croix Lake valley that drainage from Lake Duluth must surely have come through there during the maximum if not after the time of the Grantsburg (Barrens) Lake. However, my observations have been limited in the extreme.

I might suggest a compromise view on the "high level beaches". May these not in part be real beaches but formed in holes which melted in the stagnant Patricia ice? Some of the photographs (there are not many which were taken by the Mineral Land Survey) I have examined do really look like beach stratification and assortment and others are indecisive if not negative. Of course, the local level summits and the local clay pockets are indecisive. I have always felt that with the experience I gained in the three years in the north for Mr. Bean I could add materially to the solution of this problem but as noted before I was not asked for an opinion.

Almost the only criticism I found of the paper which affects the manner of presentation was the use in some places of "sometimes" instead of "locally" or "in some places". I have fallen into this common error many times myself!

I have given the ms. to Mr. Aldrich to read and he will then give it to Dr. Hansell when he comes in from the field again which may be any day now. After that I understand that you wish Mr. Bean to read it. I have not shown my written criticisms to them and my copies will be kept in my personal files only. After hearing their reaction I may have reason to write further when I return the paper. I have placed your name on the list for a copy of the new Outline.

I am certainly glad to have seen your manuscript and hope that I have been able to be of some help to you with it. I also hope that my days of usefulness in the Pleistocene of northern Wisconsin are not over despite the withdrawal of financial support for investigations. Mr. Wilson is working on his doctorate on Pleistocene and early Recent flora and (to some extent) fauna. The report on the Forest Bed will soonn be in print I hope. I also have two students writing theses on Pleistocene geology, so you can see that something is still being done.

Very truly yours,

Lecturer in Geology

UNIVERSITY OF MINNESOTA
COLLEGE OF SCIENCE, LITERATURE, AND THE ARTS
MINNEAPOLIS

DEPARTMENT OF BOTANY

Nov. 24, 1931.

Dr. F. T. Thwaites,
Department of Geology,
University of Wisconsin,
Madison, Wisconsin.

Dear Dr. Thwaites:

I am sending you herewith a copy of the manuscript of my paper on the postglacial history of the upper Mississippi as far as it is completed. The part finished is that which will be of greatest interest to you and your colleagues as it includes a discussion of the Grantsburg Lake which Hansell and I are sharing.

I will be very grateful if you and Hansell and the others, if they desire, will read it and criticize it in any way you desire.

Sincerely yours,

W. S. Cooper

W. S. Cooper, Professor,
Department of Botany

WSC:EMF

UNIVERSITY OF MINNESOTA
COLLEGE OF SCIENCE, LITERATURE, AND THE ARTS
MINNEAPOLIS

DEPARTMENT OF BOTANY

Nov. 12, 1931.

Dr. F. T. Thwaites,
Department of Geology,
University of Wisconsin,
Madison, Wisconsin:

Dear Dr. Thwaites:

Your letter was of very great interest, and it is exceedingly gratifying and encouraging to find that you have been thinking along the same lines. The exact parallel is certainly a strange coincidence. I assure you that I had no knowledge that you were working in the same direction. Flint's publications gave me the idea at first, but I had no intention of writing a paper on the subject until last August. The idea came to me quite suddenly and I finished the whole work in not more than three weeks. I am not sure whether I told you that Leverett has read the paper and is decidedly more receptive than I had expected. There are two or three references to the literature in your letter which I have not seen and am very glad to have them. I ~~would~~ will be very glad to go ahead and publish and bear the brunt of the attack. I will be very glad, however, to make it plain that you have been developing the same subject at the same time if you desire it.

I have been carrying on a voluminous correspondence with Father Retzek and have been in the field with him one day. His work seems to me a strange combination of intense enthusiasm, excellent observation and somewhat wild conclusions. I hope that sometime I can talk the matter over with you more in detail.

When I was in Madison you gave me a copy of those portions of your outline dealing with outwash and methods of using the aneroid barometer. I will be very grateful if you can spare me a complete set.

Just now I am finishing the preliminary writeup of the material dealing with the lake which is the joint property of Hansell and myself. Its existence is absolutely demonstrated on this side of the state boundary, but I cannot put my finger on its exact northern boundaries. I will send this portion of my manuscript for the consideration of your group as soon as it is ready.

Sincerely yours,

William S. Cooper

W. S. Cooper, Professor,
Department of Botany

WSC:EMF

Nov. 7, 1931

Dr. William S. Cooper,
Dept. of Botany,
University of Minnesota,
Minneapolis, Minnesota

Dear Dr. Cooper:

Your letter of the second and enclosed manuscript are at hand and I wish to thank you for sending the latter to me. I have read it and will pass it on to Messers Bean, Aldrich, and Hansoll, the last of whom is still in the field.

As soon as I began to read your paper I had an uncanny feeling wonderingg how the ideas which I had just begun to entertain when I started to revise the Outline of Glacial Geology in July had been communicated to you! This manuscript has never been off my desk in my study at home and so far as I can recall I have never discussed the matter of stagnation of continental ice sheets with anyone but Flint and a few of my students. It appears that our minds have run on exactly parallel tracks so that we arrived at the same conclusions at essentially the same time. I understand that such a happening is not uncommon for instance many great inventions seem to have been made almost simultaneously by more than one person. The language is different but the basic ideas and the manner of approach to the problem are exactly the same. I feel that we surely must be on the right track. In the new edition of the Outline (I think you have a copy of the old one, if not please advise me) I stress the stagnation of continental ice sheets during melting as one of their fundamental characteristics inwhich they differ from high gradient rivers of ice in the mountains. This theory has a vital effect on the interpretation of features ascribed to wholesale glacial erosion by continental glaciers. See Martin's ideas on the Great Lakes.

I did not begin to seriously entertain the idea of wholesale stagnation of continental ice sheets until this year. The two weeks spent with some students in Waushara County, Wisconsin in September were thefirst opportunity to try out the ideas in the field (the State Survey has withdrawn all support from me since 1928). We found that the growth of the Middle Wisconsin ice made drumlins, a sure sign of moving ice you do not mention. During recession extensive terraces of pitted outwash were formed west of the ice front between it and the highlands to the west. The edges of the two great series of terraces were called steps for they are not moraines. The western one the boys called "The High Plains" and its step "The Break of the Plains" both terms in allusion to well known features they had studied in Physiography of the United States. The "High Plains" have outliers on the "Low Plains". These lower terraces drop off to the Lowlands in a similar irregular escarpment without anything but mere scraps of till moraine. The outliers I interpret as fillings of holes which melted through the ice. True there are boulders and till on some of the minor escarpments but very rarely did we find any true moraine rising above the pitted plain to the west east of the Second or Milton Moraine. The great Mountain Morainic series, the product of a distinct readvance where seen to the north and the equivalent of Alden's Waupun, Rush Lake and St. Anna Moraines is buried under ground moraine of the Late Wisconsin in that area. I can back decidedly strengthened in the belief I had reached from theoretical considerations.

Other evidences of stagnation I have met with are standing water deposits on the tops of hills which had melted through the ice. Crevasse fillings have also been noted in the same position.

I am glad to see you take the position you do about eskers showing stagnation. It is interesting to note that that requirement was one of the things which led Trowbridge to attack the usual theory of esker formation and propose one which is just as hard to reconcile with ice in motion! Reference, Trowbridge, A. C., The formation of eskers: Iowa Acad. Sci., Proc., vol. 21, pp. 211-218, 1914.

With regard to von Bülow's theory Russell long ago stressed the influence of debris in causing stagnation. Russell, I. C., The influence of debris on the flow of glaciers: Jour. Geology, vol. 3, pp. 823-832, 1895. Would not this support the idea?

Judging from the reception given to Flint's moderate proposals I advise you to be prepared for bitter criticism and scorn from some of the old timers. They will raiseall sorts of objections most of which will not stand analysis, however. I would not pay much attention to them. But then they will get over it in time. Every new idea gets just the same reception. Hitchcock is said to have been made to give up the first suggestion of continental glaciation! Someone has to put forward the new ideas and you are in a position where the conservatives cannot harm you. I was removed from study of the Cambrian because I disagreed with one of the old timers but you being outside of the field of geology cannot be reached by any such move. I wish to offer my best wishes in the coming argument. Too long have we thought of continental glaciers as simply gigantic mountain glaciers.

Other points in favor of stagnation have been brought out by Father Retzik in his explanation of the Grantsburg Lobe. His English is terrible but I feel that his ideas are sound

I have not decided when I will publish the Outline of Glacial Geology so please do not refrain from publishing your paper on that account. Someone has to fire the first gun in the battle. Flint has been rather too conservative to realy start things going. The inferences to be drawn from the theory of stagnation are far reaching and the matter must be fought out sometime. Let's start now!

I do not think that Stehr's paper would help much. It is summarized in Hansell's report. To borrow it you can get your librarian write to the University Library here. But I scarcely think it worth the effort for your problem as it deals with a small area far to the northeast in the Lake Superior basin and was written while he was a senior.

Very truly yours,

F. T. Thwaites, Lecturer in Geology

UNIVERSITY OF MINNESOTA
COLLEGE OF SCIENCE, LITERATURE, AND THE ARTS
MINNEAPOLIS

DEPARTMENT OF BOTANY

Nov. 2, 1931.

Dr. F. T. Thwaites,
Department of Geology,
University of Wisconsin,
Madison, Wisconsin.

Dear Dr. Thwaites:

I have boldly plunged into the sea of glacial conflict with a paper of general nature dealing with the nature of the process of decline in a continental glacier. I have sent it to a number of persons, who have been thinking along these lines, for comment and criticism, and I will be very grateful for yours if you are willing to read the paper. I have already written Hansell and he states that he will be in Madison about this time. I am, therefore, sending you herewith a copy of the paper and will be very grateful if you and he and Aldrich and Dr. Bean will look it over and criticise it in any way you desire. You may feel free to be just as destructive as you wish. I desire to find the weak points in it before publication rather than after.

I find a reference in Hansell's bibliography to an unpublished paper, a B.A. thesis by R. A. Stehr, entitled "A Study of Pro-glacial Lake Deposits," 1924. I have been much concerned with such deposits in my recent work around here and I am wondering if there is anything of value in this paper. If you think there is, would it be possible for me to borrow a copy?

Sincerely yours,

William S. Cooper

W. S. Cooper, Professor,
Department of Botany

WSC:EMF

OPINIONS IN RE STAGNATION OF CONTINENTAL GLACIERS

W. B. Wright, The Quaternary Ice Age., p. 32, 1914

This condition is that there should be active motion of the glacier up to the decaying margin, so that sufficient material is carried forward to this line and deposited there. Dead ice can never make an endmoraine no matter how long it perisits.

p. 39 It is obvious, however, that the bedding of any deposits formed thus in subglacial channels at a little distance from the ddge of the ice sheet would soon be destroyed by the motion of the ice—— We may, therefore, fairly conclude that they (eskers) are more or less marginal deposits; perhaps entirely marginal

Hobbs, W. H., The Pleistocene glaciation of North America viewed in the light of our knowledge of existing continental glaciers: An. Geogr. Soc., Bull., vol. 43, p. 641, 1911 p. 642

——it becomes necessary to recast all the traditional notions concerning continental glaciers, modelled as these have been upon the Alpine glacier type.
p. 656 stresses motion in outer zone but does not mention stagnation.

The abraded rock pavements may all be explained by the grinding and plucking action under this outer zone, which during successive advances and ~~tstreats~~ occupies in turn all portions of the area.

Trowbridge, A. C., The formation of eskers: Iowa Acad. Sic., Trans, vol. 21, p. 215-216, 1914

It has been a source of wonder to all holders of the subglacial theory that esker ridges could avoid destruction by the ice constantly moving over them. That wonder is well founded. The explanation has been that eskers were made and preserved only in the last stages of an ice sheet and near its border, where movement was slight, and the weight of the ice was not too great. But eskers which are 50 or 100 miles long could hardly be supposed to have existed under these conditions——. ——the dim vision of a thick ice sheet moving over a steep, narrow, crooked ridge of non-resistant material without destroying it;——

Chamberlin, T. C., and Salisbury, R. D., Geology, vol. 3, p. 376, 1906

Deposits of streams in other positions (than near edge of the ice) would stand much less chance of developing distinct ridges before being destroyed by the movement of the ice.

Salisbury, R. D., Glacial geology of New Jersey: New Jersey Geol. Survey, Vol. 5, p. 59, 1902

detritus in the ice decreases its mobility (ref. to Russell), and decreases in mobility may go so far as to practically destroy motion. In this case the bottom, debris-charged part of the ice becomes stagnant, or nearly so, while the clearer part above overrides it. In this case the surface beneath the glacier suffers little wear.

p. 86 <u>When the ice became stagnant.</u> ——During the dissipation of an ice sheet considerable masses of ice appear to have lost, or to have essentially lost, their motion. Such isolated bodies of ice doubtless preserved their motion, in many cases at least, for a time. But when they became small, or when the local topography was unfavorable to motion, they became stagnant, and all the drift they held was let down on the surface as the ice melted.

Tarr, R. S., and Martin, Lawrence, Alaskan glacier studies, p. 41, 1914

Mention that part of Malispina glacier is stagnant. This must have first been observed by Russell who made a thorough exploration of it.

Tarr, R. S., and Martin, Lawrence, College physiography, p. 209, 1914

In its origin, nature of movement, and work performed, the ice sheet is, however, so like a glacier that, in spite of its size and other differences, it is properly to be classed as a great glacier.

p. 272

eskers cannot develop, at least not in a very perfect form, in ice that has much movement, for the tunnels would soon be closed——. The most favourable position for esker development is under the thin, stagnant front of a glacier of beneath a detached ice block, conditions that were common along the front of the receeding ice sheet.

Russell, I. C., The surface geology of portions of Menomonie, Dickinson, and Iron Counties, Michigan: Michigan Geol. Survey, Rept. 1906, 1907

p. 57

The suggestion which presents itself in this connection is that eskers are formed by streams flowing in channels of a stagnant ice sheet——.

p. 60 in reference to moulin deposits

This might happen, especially in a stagnant ice sheet——.

These quotations not checked with originals.

Russell, Mich Geol Rept 1906 p 57

The suggestion which presents itself in this connection is that eskers are formed by streams flowing in channels in or on a stagnant ice sheet - - -

p 60 in ref to moulin deposits

This might happen especially in a stagnant ice-sheet - -

I^st A. Sci vol 21

C & S III p 376

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Salisbury NJ vol 5, p 59

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Alaskan Gl Studies p 47 extensive stagnant Malaspina ice

TCM coll Ply p 204

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p 216, "the slow motion of a thick ice sheet moving over a steep, narrow, crooked ridge of non-resistant material without destroying it." — —

Chamberlin
Check in J.G. 2, 525-531

Hours. Rising hour, unless otherwise noted, will be 5:45 A. M. Breakfast, 6:15 Work, or start for work, not later than 7:00 No lunch will be served but those working near camp may leave lunches there and come in to eat them. Supper 6:30 P. M. Evenings will be devoted to study and work on inking maps, adjusting aneroid readings, checking notes, looking up information for next day, etc. etc. It is generally been hard to get enough time to do all the required office work at the Lake. Spare time in the evenings is generally evidence of slighting the work and students who have had such time have almost invariably turned in very poor maps. Work will be allowed until midnight but after 11:00 PLEASE BE AS QUIET AS POSSIBLE for others will want to sleep.

Care of instruments. Your $3.00 fee does not cover loss of or accidental damage to instruments. Please observe all cautions very carefully; better be safe than sorry. Do not set up table for telescopic instrument on pavement or travelled part of a main road. Do not leave go of alidade until table is clamped. Do not go away from table with alidade on it or from level. Look out for things falling from trains and for suction under fast trains. Put striding level firmly on its post for carrying and if possible keep a handkerchief below it. Look out for loosing open sight alidades for the sights open and pull them out of pocket or bootleg. Carry them in knapsack. Dont jump fences; go under or through them. Do not jump off cliffs. Check up number of instruments whenever you go through a fence and whenever you do some work. Follow definite straight lines so that if you do leave or drop something you will know exactly where to look for it. The woods are full of open sight alidades lost through neglect of above precautions. Set up tripods carefully on the rocks. Some regulations given above may seem unnecessary at times but follow them regardless; the unexpected can always happen. A. B. C. = always be careful. Whenever you are given an instrument first see that it is in adjustment and that you understand just how to use it. Ask questions before leaving for field.

Health. If it is warm when the trip starts do not be misled into thinking that summer has come. Follow directions about clothes and bedding implicitly. Some bad weather is almost certain. Avoid wet feet and chills. Change wet shoes and clothes IMMEDIATELY on return to camp. Try to keep as well washed as possible although shaving can be ommited part of the time. Health while working in the open is generally better than while at Madison although the change in hours may upset some for a day or so at first. The diet is selected carefully to try to minimize this. The main danger is that when working alone you might fall and disable a leg or foot. Be careful on bad ground especially where there is any snow. Snow in the loose rocks is exceedingly treacherous. Above all DO NOT STRAY FROM YOUR AREA; leave it only when absolutely necessary. There is no reason to get lost; trust your compass. Do not take shortcuts across the talus and cliffs for they take too long and are dangerous after dark. Carry matches and if you should be disabled use every effort to light a smudge in daylight or a fire at night. DO NOT STAY OUT AFTER DARK alone and cause others anxiety. Let's not have an accidnet as there has been none to date. A. B. C.

Private property. In some places you will have to trespass on private property. Some students in other courses and from other universities have been careless and have angered the natives. Please do not break any fences or open any gates. Do not work on Sunday near to houses or on main-travelled roads. Avoid contact with natives so far as possible. Remember that Northwestern trains run on left hand track; KEEP OFF TRACKS WHEN A TRAIN IS PASSING!

Park rules. The rules of the State Park forbid starting fires, cutting li. timber, and bringing intoxicants or firearms into the Park. Please obey them

W B Wright QIA 32 1914

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maps of glacial recession unusually

Dublin A geog Soc B 43 - 641 1911

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Stresses motion only in outer zone p 656 but fails to note stagnation

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Jan. 28, 1932

Dr. William S. Cooper,
The Pontchartrain Hotel,
St. Charles at Josephine,
New Orleans, Lousiana

Dear Dr. Cooper:

Reply to yours of the 19th has been delayed thinking that I would get a blueprint of some diagrams for the new Glacial Geology. However, I have been called out of town for the weekend on water supply business and then follows my exams so decided to write anyway.

The idea of a marginal zone of stagnation is, I feel, not entirely new. If I am correct Russell suggested this long ago. I have drawn two diagrams for the Glacial text. One shows the idea of Hobbs that slope sufficient for motion occurs near the margin of a continental ice sheet only. The other which is drawn on a larger scale, indicates that shearing over hills might and probably would lead to partial or entire stagnation within the very outermost part of this marginal slope. I do not think that the two phenomena are antagonistic. A point in favor of a steep marginal zone is raised by Antevs in his recent paper on Manitoba. He says that since a continental glacier is very high melting takes place only low down on its front thus keeping it steep. His arguments that it is at the same time straight do not impress me, however. He will not admit separation and burial of large ice masses. I feel that field evidence compels us to disagree with him there. Returning to evidence of marginal stagnation, my feeling is that this is a local and temporary phenomenon but is one which is much more likely to take place in continental glaciers than in the mountains. It does not exclude general or regional stagnation due to lack of nourishment. Lack of melting high on the glaciers would not affect the matter since flow would bring the ice down to the angle of repose. I note that I forgot that overloading with debris in the basal and therefore marginal ice would also lead to stagnation. I must fix the diagrams accordingly.

I fail to see that Hobbs point means anything. We could have all possible gradations in amount of snow. Enough might be blow outward to induce active motion and outward growth, a balance might occur with motion still occuring, or there might be so little snow that gravatational flow and lowering of the marginal slope was not made good. It seems to me that this condition would be much more probable than one where flow as maintained during a net loss of ground to the ice. Such a condition, that which normallly occurs in mountain glaciers, would, it seems to me, occur only when warm winds blew against the ice front and not because of diminished snowfall. I have brought out in my text that the occurence of intervals of stagnation is a characteristic feature of continental glaciers because they owe their flow to surface slope and not to inclination of the ground below.

I hope that I have made myself clear on the above, if not please write me again.

Hansell is now working in Arkansas.

Sincerely,

E. LYSLE ASCHAFFENBURG
MANAGING DIRECTOR

ROGER GENERELLY
MANAGER

The Pontchartrain
Apartment Hotel
ST. CHARLES AT JOSEPHINE

NEW ORLEANS

January 19, 1932.

Dr. F. T. Thwaites,
Department of Geology,
University of Wisconsin,
Madison, Wisconsin.

Dear Dr. Thwaites:

or one might say stagnant.

My stagnation paper is lying dormant for a time. I brought it along with the materials which I thought necessary to complete it, but upon thinking it through again, two or three points of doubt cropped up which seem to require further consideration. I would like to state them to you and find what you think of them.

When I wrote the first draft, I felt that a marginal stagnant zone, with connection with the main ice sheet still maintained, must be an impossibility. Andersen's paper in a recent journal of geology assumes this as a possibility, apparently as a matter of course without giving it any discussion. On thinking the matter over, it has seemed that perhaps he and others are right and that it is possible for the margin of an active ice sheet to become stagnant for a considerable distance back. There would of course have to be shearing of the moving part over the stagnant part. This need not take place along one plane, but might be distributed through quite a large zone.

The second point is derived from consideration of the work of Hobbs. His theory involving the blowing outward of the snow and its accumulation near the margin would suggest that a steep marginal slope might be maintained during retreat, thus permitting motion to continue in the marginal portion. Hobbs of course assumes stagnation in the interior. His facts are derived from a study of Greenland and Antarctica, both of which differ from the pleistocene ice sheets in having mountain rims. There is thus no necessary reason why the pleistocene ice sheets should have maintained steep marginal faces merely because the present ice sheets do. The mountain rim may be the differentiating factor.

What do you think of these two points and their bearing on the subject of stagnation? You can reach me at the above address until February 10th.

Sincerely yours,

William S. Cooper

E. LYSLE ASCHAFFENBURG
MANAGING DIRECTOR

ROGER GENERELLY
MANAGER

The Pontchartrain
Apartment Hotel
ST. CHARLES AT JOSEPHINE

NEW ORLEANS

February 3, 1932.

Dr. F. T. Thwaites,
Department of Geology,
University of Wisconsin,
Madison, Wisconsin.

Dear Dr. Thwaites:

Thank you for your recent letter. Your suggestions are valuable and I note with interest that you mention a recent paper by Antevs on Manitoba. This is entirely unknown to me and is an additional reason for letting the paper lie dormant until I can take it up again in connection with adequate library facilities. I am sorry to have the delay, but I want to be very sure of my ground before I close up the matter entirely.

I am shortly going up into Arkansas and will try to get into communication with Hansell.

Sincerely yours,

William S. Cooper