Reply to comments

We would like to thank the three anonymous reviewers and Marco Jorge for their comments. We are particularly pleased that three of the four reviews thought it was an important paper that should be published, and their constructive criticisms have really helped to improve it. We are disappointed that reviewer #2 thinks that we cannot extract information and meaning from the geomorphological signature of tunnel valleys. But we strongly disagree with this view and our argument is laid out below. That they ignore a large body of literature spanning many disciplines that has shown the importance of geomorphology for investigating landforms genesis we find rather perplexing. The reviewer comments are in black and our replies and revisions in blue.

- Stephen Livingstone and Chris Clark

Short Comment by Marco Jorge

INTRODUCTION

L80-84 (statement of objective): Consider rephrasing sentence (perhaps revise whole paragraph). Suggest to remove "To rectify this" (to correct this error), because it is about the lack of data rather than error. Scale -> geographic scale; Pattern -> spatial arrangement; Rectify ". . .mapping of the size, shape, pattern and [spatial] distribution to better understand spatial properties. . ." L84: "constitution" -> Composition?

We have replaced "to rectify this" with "Based on previous studies and the availability of DEMs, we are now able to undertake…" (as suggested by reviewer #3), changed "pattern" to "spatial arrangement" and "constitutes" to "is".

Reword the rationale for study area selection; e.g., that the landforms can be mapped from a DEM is unrelated to the study area.

We have deleted "they can be identified from digital elevation models (DEMs)"

Requires a study-area figure ahead of section 2. Ideally, it would include previously mapped tunnel valleys.

We have included a study area figure (new Figure 2) within which we have include sub-panels showing where the examples we present are taken from (see comments by reviewer 1).

INTRODUCTION/LIMITATIONS

Consider a more explanatory (longer) header for this section and 'challenges' as a replacement to 'limitations'. This section does not fit the introduction. It would better come under methodology.

We have moved the 'limitations' section to the methodology (now 3.4). However, we feel that 'limitations' is an accurate subheading of what this section discusses and have therefore retained it.

In what way(s) are buried tunnel valleys a limitation for the characterization of tunnel valleys? Section does not properly ponder this. What properties can be compromised?

We have extended this section to detail the limitations of infilling on each of our metrics (i.e. length, profile and width) and also stated that we cannot measure the depth of tunnel valleys.

Last sentence of paragraph L96-103: what does data from Europe show? The expectation would be for positive spatial autocorrelation – a problem for the characterization of tunnel valley spatial properties.

We compare our metrics and the distribution of tunnel valleys to those collected in Europe in the results and discussion sections and therefore do not feel that it should be included here also.

METHODS

Clarify that the USGS national elevation dataset (NED) is not a DEM per se. The NED includes several DEMs, not only the 3 m and 10 m DEMs.

We have modified the sentence to say "utilising DEMs" so that the reader is aware that the NED comprises several different resolution DEMs.

L146-147: Which criteria were used to map each of the mentioned landforms? Define each landform; use references. "Conventional criteria" is not self-explanatory

We have deleted "identified according to conventional criteria and…" as we do not want to use a lot of space defining glacial landforms that are not the major focus of what is already a long paper.

L148: Centrelines. Centrelines are not thalwegs. Revise manuscript accordingly. Were centrelines automatedly derived from valley side lines? Please describe methodology.

This should be thalwegs, which we have fixed here and throughout the paper.

L169: Justify scale chosen for analysis of longitudinal profile.

The scale chosen for analysis of the longitudinal profile was rather arbitrary, in that we wanted enough data along each line to make a decision as to whether they undulate or not and at an appropriate scale (i.e. not at metre scale where local effects become important or km-scales where we might miss smaller patterns).

L178-179: Clarify. Tributary tunnel valleys excluded from inventory?

We feel that the second sentence clarifies this point, i.e. where tributaries exist, only the longest route was used.

L181-182: Reword. It is about describing the relationship between width and local elevation gradient; not about the influence of one on the other.

We have replaced "influence of" with "relationship between".

L182: Justify 1km scale. This is a particularly important issue. Why an absolute interval, if valley length varies considerably? What about the valleys under 1 km and some kms long?

As per the comment above about scale, we had to make a decision, and felt that this scale provided a good compromise between data efficiency/processing and information. Although we may have missed some information for the very small valleys most are >5 km and so we feel the choice is justified.

L185: Reword. "Downstream distance" from what?

"from the head of the tunnel valley" added.

L191: drainage network –> tunnel valley network?

Changed as suggested.

RESULTS

L205: Perhaps would be more informative to the reader to name this section "results".

We have changed to 'results' as suggested.

Suggest reviewing usage of 'network'; 'cluster' is more appropriate; most tunnel valley clusters do not seem to be networks; rephrase, e.g., sentence L211-213.

This was also suggested by reviewer #3 (see below) and we agree that the term is not appropriate here. We have taken the reviewers suggestion and changed all references from network to cluster.

L247 vs. L399: 65 km vs. 55 km for maximum tunnel valley length. Rectify

Thanks for spotting this – it should be 65 km, which we have now fixed.

DISCUSSION

A discussion of the limitations is missing. The 'limitations' subsection under introduction is insufficient and would better be under the methodology and discussion. Include considerations on the usage of the centreline for analysing the (thalweg) longitudinal profile; and on the post-formational modification of valley bottoms, linking it to the scale chosen for longitudinal profile analysis.

As suggested above, we have moved the limitations section to the methods and expanded to include discussion of the post-formational modification of valley bottoms, and to include the initial presumption that tunnel valleys have a common genesis. See points above regarding justification of scale and use of centreline vs thalweg.

Reviewer #1

This paper is an important contribution to the understanding of the regional distribution of tunnel valleys in a Pleistocene ice sheet. The authors have conducted an intensive analysis of tunnel valleys through DEM interpretation and this paper should be published with minor revisions.

Thank you.

My main criticism of the paper is that I believe that they have underestimated the number of tunnel valleys present. It's hard to tell from their figures, but in the Saginaw lobe, where I am doing detailed mapping, I think there are more tunnel valleys than are mapped. Most of them are shallow and short. This may skew the statistics somewhat. I also believe, from just looking at DEMs and maps that they have missed tunnel valleys along the eastern side of the Lake Michigan Lobe through the Valparaiso moraine. I have not directly mapped them or studied them in the field myself.

We certainly do not pretend that we have been able to spot and map every single tunnel valley. We are covering a very large area and there are mapping problems, such as partial infilling. However, the resolution of the DEMs (3 and 9 m) now allow all but the very subtlest morphological expressions of tunnel valleys to be identified. We therefore believe that we have picked out a high percentage of all identifiable tunnel valleys. Moreover, we accept that this is a starting point, from which we hope to motivate more detailed regional studies to fill in the gaps and further progress the science.

In considering the regional distribution, it seems strange that the density of tunnel valleys in the Saginaw lobe is higher than just about any other area. And these are in the center of the lobe, where the authors suggest should have less than the margins. The original margins of the Saginaw lobe have been destroyed by overriding by the LM and Huron-Erie lobes. Do the authors have any comments on the relative abundance of TVs in the Saginaw lobe?

This is a very interesting point and although we do not know the answer, we now speculate about the importance of regional conditions in the paper. This includes the suggestion that "older or more northerly ice lobes with steeper ice-surface slopes, more extensive permafrost zones and sandier sediments … have a greater occurrence and density of tunnel valleys." [such as the Saginaw Ice Lobe] We have also toned down the discussion on the importance of ice geometry as suggested by reviewer #3.

The sections on the origins of tunnel valleys are on the right track, I think. In my area, I have found a strong association between tunnel valleys and kame-like landforms composed of sand and gravel. This supports a supraglacial meltwater source for the short tunnel valleys in this area. The outwash fans are very coarse, which suggests high discharge and perhaps a one-off type of origin. But there is no reason why they couldn't have been active for a longer period of time. Also in my area of the Saginaw lobe, I think that the tunnel valleys and outwash fans represent a change in drainage mode during ice retreat. There is a large drumlin field in which drumlins are partially buried by outwash from the tunnel valleys and I think that the drumlins were formed during advance with a distributed drainage system with high basal pore pressure and the tunnel valleys became active during retreat with a conduit-type drainage mode with overall lower basal pore pressure and higher coupling.

Thanks. Although we could not find a paper on kame deposits in the Saginaw Lobe we have commented on the evidence for supraglacial meltwater reaching the bed, referring specifically to the identification of moulin kames by Mooers (1989). The idea that tunnel valleys became active during retreat and represent a switch to channelized drainage is really neat and one that has relevance to some work I have been doing on subglacial lakes and N- and R-channels in Alberta, Canada (see Livingstone et al., 2016).

In terms of relatively minor comments, I found it annoying not to know where the detailed DEM images are located. Couldn't boxes for these figures be drawn on Fig. 3? That would help greatly in understanding the regional distribution and properties of these valleys. I was particularly interested in the catastrophic flood valley with the megaripples. I can certainly accept this as a hypothesis but I would like to know if there has been any field work done on these features. Not knowing where it is makes it more difficult to find that out.

We have included a study area figure within which we have include sub-panels showing where the examples we present are taken from.

There is a discrepancy in figure numbering between the text and figure section. Fig. 12/13 is one, and there may be others. I found a few minor typos and will upload a file with the ones that I noticed.

See below, we have made all of these corrections.

<u>Typos and additional comments from supplementary file:</u>

L276 – there rather than their

Fixed

L315 - Why is this association so rare? Out of all the tunnel valleys, it might be expected to see this more commonly.

This is intriguing and presently we are not sure why. It may be a preservation or resolution issue as they are very subtle features (<2 m high).

L334 - I disagree. Fast flow requires distributed drainage, and I don't think is consistnt with tunnel valleys.

Although this certainly runs counter to theory, we can only report what we found, i.e. that tunnel valleys occur in association with the ice lobes. However, we agree the valleys may have formed following slow-down and stagnation, and have added a caveat that they must have been fast flowing "for at least part of their history".

L451 –no "s" on "tunnels"

Fixed.

L459 – Is this based on the bedrock surface? Where glacial drift is thick and variable, the beds may not have been flat. There are bedrock features like cuestas that would not have been flat.

Not necessary – we mean the bed surface within which they formed. Certainly, the gross topography along this sector of the former Laurentide Ice Sheet is very flat, which is what has inspired this suggestion.

L462 - most are straight, trench like forms.

We agree that not all the tunnel valleys conform to this theory. We have clarified this sentence to state that it is just "tunnel valleys, which display large variations in width".

L491 - agree. Probably more likely formed during retreat rather than advance.

Thanks. We agree that they probably formed during retreat, but cannot rule out the possibility that they also form during advance and have since become buried.

L539 - this is figure 12 in the figure section.

We have corrected this.

L544 - This needs field confirmation.

We agree and have added this point at the end of the sentence.

L555 - Agree. Fining upward esker. Kehew et al. 2013

We have included this reference here.

L582 - not true for Saginaw lobe

This point is relevant at the ice sheet scale, which is what we mean here, i.e. tunnel valleys become less common towards the centre of ice masses. To help the reader we have added "at the ice sheet scale" here.

L591 - not only moraine, but non-morianal ice marginal positions

Possibly so, but this is difficult to show from our data and we have therefore left as it is.

L605 - strong association with kames in Saginaw lobe

This is a good point, and suggests contributions from supraglacial meltwater. I believe Mooers (1989) found similar deposits in the Superior Lobe and we have now commented on the evidence for supraglacial inputs to the bed in the paragraph above.

Reviewer #2

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 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.

We readily acknowledge in the manuscript that there are a range of approaches to studying tunnel valleys, including theoretical, sedimentological and morphological, and that each has its strengths and weaknesses. There have been many detailed approaches conducted by localised fieldwork and yet aspects of tunnel valleys remain enigmatic. Our approach was to assess their regional properties and of course it is not logically feasible to simultaneously conduct detailed fieldwork at these sites. Indeed, we are careful not to say at the outset that we intend to solve whether tunnel valleys form gradually or catastrophically, rather that we want to adopt an approach that can tell us something about the size, shape, pattern and distribution of tunnel valleys. We find it strange that the reviewer does not value this information on scale and shape of tunnel valleys. ***Let's be clear, tunnel valleys are erosional features and conduits for the passage of water and are wholly morphological features. To argue that it is inappropriate to address the morphology and geometry of these seems as absurd as expecting those studying rivers to ignore these aspects.*** *Furthermore, our approach isn't wholly morphological as we draw upon the rich literature of sedimentological data when discussing the implications of our results for tunnel valley formation. We are therefore disappointed that the geomorphological results of our study have been dismissed so easily. This review seems to have been badly affected by their dislike of pure geomorphological research, which has been shown to have an important place in palaeoglaciology (e.g. for palaeo-ice sheet reconstruction, identification of ice streams, interpreting subglacial bedforms).*

<u>Major issues:</u>

1. The authors mapped morphological characterististics 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.

Although we agree that partial infilling of tunnel valleys is a limitation (and did not report on depths for this reason), we strongly contend the view that our interpretations and conclusions are untenable. Dealing with a fragmented record is part and parcel of investigating the geological and geomorphological record, and indeed extends to other disciplines such as palaeontology (e.g. Charles Darwin talked about the imperfection of the geological record in the origin of the species). However, we believe it is adequate for us to learn something about the spatial properties of tunnel valleys despite the fact that we may be missing some valleys and we cannot use their depth as a metric. Buried tunnel valleys (e.g. in the North Sea and Baltic) are nearly all pre last glacial, and we could not find reference to any Wisconsin tunnel valleys in the US literature that we could not also find. Width measured at the valley edges is not affected, and while long profiles and lengths may be contaminated by some infilling they still represent minimum bounds and likely capture a large portion of the true population given how well our results agree with more detailed field studies. To further clarify our approach regarding the issue of infilling we have expanded the limitations section to discuss some of these issues (as also suggested by the short comment). In particular, we discuss what metrics can be readily identified vs those that we cannot discern without more detailed sedimentological/geophysical data.

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 favour 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 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).

Perhaps the referee didn't read the paper well enough! We do not disregard catastrophic discharge. In fact, we presented evidence to suggest that catastrophic drainage events did happen down tunnel valleys, drawing both on our own work and the literature specifically regarding boulders at the mouths of tunnel valleys. This includes outwash fans and giant current ripples, while we show that some valleys were only occupied for short periods of time.

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.

This was not our intention, and we have modified our interpretation of incremental formation to include both gradual incision and/or repeated outbursts. Significantly though, this result does suggest that the whole valley was not eroded during a 'single event' and is therefore informative.

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.

We agree, as also pointed out by reviewer #3, that the subglacial lake predictions are likely to underestimate the true distribution because they do not take account of permafrost and the model makes some assumptions (e.g. water pressure = ice overburden). We have modified the discussion to make this clear (see detailed response to reviewer #3 below). As an aside, we actually started this project to specifically look at subglacial lakes in anticipation of finding associations (e.g. Livingstone et al., 2013) and therefore it was surprising to us that there was little apparent link.

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

We have included a new location map (new Fig. 2), which shows where each of the example figures is in relation to the wider context. We have also modified the main map (Fig. 4) to include more details on the surficial deposits. However, as it is a morphological analysis of a large sample of tunnel valleys we are reluctant to split the findings by region. Rather, we have tried to be much more explicit in stating where regional variations occur and have added a new commentary on how variations in tunnel valley distribution relate to regional factors such as basal thermal regime, ice and bed topography, timing and climate.

Minor issues:

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.

We agree that this this is conceptual and not based on data from the paper and have therefore rephrased to: "The distribution and morphology of tunnel valleys is shown to be sensitive to regional factors such as basal thermal regime, ice and bed topography, timing and climate", which relates to a more detailed discussion of the regional variations that has now been included.

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.

We have re-written this statement to state that: "However, few buried tunnel valleys have been reported along the southern margin of the Laurentide Ice Sheet, and most of the completely buried tunnel valley networks in Europe relate to pre-Weichselian ice advances (e.g. Jørgensen & Sandersen, 2006; Kristensen et al., 2007, 2008; Stewart & Lonergan, 2011)."

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.

It is true that there has probably been at least some infilling of all the tunnel valleys following deglaciation. However, we only calculate the valley thalweg to decipher whether it was formed subglacially or not (i.e. if it is undulating). Please note that any subsequent valley infill from subaerial lake or river deposits is likely to ***reduce*** *the undulations (meaning we can be confident in our interpretation if they are undulating). Moreover, we manually checked that undulations are not the result of post-glacial processes (e.g. slope failures, rivers cutting across the valleys). We also recognise that there could be some error by including a higher confidence level, for which valleys must also contain subglacial landforms such as eskers and/or terminate at a moraine. Finally, this technique has been widely used in the literature to identify subglacial meltwater channels (e.g. Greenwood et al., 2007 and references therein). Perhaps this reviewer is worried that we are using long profiles for some other purpose that we are not?*

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.

We prefer to include them as they likely exist and they may help future studies. The tunnel valleys we classify into class 3 may still be tunnel valleys (i.e. they are a similar form) but they require further investigation. As the reviewer states, given the uncertainty over these landforms they are shown in a different colour in Fig. 3 and not used in the statistical analysis. This seems entirely sensible to us and reflects our caution in some of the interpretations.

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 crosscutting 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.

The use of cross-cutting relationships to produce a relative history of formation is well established and routinely used in palaeoglaciology, with superimposed glacial bedforms (e.g. mega-scale glacial lineations, drumlins and moraine) used to identify dynamic shifts in ice flow. We follow the same approach, using cross-cutting tunnel valleys, moraines and outwash fans to achieve the same objective. If a series of tunnel valleys all terminate at the same moraine and contains outwash fans at the terminus we can be reasonably sure they were formed during the same phase of ice retreat.

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

See comments above, we are not making this elementary mistake (re: drainage sets). In terms of Fig. 7d, the tunnel valley trending W-E terminates only a few km further downstream at an outwash fan (see also Mooers, 1989). It therefore must relate to a period when the ice margin was at the fan. The valley that cross-cuts it continues westwards several tens of kms further and must therefore relate to an earlier period of time when the ice margin was much further west. It is this sort of detailed morphological analysis that has enabled us to identify cross-cutting relationships and therefore to show that within any cluster of tunnel valleys formation was not synchronous. In the paper we only use cross-cutting sparingly and for cases that are certain. Again the reviewer appears to be over reacting. However, as this sort of detail is maybe not immediately apparent from the figure we have extended the caption to explain why the two valleys must cross-cut and not anastomose in this example.

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.

We agree that large discharges of water must have occurred down some tunnel valleys and repeatedly reference the work of Cutler et al. (2002) to support this assertion. However, low subglacial hydraulic gradients likely existed due to the low surface slopes of many of the ice lobes (e.g. James and Des Moines – from Clark, 1992). In line with suggestions from reviewer #3 (below), we have rewritten this in the section on tunnel valley distribution. In it we now emphasise that tunnel valleys are rarer under lobes characterised by low ice-surface slopes. Indeed, despite the shallow slopes, large or high pressure discharges could still have occurred due to lake drainage events (supra and/or subglacial).

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 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.

We agree that this paragraph is confusing and have therefore deleted it. However, the apparent tendency for tunnel valleys to form near the southerly terrestrial margins of palaeo-ice sheets is a real feature (based on the current literature) and we therefore think is worthy of mention and some comment. Therefore, in the paragraph above where we discuss permafrost we mention that the *width of the frozen toe is likely to decrease during retreat and so the decline in tunnel valleys away from the maximum limit may be indicative of a change to temperate glacier conditions.*

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.

Certainly we are not able to compare the depth of tunnel valleys beneath this sector of the Laurentide Ice Sheet with those in the North Sea due to the problem of infilling, which is why we didn't. However, we are confident in our comparison of width and our minimum lengths as these are more easily observed from the geomorphological data. Although some of the valleys may have become partially infilled making them look like they are actually multiple tunnel valley segments, in the majority of cases we can use the geomorphology to reconcile this. For instance, where there are very abrupt end and start points (e.g. Fig. 8e,f). Moreover, if many of the valleys had become fragmented due to infilling we might expect a large difference in length between the North Sea and Laurentide tunnel valleys, which would be instructive in itself. Therefore, we are happy with this comparison.

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.

We completely disagree with this point. Although depth clearly cannot be derived because of the issue with partial infilling following glaciation, the tops of the valley sides are still clearly visible and it is these that are mapped. We are careful to not talk about water fluxes or valley depths throughout the manuscript. But, as has been shown in fluvial geomorphology, you would still expect to be able to use the width as some function of the amount of water draining through it, and as such it is interesting that there is so much downstream variation in width. Indeed, we recognise the idea that a valley can erode down into the sediment or even up into the ice (e.g. Fig. 15) as one possible explanation, although there are also others.

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.

Again the referee does not seem to like a regional approach using a large sample of tunnel valleys to progress the science. Anyone is free to make detailed investigations bringing substrate data into the discussion, but it is beyond the scope of our analysis which is primarily morphological.

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 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.

As per reviewer #3s comments (see below), the use of the heading "systematic landform associations" is misleading here and we have changed to "landform associations" and made clear that this section is about what associations with other landforms tell us about tunnel valley formation. Thus, it is less about the distribution of tunnel valleys and associated landforms and more about what these associations tell us about their formation. However, we have included a new figure (Figure 2), which shows where each of the examples in the study area are taken from. We have dealt with the final sentence in detail under major issues.

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.

We believe the argument put forward by the reviewer is flawed. Firstly, why would the tunnel valleys preferentially become buried upstream? As far as we are aware, there is no evidence for this despite the number of geophysical studies that have been carried out on tunnel valleys around the world. Indeed, the lengths of the tunnel valleys reported here are very similar to other studies that have used both geomorphology and geophysical studies (e.g. in the North Sea, Germany and Denmark). With respect to their second point, the cut-and-closure channels proposed by Gulley et al. (2009) were found specifically on uncrevassed regions of polythermal glaciers and are associated with incision of supraglacial streams followed by roof closure. However, cut-and-fill channels have not been found on ice sheets where the ice is much thicker (resulting in greater rates of creep closure) and we are specifically referring to the drainage of supraglacial and subglacial lakes. Subglacial lakes originate at the bed, which renders the reviewer's whole argument redundant, while there is a large body of research that shows that drainage of supraglacial lakes to the bed occurs via hydrofracture and does not drain englacially for long distances (e.g. Zwally et al., 2002; van der Veen, 2007; Das et al., 2008; Das et al., 2008; Shepherd et al., 2009; Krawczinski et al., 2009; Bartholomew et al., 2010; Selmes et al., 2011; Sole et al., 2011).

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 lot 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.

We have included a location map within which we have include sub-panels showing where the examples we present are taken from (also see comments above). The term "Giant Current Ripples" has been used instead of "sinusoidal bedforms" in Fig. 12. We have added scale bars to Fig. 16. Although the tunnel valleys are out of scale in Fig. 14 the idea is to show the general distribution, *hence the use of boxes to refer to other locations where tunnel valleys occur. We have therefore left these as they are. Fig. 15 presents a number of conceptual theories to explain the downstream variation in width – the aim is that these will stimulate physical modelling studies and we are therefore happy to keep Fig. 15c as it is.*

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

L411 - We have included "mega-scale glacial lineations (MSGLs)".

L937 - fixed.

L940 – changed from "is" to "it"

We have checked through the other lines highlighted by the reviewer but cannot determine what is meant.

Reviewer #3

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).

Thank you.

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 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.

Point (1) - we set out to improve knowledge on tunnel valleys by increasing their known distribution and providing information on their morphological properties, and then to see if such information can help to resolve current debates regarding gradual or catastrophic formation. In fact, our findings demonstrate that both can happen and so we do not assume one size fits all. But we did not want to put the conclusions at the beginning of paper. To try and address the question of what is or isn't a tunnel valley/channel we have added in an extra section where we compare the morphological properties of the two. This includes a comparison of tunnel channel and tunnel valley spacing, length and width. The results show that we cannot tell them apart, and therefore that they are equifinal, and we discuss the implications of this, both for our own work and future studies.

Point (2) - we agree that local geology and glaciological contexts are very likely to provide important influences on the formation, preservation and visibility of tunnel valleys, but we also regard that channels are morphological features primarily eroded and so it is sensible to investigate their morphology. We fully agree that the whole story cannot be written from our analysis, and hope that this contribution assists in making the next steps. To try and address this point we have put much greater emphasis on regional variations in basal thermal regime, age, dominant grain size, timing and climate throughout the paper (see series of comments below).

—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 stills 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.

Our intention was to improve knowledge on tunnel valleys by providing information on their distribution and morphological properties, and then to see if such information can help to resolve current debates regarding gradual (Tunnel Valleys) or catastrophic (Tunnel Channels) formation. So in that respect we do make the initial presumption that there is a common genesis and use this as a basis for exploring relationships between form, distribution and process, which could challenge this. To make this clear we have added the following sentence into the introduction:

"… we treat all linear depressions of the appropriate scale and morphological characteristics as the same thing, whilst recognising that in detail this might be a grouping of a number of types".

And another sentence in the limitations section:

"In analysing this large dataset of tunnel valleys along the southern margin of the Laurentide Ice Sheet we make the initial presumption that tunnel valleys have a common genesis and then search for circumstances and data that challenge this. This allows us to focus on possible relationships between form, distribution and process."

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

Building on the points above, we have now extended the introduction to be very clear in that we initially use the term 'Tunnel Valley' in its broadest sense (sensu lato) such that it includes depressions that could actually be tunnel channels. Thus, initially we group all linear depressions of the appropriate scale and morphological characteristics as the same thing and call them as such throughout the paper. However, our results showed that actually there are different types and we make this clear in the implications section and conclusions, where we discuss gradual vs catastrophic formation.

—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 use of the term 'network' was also brought up by the short comment and we agree that this is not appropriate here. We have therefore changed it to refer to tunnel valley 'clusters' as suggested by Marco Jorge. We have removed the term 'mature' from the manuscript, recognising that the term is loaded and quite provocative. We have also removed 'phase' and included 'time period', with an additional caveat that not all the tunnel valleys in a cluster may have formed simultaneously.

—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.

We agree that we should be careful with how we use the results presented in Livingstone et al. (2013). We have therefore clarified in the discussion that the modelling presented by Livingstone et al. (2013) is likely to underestimate the true distribution of subglacial lakes. The specific changes are dealt with in more detail below.

—Finally, the authors have done a hell of a lot of work and have produced a terrific 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 there conclusions can be applied to all features they interpret to be TV/Cs. Here are some more specific comments.

We have responded in detail to these points above.

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.

We agree this is wrong and have deleted this sentence from the abstract.

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.

Certainly, we see evidence for both 'gradual' and 'outburst' formation of tunnel valleys. But the evidence for outburst formation is less widespread and we cannot determine (based on our morphological analysis) whether the high-discharge drainage events created the tunnel valley or drained down it. That is why in the abstract we state that: "Our data and interpretations support *gradual tunnel valley formation with secondary contributions from flood drainage…". We have been through the rest of the paper to make sure that this is a consistent message.*

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 daske Videnskabernes Selskabs Forhandlingar 1903, v. 2, p. 1- 152.

We have included this reference.

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.')

We agree that this statement is very strong and have changed to read "there is still uncertainty over how tunnel valleys form".

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

Deleted "(bankfull flow)"

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

We have added "of the preglacical bed" to the sentence to clarify what is being hydrofractured and brecciated.

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. . ..'

This point was also made in the short comment and we have replaced "To rectify this" with "Based on previous studies and the availability of DEMs, we are now able to undertake…"

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.

We agree and have changed 'geological record' to 'glaciated landscape' here and elsewhere throughout the paper as suggested by the reviewer.

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 Syverson 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.

Thanks for this, and for the tips about state geological surveys. We have read some of these and they provided quite a bit of help in terms of identifying tunnel valleys. In terms of the Kent Syverson report, we have found and read this document, but although it provided a useful background to the region it did not say much about the meltwater signature.

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

Thanks for pointing this out. Fixed!

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.

We have expanded this section to mention how Cutler et al. (2002) used palaeodischarge estimates on boulders as evidence for large discharge flood events.

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.'

We have changed "enigmatic" to "difficult to discern".

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?

We agree this sounds strange and so have deleted "proglacial and 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? 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; drumlins, 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.

This is perhaps a provocative sentence, but our aim was not to suggest that we think there is a continuum, rather that we do not know whether 'meltwater channels' and 'tunnel valleys' belong to the same population. We agree that the choice of words used is perhaps not helpful and the sentence is maybe loaded towards suggesting a continuum. We have therefore rewritten the sentence so that it now reads: "Linear incisions similar to tunnel valleys but of much smaller size (tens of metres in width) and called subglacial meltwater (or Nye) channels are also common in glaciated landscapes (e.g. Greenwood et al., 2007) but it is generally presumed that these are not part of the same population as tunnel valleys; that they are different landforms distinguished by size but perhaps also by process".

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 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."

We agree with rewording suggested by the reviewer here and have included the sentences in the text. To avoid confusion, we have also expanded the first sentence in this paragraph to include: "… and use the term non-genetically in reference to both tunnel valleys and tunnel channels." Moreover, as this is important to state from the outset we have included a new discussion on the terminology, where we outline the different words used to refer to these features, and then state that we use the term tunnel valley in its broadest sense (sensu lato) to include depressions that could also be tunnel channels.

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.')

We have changed here to "valley bottom" but elsewhere it is more accurate to state that we have mapped the valley thalweg (see short comment).

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.

This was not our intention. We just wanted to state that tunnel valleys in the same cluster were likely formed in a similar time period when the moraine was formed, but this does not mean they were all operating at the same time. To clarify this point, we have changed "drainage phase" to "time period (although they may not all have been operating at the same time)".

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.

This is a good point, and one which we had not considered. However, we would still expect moraines or stagnation features to fill the tunnel valley if this occurred. Thus, where moraines are clearly observed and do not cut across the valley I think our interpretation is probably the simplest.

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 interpretrations (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 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.

Further to the general comment, we agree that we have gone a bit far with the description (and interpretation) of the distribution of tunnel valleys. We have therefore softened this section along the lines suggested by the reviewer. This has included deleting text on their relation to suture zones and avoidance of long axes of major ice lobes and replacing with "Indeed, while clusters of tunnel valleys occur somewhat evenly along much of the Wadena, Itasca, Superior, Chippewa and Saginaw ice lobes, they…" Further to comments by this and another reviewer we have also removed the word network and replaced with cluster.

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.

To clarify the use of the terms in this paragraph we have replaced "basins or sub-basins" with "depressions in the landscape". We have also removed the word sub-basin altogether, instead stating the "tunnel valleys are downstream of the present-day Lake Superior Basin".

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.

See comment below, in the discussion we are now careful to clarify that the modelling presented by Livingstone et al. (2013) is likely to underestimate the true distribution of subglacial lakes.

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

We have replaced 'network' with 'cluster'.

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

Changed.

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.

This relates to the reviewer's major criticism and we have dealt with this in detail there. However, it is worth adding here that we have now included a caveat in the limitations section that we initially presume that all tunnel valleys did form in the same way, and then set out to challenge this. Furthermore, we have added in a section in the implications section, where we look specifically at tunnel channels vs tunnel valleys sensu stricto and discuss the implications in relation to the problem of equifinality.

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.

We agree, the question we set up in the introduction, was poorly phrased. We are not looking for commonality, rather interesting relationships between tunnel valleys and other landforms that may tell us something about how tunnel valleys form. We have therefore re-phrased the question in the introduction to: "What can associations with other landforms tell us about tunnel valley formation?" We have removed "systematic", and instead refer to "landform associations". In this paragraph we also now highlight the regionality of this phenomena: "This association highlights the importance of regional variations in controlling tunnel valley formation and morphology; in this case, it is the local geology (Cenozoic and Cretaceous shale and sandstone) and presence of permafrost that likely controlled the initial formation of the hill-hole pair (e.g. Bluemle & Clayton, 1984) and which subsequently triggered tunnel valley growth."

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.

See above, we have now rephrased this section to talk about how landform associations can be used to discuss how tunnel valleys form.

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?

As stated above, the intention of this section was poorly phrased – we are not looking for commonality, rather interesting relationships between tunnel valleys and other landforms that may tell us something about how tunnel valleys form. As such, the association between eskers and hummocky moraine has already been made, while we feel that the landforms we have picked out and their relationship with tunnel valleys tell us something about how they formed or the processes operating (e.g. growing out of hill-hole-pairs, giant current ripples and their association with flood events and the use of outwash fans and moraines to reconstruct the relative meltwater history). We have therefore re-phrased this section to make clear what our aims are (see comment above re. L290).

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 ˇ compare these lobes! You have no basis to do so. Or rather, the better answer goes beyond comparative geomorphology and has more to day with climate, sedimentology, etc.

As discussed previously, we tend to agree with the reviewer that regional conditions are important for tunnel valley genesis and the form they take (e.g. see section 5.2), and that we have overplayed the role of ice geometry relative to other factors such as permafrost in explaining the spatial distribution. We have therefore taken on board the comments above and elsewhere in this review to re-write this paragraph, picking out some of the key regional influences that might result in the observed distribution. However, we still feel that geometry has an influence, and in particular the ice-surface morphology – i.e. that very shallow ice surfaces will hinder conduit formation.

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!).

We agree that this modelling likely underestimates the true extent of subglacial lakes, and we do discuss this possibility at the bottom of this paragraph (i.e. that the model does not include permafrost). However, given that two reviewers mention this, we have further soft-pedalled this association by changing the section to read: "If their analysis is correct, this suggests that the drainage of subglacially stored water was not the main control on tunnel valley formation. But the modelling may well underestimate the true extent of subglacial lakes (i.e. the prediction in Fig. 3 is a minimum distribution) as the predictions do not account for the possibility of water ponding behind frozen margins as suggested by Cutler et al., (2002) and Hooke & Jennings, (2006)."

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.

We already acknowledge the vast amount of work that has been done linking permafrost conditions and tunnel valley formation; e.g. "The prevalence of tunnel valleys along terrestrial margins hints at an important role of permafrost in their formation (e.g. Wright, 1973; Piotrowski, 1994, 1997; Cutler et al., 2002; Jørgensen & Sandersen, 2006)." However, we have expanded the paragraph to show that permafrost conditions were extensive in the Upper Midwest and that landforms such as hummocky terrain, hill-hole pairs and tunnel valleys have all been linked to frozen ground conditions.

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.

Although not the target of this study, tunnel valleys/channels have been shown to form in bedrock (e.g. Regis et al., 2003 – beneath Superior Lake) and therefore it does not preclude tunnel valley formation. We have therefore left this sentence as it is.

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.

Thanks.

Line 407. I don't want to belabor the point, but this argument works only if the authors can demonstrate that all TV/Cs have a common genesis.

See comments above.

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

We only use subaerial streams as an example of how the geometry of channels is expected to change given different drainage origins (i.e. equilibrium system vs. catastrophic event). Certainly there are additional caveats associated with the formation of channels under ice (e.g. whether it will cut up into the ice or down into the sediment), but the general premise is still expected to be valid (i.e. greater discharge = wider). We go on to discuss reasons why the width of tunnel valleys does vary and why it doesn't show a general trend of widening or narrowing.

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.

This is certainly true, although it remains an open question whether the tunnel valleys formed during the surge or after it (see suggestions by reviewer #1). However, this might be an additional reason why tunnel valleys are not so prevalent beneath the James and Des Moines Ice Lobes, which surged to their maximum extents, and we speculate on the influence of rapidly advancing and retreating ice lobes in the discussion in the context of regional variations.

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.

See previous comments and actions taken in response to 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 drumlin hypothesis.

We agree, and do not mean to infer that the work of Clayton, Jörgenson, Colgan, Cutler et al. is in any way related to the Shaw hypothesis. Indeed, we are careful in this paragraph to not include any of these references. But to highlight this point we have included references to these authors after the sentence: "Despite the lack of support for a mega-flood genesis of whole tunnel valley clusters, drainage of stored water down individual valleys almost certainly did happen".

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.)

We have reworded this section so that it does not sound like all TVs form in this way. It now reads: "…as valleys are also found…"

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?

This relates to the reviewer's major criticism and we have dealt with this in detail there, and by reordering this point in the paper.

Line 600. The Superior lobe surged, too.

We have amalgamated this section into the paragraph on regional variations in the distribution of tunnel valleys and deleted the part about the Superior Lobe.

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

We agree and have rewritten as follows: "Growth likely proceeded up-ice from the margin rather than down-ice from a stored water body because tunnel valleys preferentially terminate at ice-margin positions irrespective of their size (e.g. see very small tunnel valleys along the southern margin of the Green Bay Lobe, Fig. 4)."

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?

We agree that this is a bit speculative and that they are probably difficult to differentiate from 'plunge pools' (e.g. seen on the channelled scablands). We have therefore deleted this part of the sentence, and moved the other part about hill-hole pairs to further support our argument for headward growth.

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

We have deleted 'paradigm' and re-written as "…been two explanations for the formation of tunnel valleys:"

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.

In this paragraph we do meant to refer to the results of Livingstone et al. (2013). Rather, that because of our history in looking for subglacial lakes we went into this work with a hypothesis that the tunnel valleys were formed by subglacial lake drainage events. But the data did not support this idea. So as not to confuse the reader we have modified the reference to: "(e.g. Livingstone et al., 2013, 2016)" and deleted "link with predicted lake locations".

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.

We have deleted "well" here and elsewhere throughout the manuscript.

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.

This relates to the reviewer's major criticism and we have dealt with this in detail there. With regards to 'hiding' the outburst origin, this was not on purpose and probably reflects the fact that most of the evidence supports gradual formation. For instance, we do mention the outburst origin before this point with regards to the giant current ripples and outwash fans. In the abstract we do state that "Our data and interpretation supports gradual (rather than a single-event) tunnel valley formation with <u>secondary contributions from flood drainage of subglacial and or supraglacially stored water down individual tunnel valleys</u>". Rather than making the caveat about formation for every graph, we now make the point about initially assuming all tunnel valleys have a common genesis in the limitations section.

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.

This was a comment of all the reviewers and to rectify this we have included a new figure (Figure 2) that shows the field area and the locations of all examples shown in later figures.

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?

As intimated by the reviewer it is there as a contrast and allows us to highlight the different geometry and planform that allow us to differentiate it from a subglacial tunnel valley. We would therefore rather keep it.

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.

Point (1) - the valleys mentioned display the criteria of a tunnel valley and we therefore think they should be included. We felt it was important to be consistent in the application of our criteria so that the mapping was not biased but our own prejudices.

Point (2) – We have removed the thin black line.

Point (3) – we have stuck with the Fullerton map, but have expanded it to include all superficial deposits of the last glaciation so that regional variations in grain size can be discussed.

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

See comment above (re: Line 933).

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

It does deal with a bedrock canyon, but it is an example of an observed single large flood event carving a channel.

Line 956. This Langlade image seems not be 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?

We agree that the Langlade image does not look like a typical tunnel valley and is very wide (up to 8 km). However, it has eskers and outwash fans along its length and a gently undulating profile. We therefore have given it a confidence of 2 (probable tunnel valley).

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.

See comment above (Line 963) – we have included a new figure which shows the locations of all our example 'landforms'.

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.

Given previous comments we have deleted this figure and the related discussion.

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

Many thanks for this really useful and informative review. It has really helped to clarify the paper and we think the revision is now much improved because of it.