

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 #2

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This paper is a contribution to the ongoing research on tunnel valleys, which certainly is a very important topic in paleoglaciology that needs to be deepened if the behavior of past ice sheets (stability, meltwater drainage, landforming processes) is to be better understood. Therefore, this study is a timely contribution that surely would not pass unnoticed and I fully sympathize with the authors in their effort to learn more about these fascinating landforms that have attracted a lot of attention and generated yet more controversies. The aim of this paper is to illuminate the origin of the tunnel valleys in the southern sector of the Laurentide Ice Sheet, in particular to test the catastrophic formation theory against the gradual formation theory. In order to do so around 2000 tunnel valleys have been mapped yielding valuable data on their geometrical characteristics

C1

as seen in the present relief.

Unfortunately, despite my unrestricted appreciation of the great effort undertaken by the authors I find numerous methodological and conceptual flaws that resulted in interpretations and conclusions not supported by the data presented. In short, the present manuscript is an example of what one may call “extreme geomorphology”, i.e. interpreting the origin of complex glacial features EXCLUSIVELY from their shapes – below I will try to demonstrate why this approach is flawed in the context of tunnel valleys.

Major issues

1. The authors mapped morphological characteristics of tunnel valleys as seen from the relief of the present land surface and use these characteristics to discuss the formation processes. This is a fundamental flaw because the present relief does not describe the geometry of the channel when it operated but rather the geometry of the infill deposits. In particular, nothing is known about the morphology of the channel bottoms and consequently about the channel depths, longitudinal profiles (flat, adverse or undulating) and their true lengths (which may be much different from the apparent lengths seen at the land surface). In short, little is known about the actual geometry of the valleys and therefore any interpretation and conclusion based on the geometry is untenable. The authors mention this issue in passing (line 96) and yet ignore it entirely in the rest of the paper.

2. No original data are given on the nature of the deposits at the mouths of the tunnel valleys. Without such data any inferences regarding catastrophic vs. gradual water discharge through the channels are speculative at the very best. Still, the authors conclude that they favor the gradual discharge. However, the presence of coarse grained deposits including well-rounded boulders at the mouths of many tunnel valleys is well documented (e.g. in the papers referenced). This material MUST have been transported under high-flow conditions. Without describing the character of the deposits dumped at the channel mouths one cannot conclude anything about the discharge

C2

dynamics. What would help would be some data about the bottom profiles of the channels (steep adverse slopes necessitating highly pressurized, dynamic flows or shallow flat profiles suggesting more steady-state drainage), but these are not provided, either (see point 1 above).

3. The authors confuse incremental origin suggested for some of the tunnel valleys with steady-state, low-discharge gradual origin. These things are not the same. The fact that some of the valleys in the study area can be traced back from one moraine zone to another along the deglaciation path does not mean that these valleys must have formed “gradually” – indeed, they still could have formed in a series of cataclysmic outbursts through the same subglacial channel separated by phases of relative tranquility. But this can’t be constrained because sedimentological and bottom-profile data are lacking.

4. The authors repeatedly refer to the apparent sparsity of tunnel valleys connected to the past subglacial lakes whose location was mapped in the earlier paper (Livingstone et al. 2013) as indication of gradual rather than catastrophic drainage. This inference cannot be made because the postulated subglacial lakes were almost exclusively located in parts of the present Great Lakes basins, i.e. in areas not included in the present manuscript that only encompasses the exposed land surface. There are no data to validate the suggested lack of connection between the postulated subglacial lakes and the tunnel valleys.

5. The paper does not read well, it contains numerous loops and is not well structured. In particular, the reader lacks any information about the location of the study area (position, paleogeography, major ice lobes, etc.) and must fish this information out from bits and pieces dispersed throughout the whole paper. One first finds out where we are from Figure 3, way back into the paper. Repeatedly, there is a mix of own data with data from the literature, and descriptions are mixed with interpretations. It would be much more transparent to describe the findings separately in the context of individual major paleoglaciological systems (i.e. the ice lobes whose behavior could have been different) rather than putting all geomorphological data into one basket for

C3

most of the paper. Accordingly, Figures 4, 5 and 6 are meaningless because they contain data lumped together from different paleoglaciological systems.

More specific issues (mostly related to 1.-5. above)

1. The abstract says that the “tunnel valley morphology is strongly modulated by local variations in basal conditions (...) and hydrology (...)”. There are no data in the paper to validate this statement.

2. Line 99. “. . .buried valleys are rare. . .” in the study area. This statement is unsubstantiated by any data in the paper, nor any of the earlier studies referenced.

3. Line 168. “To determine whether valley thalwegs are undulating the number of (...) slope segments (...) were calculated.” This is plainly wrong and bears significantly on the outcome. Again, when applying the land surface morphology you do not calculate the relevant THALWEG morphometry but the morphometry of the top of the valley INFILL. If one claims that there is no infill to mask the bottom profile, one would have to present strong field evidence. This is entirely lacking.

4. The tunnel valleys are grouped into three classes based on subjective confidence-levels, whereby class 3 consists of all kinds of channels whose subglacial origin lacks any support. Even though this class is not included in the following statistical analyses, these channels should not have been considered at all in the first place.

5. Line 195 and 279. How do you know that the “drainage-sets” you distinguish were indeed “formed during the same drainage phase”? The cross-cutting relationships interpreted exclusively from the morphology (i.e. not considering the sedimentological record below the ground surface) can be illusive – see e.g. the comment on the cross-cutting channels below. Examples of moraines overlapping valleys and valleys cutting through moraines (Figs 10 and 16; all important for the interpretation) are so poorly visible that the reader can’t make any own judgement.

6. Line 353. The claim that “cross-cutting relationships indicate that not all tunnel

C4

valleys were acting synchronously” can’t be validated because it is exclusively based on the morphology. Even assuming that the mapped relief indeed is the relief of valley bottoms, this is claim is still unsubstantiated. An illustrative example is given in Fig. 7D where two channels are interpreted as cross-cutting, and taken to indicate different phases of formation. This is by far not necessarily so, simply because of the possibility of an anastomosing channel network consisting of diverging and converging channels that operate simultaneously. When exposed, such a network may look like channels cross-cutting one another, yet this is not any evidence of their different ages. Such networks are well known e.g. from 3D seismic data from the North Sea but in the North Sea there is also insight into the substratum enabling proper interpretation utilizing the relationships between the infill deposits and the erosional surfaces marking channel bottoms.

7. Line 369 and elsewhere. The authors repeatedly speculate about the low subglacial hydraulic gradients modulated by low ice surface gradients of the ice lobes at the southern fringe of the Laurentide Ice Sheet and use it in support of the gradual drainage. While this may have been the case in some areas, in many other areas there is clear evidence of large accumulations of rounded boulders at the mouths of the tunnel valleys (e.g. Cutler et al. 2002 referenced in the ms). In order to move boulders over 1 m in diameter the water flow velocity MUST have been high, and thus the hydraulic gradients MUST have been steep. It should be emphasized that the pressure of water in the subglacial channel is not only related to the ice thickness immediately above it (which could have been relatively small), but also to the pressure of water further up-ice as far as the systems are connected. Therefore, high pressure close to the ice margin could have been caused by high pressure of water much further under the much thicker ice.

8. Line 387. The authors postulate “the paucity of tunnel valleys towards the centre of former ice sheets” and interpret it to “be indicative of a change to temperate glacier conditions” (Line 397). First, I see no logic in this statement and second, as we know

C5

from the literature the up-ice decrease in tunnel valley occurrence is accompanied by an increase in the frequency of eskers that can be considered equivalent to tunnel valleys (typically on hard beds) (see multiple papers of G. Boulton). Therefore, any straight-forward conclusions from the paucity of tunnel valleys in the up-ice areas are unsubstantiated.

9. Line 400 and forward. The comparison between the morphologic parameters of their tunnel valleys and those in the North Sea is misleading because there is a very high chance that the tunnel valleys mapped in this study represent only a small portion of all tunnel valleys and the small segments treated as single valleys actually are parts of one long tunnel valley. This is because that, again, the tunnel valleys in this study are not mapped from their true thalwegs, contrary to the tunnel valleys known from geophysical studies in the North Sea.

10. Paragraph starting with Line 426 and elsewhere. The reasoning about channel width and volumes of water leading to the exclusion of catastrophic discharge are interesting but flawed. This is because (1) the width of the channel as seen in the landscape only refers to the width above the infill sediments, and (2) more importantly, since the depth of the channel and its cross-sectional geometry are unknown, nothing conclusive can be said about the dynamics and fluctuations of water fluxes. This is because a narrower channel can still drain more water (rather than less, as the paper postulates) than a wider channel if it is sufficiently deep.

11. Paragraph starting with Line 451. Here we have speculations about the influence of local basal and hydrological conditions on the tunnel valley formation. Regrettably, no DATA constraining this discussion is presented in the manuscript.

12. Chapter 5.3 on landform associations. This chapter is difficult to follow and understand due to the lack of thorough description of specific areas used to illustrate the examples. Rather than organizing this chapter by specific landforms, it would be much more transparent to describe specific areas and illustrate landform associations

C6

occurring there. But even then it would be weak because the only data available is the surface relief and everything on the internal composition, deposits, structures, etc. is lacking.

13. Line 583. In order to support the inference that large drainage events were not the primary mechanism of tunnel valley formation an argument is given that the lengths of tunnel valleys are “orders of magnitude less than the distance up-glacier (...) that supraglacial and subglacial lakes are commonly documented in Greenland and Antarctica”. This can be questioned because (1) the authors only mapped tunnel valleys with a topographic expression that likely only represent a portion of the whole population of the tunnel valleys in the area (including the buried ones), and (2) as we know from the recent paradigm shift in the englacial drainage research (the cut-and-closure model; e.g. Gulley et al. 2009, *J. Glaciol.* vol. 55, no. 189, and some following articles) the englacial channels may be oriented nearly parallel to the ice surface instead of penetrating a glacier at a high angle (the old Shreve’s theory), and therefore can drive water from supraglacial lakes for long distances englacially before it reaches the bed.

14. Locations of figures with morphological examples are not shown in any reference map; Fig. 12 refers to “Giant Current Ripples” but I can’t find them in this figure because they are not labeled as such there; tunnel valleys in Fig. 14 copied in from Fig. 3 are totally out of scale of this map showing nearly the whole Northern Hemisphere and thus invisible; Fig. 15C is redundant because you know nothing about the (true) bottom profiles of the tunnel valleys; Fig. 16 lacks scale bars; Fig. 18 is trivial and brings nothing new.

15. Multiple typos and awkward/unclear expressions, e.g. Line 38, 195, 226, 411 (what is MSGs?), 418, 592, 937, 940.

In sum, I doubt if the aims of this study can ever be achieved using the purely geomorphological method while any data regarding the geology and sedimentology of the tunnel valleys and the related features mentioned in the paper (utilizing e.g. geophysi-

C7

cal, borehole and other field data) are not considered.

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

C8