

# ***Interactive comment on “Morphological properties of tunnel valleys of the southern sector of the Laurentide Ice Sheet and implications for their formation” by Stephen J. Livingstone et al.***

## **Anonymous Referee #3**

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Review of Livingstone and Clark “Morphological properties of tunnel valleys of the southern sector of the Laurentide Ice Sheet and implications for their formation”

This is an exciting and welcome paper; however it needs some major revision. Tunnel channels and tunnel valleys have been studied in the Upper Midwest of the United States for over 40 years. And although some regional compilations have been put together before, this paper gives us a complete map showing the distribution of topographic features the authors interpret to be tunnel valleys (and/or tunnel channels).

I would suggest my major criticisms are that the authors assume that (1) there is a ‘one-size-fits-all’ explanation to the distribution of tunnel valleys/channels and that (2) a regional geomorphic analysis is sufficient to make informed interpretations of tunnel

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valley/channel genesis. As a geologist who has worked on some of these tunnel valleys/channels, it is my experience that it is not often obvious what is or isn't a tunnel valley/channel. Also, it is quite clear to me that the distribution of tunnel valleys/channels is sensitive to many factors (which the authors to some degree refer to) including basal thermal regime, age, dominant grain size, timing and climate. The local geology is an essential ingredient of understanding geomorphic development. A regional geomorphic view is important, but I regard the conclusions of this paper, based solely on this map, as being tenuous. The map is a 'great map,' but it is not clear how much it tells us about tunnel valley/channel development. The article should be published, but the authors need to soft-pedal their interpretations. This pure geomorphic analysis cannot give the whole story.

My review is rather critical, but this likely arises from my geologist perspective where a holistic understanding of a relatively small region is believed to be necessary to understand geologic history and landform genesis. It also arises from many years of discussions about these features and being perplexed with regional variations. Here are some general comments followed by more specific ones.

—The authors are inconsistent on expressing their interpretation(s) of the genesis of the TV/Cs. That is, in the abstract and elsewhere, it is clear that they interpret that a gradual genesis (TV) is more likely than an outburst one (TC). However, it is clear elsewhere, that they state both processes can work. Nonetheless, there still seems to be a driving assumption, without stating so, that everything they identify as a TV is 'one thing.' That is, they implicitly assume that there is one explanation for all of these geomorphic features. There is actually no scientific basis to assume this, except for their impression that TVs look similar to each other. If they think that everything they identify has the same genesis, they have to argue this—otherwise they are simply assuming that, just because they look alike, they must have been formed the same way.

—Associated with the comment above is that the authors seem to discount that some

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‘tunnel valleys’ are actually ‘tunnel channels.’ There are a number of papers they cite in which the papers’ authors are quite convinced that the tunnel feature they are looking at is a channel, not a valley. And by this they mean that the channel was occupied bankfull when the channel formed, and the channel formed catastrophically. Though the authors mention ‘tunnel channel’ in the beginning, it is not at all clear whether or not they accept that they are TCs. There is no place that they say that ‘we do not believe that there are any actual tunnel channels.’ If TV/Cs are multigenetic, then a combined analysis of all is flawed.

—There are a number of terms that the authors use without explanation that have ‘loaded’ meanings that are not necessarily meant in the way the author’s indicate, or, more seriously, they bear with them a ‘conclusion’ about the nature of these features. The word ‘network,’ so appropriate with subaerial drainages, is used to describe regions with several tunnel features. However, if the tunnel features are tunnel channels, and perhaps formed one at a time, they are not ‘networks.’ They are not ‘networked;’ they are individual channels. However, by using the word ‘network,’ the authors imply the tunnel features are operating simultaneously; the reader then gets the unargued view of the authors that the tunnel features are running at the same time. I ask the authors to find another term to replace ‘network.’ Another example of a term is ‘maturity’ which props up in one place (see below). A third would be ‘phase;’ also a loaded term (see below).

—The mapping of subglacial lakes (in modern ice sheets and for Pleistocene ice sheets) is an important development. The Livingstone et al 2013 paper is a great step forward in our understanding, but in places in this paper, the authors refer to this paper as if it is correct. One colleague of mine has modeled subglacial water in the Upper Midwest and gets subglacial lakes in many places Livingstone and others don’t. This work is not published, but it simply means that the current authors should tread lightly with the ‘truth’ of their results.

—Finally, the authors have done a hell of a lot of work and have produced a terrific

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map. However, as I indicated above and below, I believe that their analysis is flawed and that it is difficult to come to specific generalizations about TV/Cs that apply to all. Rather, they should emphasize regional variations and be more humble about assuming that their conclusions can be applied to all features they interpret to be TV/Cs. Here are some more specific comments.

Line 16. It is misleading to say that TV's 'tend' to be associated with giant ripples and hill-hole pairs. Most hill-hole pairs do not have TVs and most TVs do not have hill-hole pairs. Only one example of giant ripples was found. Even outwash fans and eskers are not 'tendencies' although many TVs have both. I recommend getting rid of at least the giant ripples, if not the HHs as well.

Line 25-26. The authors' viewpoints shift regarding to TV genesis. Here, it is pretty clear that they infer a 'gradual' origin over an 'outburst' origin. However, it is clear elsewhere in the paper that both occur. The abstract should be changed to reflect this. TVs are equifinal.

Line 34. A paper on tunnel valleys should likely refer to the oldest (?) reference: Ussing, N.V., 1903, Om Jyllands hedesletter og teorierne for deres Dannelse. Oversigt over Det Kongelige danske Videnskabernes Selskabs Forhandlingar 1903, v. 2, p. 1-152.

Line 42-43. This is quite an overstatement. The underlying processes are very well understood (!) — most of what they call TVs are cut by subglacial meltwater erosion. We perhaps understand them better than drumlins, eskers and even end moraines. Yes, there is a discussion on water source and basal thermal regime, but there is not 'considerable uncertainty.' (And regarding their statements on water source and basal thermal regime, this paper does not seem to solve any of this 'uncertainty'.)

Line 57. Something wrong here — 'sheet flood' implies a sheet; 'bank full' implies a channel.

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Line 73. This needs to be clarified—what is being hydrofractured and brecciated? The overlying ice?

Lines 76-80. This is good that the authors understand that an understanding of tunnel valleys/channels cannot be achieved by geomorphology alone. Sedimentology, stratigraphy, climate, glaciology and theoretical considerations must be included. However, earlier studies have also included maps with distributions—rarely were single TVs investigated. We actually know quite a bit about state-wide and interstate distributions. This paper is a welcome compilation of much of this work (and many previously unidentified TVs), but using the word ‘rectify’ implies that something wrong was done in the past and the authors are coming to the rescue. They could say instead something like, ‘Based on previous studies and the availability of DEMs, we are now able to examine the regional distribution of TVs. . . .’

Line 85. Yes, landforms are part of the ‘geologic record,’ but most uses of the term ‘geologic record’ by geologists means information found in rock and sediment and implying some chronologic knowledge. What the authors are identifying is tunnel valleys in a glaciated ‘landscape.’ ‘Geological record’ sounds inaccurate.

Lines 105-110. This is welcome that the authors site the many works on TVs that have been done. Perhaps they are aware that many more TV/TCs are mapped and described in reports and maps of the state geologic surveys. For example, Kent Syver-son wrote a fine report on Chippewa County (WGNHS), which is featured in one of your figures. It would be good to check his story out. In any case, the authors have done a great job in being inclusive.

Lines 132, 133. ‘Sjogren’ is misspelled in many places in the paper.

Line 140. The analysis of Cutler et al is a little more in-depth than this sentence might imply. They made paleodischarge measurements based on boulder sizes that implied large discharge rates. A reader might think that the authors saw ‘big rocks’ and thought ‘big water.’ It was more sophisticated than that.

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Line 156. As I imply earlier, TVs are interesting, but we know quite a bit about them, but some aspects are still unknown. I am not sure if this makes them ‘enigmatic.’

Line 160. ‘Fluvial river’ sounds strange. Are there any rivers that are not ‘fluvial?’ And are not proglacial streams ‘fluvial,’ too? How is a proglacial river not fluvial?

Line 163. This sentence opens up a can of worms. Hidden in this comment is an assumption that there is a landform that could be seen to be an immature tunnel valley. Can this be true? Have you seen any? How do tunnel valleys evolve? And do you have examples of TVs in youth, maturity and old age?

Line 163. Certainly there is evidence for smaller subglacial streams beneath glaciers, as shown by imaging of the base of the Antarctic ice sheet, or as seen in ‘canal’ and other glacial sediments. Are these part of a ‘continuum’ with tunnel valleys? I think not. By using ‘less mature’ and ‘continuum’ the authors are making the danger of linking many forms together that may not be genetically similar. There certainly are geomorphic features on this planet that show evolutionary forms (alluvial fans, hillslopes) as well as ones that form continua (eolian dunes; drumins, drainage networks), but this does not mean all landforms are evolutionary or part of a conituum. Here too is an assumption about the nature of tunnel valleys and subglacial streams that is merely implied and assumed. There seems to be an urge on the part of some geomorphologist to see everything as part of a continuum.

Line 167. ‘Potential’ tunnel valleys. The term ‘tunnel valley’ and ‘tunnel channel’ are interpretations; it is important to remember this. It would be helpful to have a term like ‘tunnel-valley-or-tunnel-channel-like valley’ which would be a non-genetic name for these features. However, that is rather clumsy. This sentence would be better for me if it said: “All valley forms that potentially could be interpreted as tunnel valleys or tunnel channels were mapped, and then each was tested to see if it could be shown to have been formed subglacially, and thus, be interpreted to be a tunnel valley or tunnel channel.” And, prior to the next sentence, it would help to add “One way

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to strengthen a subglacial interpretation would be to demonstrate that the longitudinal profile slopes upward towards an associated ice margin or that the profile undulates.”

Line 167. ‘Thalweg’ is not quite the right word. You simply mean the valley bottom,. Avoid fanciness when it is not necessary. Subaerial rivers have, in fact, thalwegs that undulate, going up and down through riffles and pools. I suggest to get rid of ‘thalweg.’)

Line 187. ‘Phase’ in the way they use it, also implies that the TV/C’s are operating at the same time. This term is ‘loaded’ and has either unintentional or, worse, unsupported implications.

Line 202. Your point (3) can be explained otherwise. It is not uncommon for TV/Cs to be filled with stagnant ice during retreat, leading to collapse after retreat. A ‘breached moraine’ may not be a sign of continued TV/C activity, but rather collapse of buried ice into the TV/C, implying the opposite about TV/C activity. My guess is that it would be hard to differentiate these.

Line 212. From reading these lines and looking at Fig. 3, it is not clear to me that you are describing what is shown. First, and again, I think you need to remove ‘networks’ and replace it with something else. But you need to look again at you map and simply describe what you see, and not try to force coming interpretations (Figure 13). Perhaps I would write something like “Certain ice lobes completely lack TV/Cs, or have very few (James, DML, Michigan, Huron-Erie) while others have TV/Cs that are somewhat evenly occurring along much of the lobes’ margins (Wadena, Itasca, Superior, Chippewa, Saginaw). Still others have TV/Cs along lateral lobe margins (Green Bay, parts of the DML). Some TV/Cs are more prominent at retreatal positions, others at the LGM margin. In fact, it is difficult to describe clear tendencies in TV/Cs occurrences that are valid throughout the study area.” It seems that the TV/Cs actual do run down the center on the lobe for the Saginaw, retreated Langlade, Chippewa, Superior, Wadena and Itasca lobes, so your statement of ‘avoidance’ doesn’t seem to match the figure. I also fail to see any prominent TV/Cs in what you call suture zones (interlobate

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areas?). If you see these, you need to give an example. You should also be aware of what may be perceived as an interlobate zone on your map might not be valid because the TV/Cs may have developed at a time when there were not two lobes adjacent. To be clear, let me repeat what I say here—I simply do not recognize what you say when I look at your figure and read this text.

Line 220. You need to be clear about the use of ‘basin’ and ‘subbasin’. You mean some kind of depression that would have been basin-like when covered by ice. Certainly the Great Lakes represent such basins. The Saginaw does originate in an arm of Huron, but the Langlade and Chippewa do not originate in sub-basins of Superior. And the Des Moines does not have any basin at all, but it does have a trough (created by ice streaming, likely). In other words, I think you have expressed these ‘lows’ inconsistently and inaccurately.

Line 224-227. See intro of this letter to see what I feel about the subglacial lakes. Here, to point out why some TV/Cs have predicted subglacial lakes and others don’t, says more potentially about the modeling of subglacial lakes than it does about TV/Cs.

Line 229. To repeat, I think ‘network’ is a loaded term and bears with it unproven implications.

Line 245. I suggest to replace in the heading ‘a tunnel valley’ with ‘tunnel valleys’ or ‘tunnel valleys/channels’

Line 264. Figure 8 shows a weak relationship, it says here. As I say in the introduction to this review, there is a philosophical problem with placing all TV/Cs in the same study. If they actually do have different origins (for example, gradual vs. outburst, or that some of the ‘TVs’ are actually palimpsest meltwater channels), this means that analyzing them together is somewhat illogical, or potentially less meaningful. Certainly, these relationships can be mentioned, but the authors MUST point out the potential weakness of the assumption that there is a common genesis to their TV/Cs.

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Line 290. This is an interesting relationship, but it seems be a good example of how different TV/Cs are in different places. The hill-hole pairs in ND are controlled by the presence of permafrost (according to Clayton) and especially the Cretaceous SS-Sh bedrock which affects the groundwater. The thrusting involved in the hill-hole formation 'uncorks' the subglacial water source and a TV/C is created. However, it seems for this reason odd to have these listed in a section on 'systematic associations.' The ND examples are an exception to the rule that TV/Cs and HHs are most often unrelated.

Line 302. As with hill-hole pairs, there is even less of a general relationship between giant ripples and TV/Cs. It is very cool that they have found some in one place, BUT again, this is an exception to the rule that giant ripples and TV/Cs are usually unrelated.

Line 302. The most common landforms associated with TV/Cs are not even mentioned here, including eskers, but even hummocks. Most TV/Cs that I have seen in WI and MN are associated with ice-margin positions marked by extensive hummocky topography. Why have these been excluded?

Line 319. As my comment for Line 212 says, I simply cannot see the patterns the authors say they see. Even worse, by this point at line 319, this 'unsubstantiated' observation is now presented as a general rule 'strongly correlated' to ice geometry. TV/Cs ARE NOT more common in interlobate areas—where do they see this? Furthermore, it is true that there are no TV/Cs at the center of the James and DML, BUT they DO occur in the Superior, Wadena, Itasca and Saginaw. How can you make this general statement which is demonstrably false by your own Figure 3? And why are these lobes (and their TV/Cs) different? My thoughts turn to that the James and DML are younger, advanced in a warmer climate, formed by extensive ice streams, and they deposited clay loam tills, whereas as the other lobes are older, advanced into permafrost terrains, are dominated by sandy sediments. In other words—you cannot compare these lobes! You have no basis to do so. Or rather, the better answer goes beyond comparative geomorphology and has more to do with climate, sedimentology, etc.

Line 333. Here is an example of what I mentioned in the introduction. First, according to the various interpretations of TV/Cs in the literature, a subglacial water source is not necessary (Mooers for example). Second, and more important here, just because Livingstone et al 2013 don't find modelled subglacial water, does not mean there wasn't any! This means the strong conclusion of lines 335-336 is overstated (and might come back to bite the authors). To soft pedal would be to say "It might be that Livingstone et al 2013 have underestimated the distribution of subglacial water, but if their analysis is correct, then the storage of subglacial meltwater is not necessary for TV/C formation." But, if Livingstone et al 2013 do not consider a frozen margin (which likely was present along so much of the LGM margin), it makes it seem illogical to apply their lake study at all. One last extension of this topic that the authors may be unaware of is that permafrost features are rarer in Iowa and Illinois (where TV/Cs are absent!).

Lines 377 and 379. The idea of permafrost was not invented by these authors to explain TV/Cs, especially in the Upper Midwest. Rather, there is abundant evidence for well-developed permafrost that can be shown to have existed before, during and after the LGM. This means that it is a clear 'boundary' condition when the ice is at the LGM margin. This also helps explain the abundant hummocky topography, by the way.

Line 389. It is impossible to get TV/Cs where there is thin till over crystalline bedrock. That is, it is not 'partially' controlled; it is 'completely' controlled.

Line 394. This is a very interesting comment and one that needs some development (but not in this paper). If a frozen margin is so important for the broad geomorphology of the Midwest lobes, how does it change during the retreat of the ice? We know that permafrost conditions existed during much of LGM retreat in the Upper Midwest, but was the toe always frozen? When the ice was at the LGM, there was sliding (and drumlins) not far up ice. Could the frozen toe get re-established as the ice retreats? Interesting question.

Line 407. I don't want to belabor the point, but this argument works only if the authors

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can demonstrate that all TV/Cs have a common genesis.

Line 426. It is not clear to me that this argument from subaerial streams is applicable for the TV/Cs.

Line 492. In some places in the Midwest, the tunnel channels are considered to be a features related to surging lobes/ice streams. In other words, these are not stable ice lobes.

Line 511 and following. For this discussion, especially about hill-hole pairs and giant ripples, please refer to my comments from Lines 290 and 302.

Lines 553-555. The outburst origin proposed by Clayton, Jörgenson, Colgan, Cutler and others bears little resemblance to Shaw's ideas. That is, it is unfair to pair the unpopular hypotheses of Shaw with these researchers, all of whom reject, for example, Shaw's derumlin hypothesis.

Line 555. Mooers is a good paper, but it is only about a few TV/Cs in Minnesota. He is not necessarily trying to say all TV/Cs form this way. His few are not the 'many' you describe. You cite him inaccurately. (I think, BTW, he may be correct for the TV/Cs he describes.)

Line 570 and following. Here (suddenly? Finally?) the authors are now talking about outburst floods to form TV/Cs (channels). It is clear that they accept this genesis! However, still, in their analysis, they are assuming all TV/Cs form in the same way. Why is it not clear from the beginning that the authors think these to be equifinal?

Line 600. The Superior lobe surged, too.

Lines 600-603. This argument is not clear. I have read it several times and it does not make sense to me.

Line 606. Could your amphitheater heads be 'plunge pools' of supraglacial lakes that hydrofractured to the glacier bed? Can these exist? What would be the geomorphic

evidence for them and how would you distinguish them from your headward migrating channels?

Line 609. Delete 'paradigm.' There have been two primary 'explanations.' 'Paradigm' is not the right word.

Line 615. Again, I think the authors are standing on thin ice by too strongly applying the results from Livingstone et al 2013 without appropriate caveats.

Line 619. OK, I can buy the ideas that in some areas the TV/Cs seem to be 'organized.' But 'well-organized?' Sand dunes are well organized—drumlin fields tend to be also, but it is overstating it here to say that TV/Cs are.

Line 635. Once again—how do the authors want it? They cannot admit to the idea that there is more than one way to make a TV/Cs and then treat them analytically as if they are all the same. It is illogical. Also, here, this outburst origin seems to be rather 'hidden' in the back of the article. If they really truly believe that these form in more than one way, then this needs to be a theme expressed at the beginning (and in the abstract). And every time they introduce a graph that shows all collectively, they must express a caveat.

Line 933. Figure 2. This is an excellent figure! But I would like another figure in this article that shows the location of these maps; just saying 'Superior Lobe' is not satisfactory—I want to know where these exactly are from. This will help the reader convince herself if she were to check it out.

Line 942. I would remove F. It is not even a tunnel valley. It is not clear why this is here; perhaps as a contrast to the others?

Line 947. Despite all my critical comments, I really like this map. However, it is weakened with some minor comments. (1) It is difficult to believe that the 'long lines' above the two 'ee's in Green Bay and SW of the 'L' in Huron-Erie Lobe are actually TV/Cs. (2) the thin black outline—what does that show? It is odd—where is it from? It seems

to show glacial Lake Wisconsin; why? It also goes 'inside' the LGM moraines in Illinois—it looks like a mistake (also in the James Lobe). (3) It is alright to use Fullerton, but his map of moraines and hummocky zones is not quite correct.

Line 963. As in Figure 2, I would like to see these maps indicated in a reference map.

Line 972. Is the Lamb reference appropriate here? Doesn't it deal with bedrock rivers? Is it applicable?

Line 956. This Langlade image seems not to represent TV/Cs. The drumlin-cutting swath in the middle may be a deglacial outwash stream (judging by the 'valley' sides and the geometry). Why do you think it is a TV/C?

Line 990. Where is this in Minnesota? Since most giant ripples in the world are associated with catastrophic discharges, (and there have been several in Minnesota!), is it possible that this is a spillway? I cannot judge if I don't know where it is from.

Line 997. Here now I understand why you emphasize interlobate areas! However, I still fail to see a pattern on your map that looks anything like how you 'would like' it to show.

Thanks for doing this impressive work! And I hope you can incorporate my criticisms to make it even better.

PS I have just read the other comments for the first time and I see that I am not alone in many of my criticisms.

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Interactive comment on Earth Surf. Dynam. Discuss., doi:10.5194/esurf-2016-12, 2016.

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