

Subject: [Fwd: B25012 Decision Letter]
 Date: Wed, 09 Jan 2002 22:14:20 -0500
 From: Paul Bierman <pbierman@zoo.uvm.edu>
 Organization: Univ. of Vermont
 To: "Karen L. Jennings" <kjennings@zoo.uvm.edu>

yee hawwww...whopeeeeeeee

you're in!

give me an email or call and let's figure what to do next!

P.

editing@geosociety.org wrote:

>

> Dear Paul,

>

> I have now received 2 reviews of your manuscript, entitled "Timing and
 > style of deposition on humid-temperate fans, Vermont, USA" [Paper
 > #B25012], and a recommendation from the Associate Editor, Marith
 > Reheis, which I enclose for your reference. I am quite backed up here, so
 > your manuscript has languished on my virtual desk for a few weeks now,
 > but I am happy to report that the news is good.

>

> Based on these materials and my own reading of the manuscript, I find
 > that the contents of your manuscript certainly merit publication in THE
 > GEOLOGICAL SOCIETY OF AMERICA BULLETIN after revision. I
 > encourage you to submit a suitably revised version of your manuscript.
 > Please include a detailed response to the reviewers' and Associate
 > Editor's comments with your revision.

>

> The Associate Editor, Marith Reheis, has summarized the reviewers'
 > comments well. There is agreement that a beefed-up discussion would
 > be welcome; however (you knew this was coming), please attempt to hold
 > the overall length of the manuscript more or less constant. In addition,
 > please address concerns about figure/table integration and use of the
 > Data Repository.

>

> Please submit your revised manuscript by March 10, 2002. If you do not
 > plan to submit a revision, or if you cannot do so in the time allotted, I
 > would be grateful if you could let me know as soon as possible.

>

> When you are ready to submit your revision, please use the link below.

>

> [http://gsa-bulletin.allentrack.net/cgi-bin/main.plex?form_type=revise_ms_](http://gsa-bulletin.allentrack.net/cgi-bin/main.plex?form_type=revise_ms_splash&ms_id=143&ms_rev_no=0&j_id=1&ms_id_key=joH5tKFn8yYVY&p_id=7889)
 > [splash&ms_id=143&ms_rev_no=0&j_id=1&ms_id_key=joH5tKFn8yYVY&](http://gsa-bulletin.allentrack.net/cgi-bin/main.plex?form_type=revise_ms_splash&ms_id=143&ms_rev_no=0&j_id=1&ms_id_key=joH5tKFn8yYVY&p_id=7889)
 > [p_id=7889](http://gsa-bulletin.allentrack.net/cgi-bin/main.plex?form_type=revise_ms_splash&ms_id=143&ms_rev_no=0&j_id=1&ms_id_key=joH5tKFn8yYVY&p_id=7889)

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>

> I thank you for submitting your best work to THE GEOLOGICAL SOCIETY
 > OF AMERICA BULLETIN.

>

> Sincerely,

>

> Allen Glazner

> Editor

> THE GEOLOGICAL SOCIETY OF AMERICA BULLETIN

>

>

>

> -----
 > Associate Editor Evaluations:

> New Data: Agree Completely

> Conclusions Supported: Agree Somewhat

> Data Separate: Agree Completely

> Previously Published: No

> Broad Interest: Agree Somewhat

> Appropriate: Agree Somewhat

> References: Agree Completely

> Quantitative Evaluation: Neither Agree nor Disagree

> Clearly Written: Agree Completely

> Title Appropriate: Agree Completely

> Organization: Agree Somewhat

> Condensation: Disagree Somewhat

> Replace Text: Agree Completely

> Illustrations: Yes

> Appendix: Yes

> Data Repository: Yes

> Identified: N/A

> Hard Copy: Yes

> Overall: Very Good

> Acceptance: Acceptable, but requiring moderate revisions

>

> Associate Editor(Remarks to the Author):

>

> Comments from both reviewers support the recommendation that the
 > authors should try to expand their currently rather skimpy discussion to
 > address the broader implications of their study, including such questions
 > as (1) the lack of debris-flow deposits in these fans, (2) the scattered
 > nature of the correspondence between depositional events; (3) the
 > correlation, or lack of it, of stability and deposition events to those in other
 > types of records; (4) should this lead folks to avoid studying fans to obtain
 > records of response to climate change, and (5) can you use the
 > morphology or position of the fans to prospect for other fans that retain
 > good records (e.g., Maidstone wasn't very helpful, with only a ~200-yr
 > record).

>

> As AE, my chief complaint with the format of the paper is the use of tables
 > as substitutes for figure legends (or figures; see my comment #13). It is
 > absolutely unwieldy to have to compare a table with each figure to
 > understand what units are in the figure and will impose stiff restrictions on
 > layout (each corresponding table must be next to its figure). I strongly
 > recommend that the authors (1) put parts A and B of each figure at the top
 > of a page, plotted at the same scale; (2) show where parts A and B
 > intersect (see comment 3 on fig. 4A); (3) reformat the corresponding table
 > so that it's a legend at the bottom of the page. It is also more than a little
 > confusing to have the complexity of different letter abbreviations from
 > figure to figure. Isn't there some way to standardize at least SOME of the

> units, like the gravel or sand beds? I suggest putting the detailed logs
> used as figures into the data repository. Then in the paper, the authors
> could remove all the clast outlines, which really clutters up the figures,
> and use shading etc. to help identify certain lithologies that are repeated
> from figure to figure.
> The following numbered comments are keyed to the red numbers in the
> text. Good luck with revision. And, by the way, feel free to contact me if you
> have questions! (my entry box says--do not reveal your names)--
> Regards, Marith Reheis
>

> 1. (p. 5) This description of digging deeper in each trench needs to be
> clarified. The whole trench? One or more than one spot? By hand or by
> backhoe?
>

> 2. (p. 10 and fig. 4) I don't understand why well-sorted glacial-outwash
> gravel is considered to be a fan deposit, as shown in fig. 4A by the
> dashed line. Furthermore, the text then describes the overlying 0.5 m of
> silt to also be fan deposition. I never heard of well sorted silt being
> something that fans lay down.
>

> 3. (fig. 4 and all subsequent figures) You should show on the logs where
> the top and stem trenches intersect. From DR3C, it appears that 4A and
> 4B should intersect at A and B', but I can't match the stratigraphy across.
> Why not?
>

> 4. (p. 11) I'm not familiar with wet-climate fans, but in the arid West, fan
> surfaces are usually not scoured; the only erosion is in channels. Maybe
> a reference to humid fan processes is needed here to support this.
>

> 5. (p. 10-16 and figures) What is the rationale for the order of discussion?
> Seems more logical to start with the two fans in glacial-valley settings and
> then proceed to the fans on younger terraces, then end with the Maidstone
> fan.
>

> 6. (p. 11) I am concerned about the Maidstone chronology and the age of
> the paleosol. This whole thing hangs on the assumption that the basal
> age on twigs is the correct one. While on the subject, why does the text
> continually use 150 yr B.P. as the basal age when table 1 and the figure
> both give much larger age ranges? But if one accepts that number, then
> this is amazingly fast. It requires deposition of ~4.5 m of sediment and
> then formation of a spodosol (which requires at least ~100 yr to form,
> according to Birkeland), and then burial and formation of another weak
> soil, all in 150 yr. If you really think this is so, it shouldn't be glossed over
> so quickly, and it ought to be included in the main discussion section as
> documentation of VERY fast deposition and soil formation.
>

> 7. (p. 13) The last sentence ascribes the sand and gravel below the Ap in
> the Bristol fan to post-clear-cutting sedimentation. BUT! The youngest
> age control is 3200 yr B.P. just below this unit UG. Why not due to some
> intense storm between 3200 yr and settlement? Sounds like you are
> forcing a correlation to woodcutting.
>

> 8. (p. 14) Why assume that the fan sediment is reworked from upfan?
> Why not from older colluvium on the hillslopes? DR6C shows there is not
> much fan uphill from the trench. Also, the only date that is out of strat.
> order is C8, and it's in the same basal unit as C22, so maybe it's just a
> bad date due to bioturbation or contamination.
>

> 9. (p. 14) I don't know where these soil development ages are coming
> from. The text says from the Maidstone fan, but on face value the A/E/Bs
> sequence in the Maidstone paleosol developed in less than 150 yr.
> Sounds like armwaving.
>

> 10. (p. 16) I wouldn't use the aggradation rates from Hancock fan in this
> discussion. It appears to be constant there from the initiation of the fan
> (which could have been due to terrace stabilization on which the fan is
> built and not to some other event) until settlement time.
>

> 11. (p. 18, 16, and fig. 9) On p. 18 there is a discussion of depositional
> pulses at 6000 yr B.P. on three fans, but on p. 16 you talk about increased
> sedimentation at 3600 yr B.P. and nothing said about 6000. In fact I see
> little evidence of either!
>

> 12. (p. 23) I really think these aggradational pulses are being
> over-interpreted. The ~13 ka pulse is recorded in one fan, as is the 6-4.5
> ka pulse. Bridgewater has a pulse that starts around 3.5 ka but Bristol is
> forming a paleosol then and the others are just chugging along. You have
> a case for the Pleistocene-Holocene transition time and the settlement
> period, and the rest is noise. I'd emphasize the stability periods, which do
> seem to hang together.
>

> 13. (table 8) This table should have data from the Bierman et al., Brown et
> al., and Noren et al. studies added to it, and better yet it should be
> formatted as a figure without all that descriptive text, on a time scale with a
> column for each data source.
>

> Reviewer #1 Evaluations:
> New Data: Agree Completely
> Conclusions Supported: Agree Completely
> Data Separate: Agree Completely
> Previously Published: No
> Broad Interest: Agree Completely
> Appropriate: Disagree Somewhat
> References: Agree Completely
> Quantitative Evaluation: Agree Completely
> Clearly Written: Agree Completely
> Title Appropriate: Agree Completely
> Organization: Agree Completely
> Condensation: Disagree Completely
> Replace Text: Disagree Completely
> Illustrations: No
> Appendix: No
> Data Repository: No
> Identified: N/A
> Hard Copy: N/A
> Overall: Excellent
> Acceptance: Acceptable for publication in the <I>Bulletin</I>, after only
> minor modifications
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> Reviewer #1(Remarks to the Author):
>

> This paper provides by far the most thorough and chronologically detailed
> study of Holocene fans in New England, and the findings have broad

> implications for Eastern North America. Because the fans provide a
> Holocene history of their drainage basins, they have the potential to show
> the effect of climatic change on landscape, an effect that has been studied
> far more extensively in dry climates. The finding of at least some
> synchronicity in regime from fan to fan suggests such an effect is
> significant, although, as might be expected, it appears to be weaker than
> in dry regions - but demonstrating this with a good chronology is itself a
> major contribution. The sedimentologic results raise intriguing
> questions, and should serve to stimulate much related research. For
> example, the lack of debris-flow deposits is in sharp contrast to the
> dominance of such deposits in small fans in the Appalachians south of
> the glacial border. Might the "paraglacial" setting of the Vermont fans be at
> least partly responsible for differences between the two types of fans? Or
> are the differences due to the difference in modern climate?
>
> The data collection was well planned and rigorously executed. The
> number of high-quality radiocarbon dates is spectacular. The paper is
> well organized, written, and illustrated. About the only criticism I have is
> that some of the lettering on the figures is a bit small. I also question
> whether specific figures in the Data Repository should be referenced. As I
> understand it, the paper should stand by itself.
>
> There seems to be a contradiction between the last phrase of the
> Abstract, "...most episodes of aggradation or scour in the Holocene
> cannot be correlated between fans" and the phrase in the Conclusions
> that "Simultaneous periods of increased aggradation on multiple fans
> suggest..." As I understand it, in the first phrase you are referring to
> individual events. This should be made clear.
>
> One misstatement that needs correction - on p. 20, the authors state that
> "Fans in Vermont are older than those in Virginia." Actually, there are many
> fans in Virginia much older than those in Vermont. To make this a true
> statement, it should be changed to "...than those studied by Kochel and
> Johnston (1984) in Virginia."
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> Reviewer #2 Evaluations:
> New Data: Agree Somewhat
> Conclusions Supported: Agree Completely
> Data Separate: Agree Somewhat
> Previously Published: No
> Broad Interest: Agree Completely
> Appropriate: Disagree Completely
> References: Agree Completely
> Quantitative Evaluation: Agree Completely
> Clearly Written: Agree Somewhat
> Title Appropriate: Agree Completely
> Organization: Agree Somewhat
> Condensation: Agree Somewhat
> Replace Text: Agree Somewhat
> Illustrations: Yes
> Appendix: Yes
> Data Repository: Yes
> Identified: N/A
> Hard Copy: N/A
> Overall: Very Good
> Acceptance: Acceptable, but requiring moderate revisions
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> Reviewer #2(Remarks to the Author):
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> Comments keyed to numbers in the manuscript margin
> 1. Any need to emphasize base-level control? The steep, freshly
> deglaciated slopes began to erode without "knowing" about base level,
> right? Until vegetation became established, erosion processes typical of
> construction sites and mass movements likely removed sediment at a
> rapid clip, producing discontinuous gullies that coalesced into channels,
> meeting headward-cutting stream systems somewhere in between.
> 2. What is wonderful about the fans you've selected is that they record
> catchment events with some clarity. A river runs past most alluvial fans in
> moist areas and past some in arid zones, nipping at the stratigraphic
> record and making it likely that much of the sediment delivered from the
> basin bypasses the fan area. The reader needs to be reminded that you
> systems are "special".
> 3. Do you have any estimates of sediment yields during these events?
> Even informal estimates provide a valuable context for discussions of the
> significance of extreme events, and for categorizing the "aggradation"
> rates you report later.
> 4. Do you mean (Figure 3; Data Repository File DR1). Somewhere here
> you should formally signal the reader about references to these data
> repositories!
> 5. I'd be a little more cautious! Not all fans in Vermont have the attributes
> of those you trenched. Other fans likely show mainly a record of erosional
> reworking as the feeding drainage swings back and forth across the fan.
> 6. The pollen record and most of the dating I know suggests that
> vegetation does not appear immediately after deglaciation. The
> pollen-barren zone above refusal in most cores records this time interval,
> which likely had a duration of at least several hundreds to low thousands
> of years. I don't think that Davis and Jacobson meant to say that thick
> forests appeared immediately. Don't they also discuss a hemlock blight
> that removed the species from New England for thousands of years?
> 7. What sorts of deposits are there upstream in these small catchments
> covered with glacial sediment? Are there terraces? Older fans?
> 8. Is it possible to interpret the old W10 age in terms of process? I'd not
> use "systematic" without more data.
> 9. Isn't it traditional to describe large gravel as "coarse".
> 10. Why do you say immediately when your organic ages are ~1700 years
> younger than the age of deglaciation you give earlier. An appeal to
> process or different wording would sound better here.
> 11. I don't quite grasp your logic here, but the degree to which your fan
> channel (if there is one) incises between depositional events will help
> determine the size of subsequent events that get recorded.
> 12. You should be more cautious about how you use some of this soil
> terminology and cite a source or two to be safe. In parts of southern
> Vermont, inceptisols that have a beginning E-horizon are found beneath
> northern hardwood vegetation, though I suppose they might have
> developed beneath conifers. Designating a profile as a spodosol
> requires, strictly speaking, considerable extractive chemistry for Al, etc.
> E-horizons are leached, by definition! Are your B-horizons Bc or Bw??
> Your soil colors don't seem that red. A quick review of Birkeland would
> help formalize your use of soil terminology here and elsewhere in the
> manuscript.
> 13. Fan stratigraphy is inherently discontinuous, but I have no reason to
> believe that your interpretations aren't reasonable! An interpretation is just
> that, guided by the data and the models you are using.
> 14. It seems to me that these colors are developing at mighty rapid rates.
> Do you envision the fan as continuously forested? Is it possible that the

> oxidized colors are mottling driven by "perched" groundwater of the time?
 > I would have thought that a good color B might take a couple of thousand
 > years to develop, at least in till. Your radiocarbon ages give you good
 > control on rates.

> 15. This is interesting and should help guide your model of deposition.
 > Does the gravel represent traction load and the sand material in
 > suspension? What do your paleo or modern observations tell you about
 > channel depths on these fans? Does gravel only spread onto a fan when
 > the channel becomes blocked or when it has become filled between
 > major events? What I am trying to get at here is how you think the process
 > works, since that is central to your argument about recording of big
 > storms vs small storms and relating modern observations to what you
 > find preserved in the record.

> 16. This figure serves two ends: correlating periods of aggradation and
 > documenting their rates. I like the former, but suspect that there must be
 > some way to portray the latter on a unit area/thickness basis. The fans
 > are of such different size that strict volume/time seems a limited way to
 > portray aggradation. I'd also expect that the delivery of sediment stripped
 > by a big regional event would record vaguely comparable unit erosion
 > rates, given what I presume is minimal near-channel storage. Doing
 > some envelope approximations, I estimate that most of the fans record
 > erosion rates equivalent to ~ 60 tonnes/km².yr., which seems
 > reasonable, if somewhat high. Perhaps this supports your argument that
 > the fans are relatively effective traps. Maidstone's erosion rate of ~ 3
 > mm/yr is clearly unsustainable, but I'd like to be able to think about it in
 > light of the other values. I think the value for fan aggradation should be a
 > thickness, rather than 4770 m³.

> 17. Why is this Discussion-it and the next couple of sections are mainly
 > results without much appeal to the broader topics that you listed in your
 > introduction. It's almost as though you feel the need to establish here that
 > what you've described are fans but I think you've done that already and
 > that most of these two pages should be one section back.

> 18. Is it possible that the fans only had limited areas that were sufficiently
 > stable for soil development?

> 19. This seems rapid and should be a matter of discussion, since you
 > have the data to constrain these rates: compare to Birkeland's values or
 > other dated sequences from New England. This still reads like results.

> 20. Might want to start with this hedged but positive statement: It is
 > remarkable that there is any correlation and impressive that each of the
 > fans records the historic pulse. Does this mean that there was a regional
 > event at about 9300 yr BP and there's not been anything that big since?
 > What do the lake records say?

> 21. Whew!! Shorten this up: too long and complex for the reader to
 > follow.

> 22. This way of thinking about how fans record climate seems like a
 > focus for your discussion. But you don't anticipate it in your opening
 > paragraph, so it is a surprise here. If the latter part of your RESULTS
 > section included "Fan Sedimentology and Stratigraphy", "Fan
 > Development" and "Soil Development", you could jump right into the
 > discussion of fans, stability and climate. If you look back at your
 > introduction you'll also find you "set up" a discussion of storm
 > size/recurrence interval that you would do well to revisit here, even if it is
 > hard to be specific.

> 23. Why not start with this good summary statement and then go to the
 > more specific discussion?

> 24. This written comparison would make an excellent summary figure!

> 25. What correlation would you expect amongst these records? If your
 > record is one of big storms (and not fires or blight, etc), I wouldn't expect
 > much of a relationship between pollen and your record.

> 26. See earlier notes on the likely nature of slopes recently bared by ice
 > retreat: sediment will bleed off these areas with spring snowmelt or
 > ordinary storms until some soil strength become established regardless
 > of the position of the polar front.

> 27. (Figs. 4-8.) I think that these complex diagrams would be improved by
 > an explanation for each one. You wouldn't have to vary it much! The
 > descriptions in the figure captions don't work well with the figures in their
 > reduced form: what is grey? what is black? what is thick? Thin?
 > Diagonal stripes? Closer stripes? Only the highly motivated readers are
 > going to look carefully at these figures, but you should make it easy for
 > them! Flipping back and forth to the corresponding tables is going to be
 > aggravating! You should also note that Figs. 4-8 each have a. and b.
 > panels.

> 28. (Fig. 9). The shading here seems too light. Is there any reason why
 > you didn't calculate the aggradation rate on some unit area basis (depth
 > on the fan's thickness removed from the catchment?). See also note 16.

> Table 1. Do you have sufficient data to make the into a figure of frequency
 > versus time, a sometimes useful approach?

> 29. (Tables 3-7). Would it make sense to have these dense arrays of
 > information in an appendix? I think it is valuable information, but it is hard
 > to read and, as I note on Table 3, there are odd bits of information that
 > make sense only with respect to the figures. Note also the questions
 > about soil terminology.

>
 > Data Repository: I don't understand what you have placed in here and
 > what is in the body of the manuscript. The text and the tables in the
 > repository are substantially the same as what you've put in the
 > manuscript. The figures, on the other hand, are mainly different and add
 > information. Since someone requesting the data repository will already
 > have the journal article, I'd suggest limiting what's in the repository to
 > what is not in the manuscript. Alternatively, you could shorten and alter
 > the text descriptions of the individual fans and refer the reader to the
 > repository.

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