

Interactive comment on “Influence of high-order mechanics on simulation of glacier response to climate change: insights from Haig Glacier, Canadian Rocky Mountains” by S. Adhikari and S. J. Marshall

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We thank M. Pelto and reviewers J. Johnson and T. Zwinger for their positive reviews and constructive comments. We have carefully considered the comments in the revised manuscript. Amendments are made (see the attached manuscript) in **RED**, **BLUE**, and **CYAN** to address comments by Pelto, Johnson and Zwinger, respectively. Some of the comments, although very useful, cannot be accommodated in the manuscript, partly because they do not entirely fit into the context and scope of the paper. These are all explained in this write up in a point-by-point fashion.

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To: M. Pelto

Most of your concerns were addressed in a short comment posted by S. Adhikari. Here we update how these are incorporated in the revised manuscript (see **RED** texts in the attached manuscript); pages and lines indicated below are those of revised manuscript.

Note that while we agree with almost all of the points made in your short comment, and while the regional mass balance aspects are interesting, we view these as tangential from our main objectives (a focus on dynamical modelling of Haig Glacier). We have therefore added a few comments to the manuscript, but generally don't follow up with discussion of the broader region.

1. 1713-8: *It is worth noting that glaciers lacking a consistent accumulation zone will not survive even the current climate (Pelto 2010; 2011). Five out of ten years with no accumulation zone, AAR of 0, fits this description.*

Agreed. Comment to this effect added (see page 18 line 25 to page 19 line 2).

2. 1713.9: *An average AAR of 0.14 indicates a very negative mean annual mass balance (Ba) for Haig Glacier 2000-2012. A reference should be made to the mass balance of Peyto Glacier, the nearest glacier with a long term record that had negative mass balances each year from 2001 to 2011, with an average of -730 mm/a from 2000-2011.*

We opt not to discuss about the mass balance of Peyto Glacier, as the focus of this paper is not really to elaborate on the balance state of Haig Glacier in the regional context (see also our short comments #3 and #5).

3. 1718-18: *For both the PS and SD model briefly state what was the motivation for using each model? Is it for a differential diagnosis of the importance of the various stresses?*

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No amendment made; see our short comments. In summary, these are two common models in application for valley glacier modelling, so it is important to understand their behaviour.

4. 1722-12: *Is there a quantitative measure of the velocity fit for each model from Figure 6g that can be provided?*

Now provided. See page 16, lines 22–23.

5. 1725-7: *Given that the glacier has had no accumulation zone in 5 of the last 10 years is this an appropriate description and why would you expect the glacier to survive current climate?*

We agree again, but there is still a question of time scale and the role of dynamics as the glacier melts away. No amendment made. See our short comments as well.

6. 1728-4: *The over deepened basin should develop a small lake, which would impact glacier retreat. Even small lakes have been observed to enhance the retreat of glacier termini through increased ablation and when the ice is very thin breakup. If a lake does not form in the depression, why not?*

Brief explanation is provided regarding the possibility of future lake formation. See page 19, lines 2–9.

7. 1728-9: *Figure 1 indicates peak mass balance near the divide. Given the eastward shift of the divide this indicates significant thinning of the accumulation zone, which would not occur if this was still an accumulation zone, given the slow dynamic response here. Pelto (2010) and Paul et al (2004) both note that thinning and retreat at the head of the glacier is a sign of glacier disintegration and loss.*

See our response #1.

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8. 1728-22: *The output of Figure 12 for the areal extent and thickness of the glacier in 2050 are quite similar indicating the robustness of the output. Figure 9 documents the velocity for the different models for an advance scenario. It would be useful to see the velocity distribution at the 2050 time step for the modeled glacier in Figure 12.*

No amendment made. See our short comments.

9. 1730-2: *Reference to Bolch et al (2010) would place Haig Glacier in the context of other glaciers in the region during this approximate period.*

Again, we see this as a different story (and a distraction) from our current focus, which is on dynamical modelling at this site. No amendment is made in the revised manuscript. See also our response #2.

10. 1730-12: *Is it worth contrasting the results here to those of Marshall et al (2011), which used a more heavily mass balance less dynamic model approach?*

As above. See our short comments as well.

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To: J. Johnson

Pages and lines indicated below are those of revised manuscript. Find **BLUE** texts in the attached manuscript, whenever pointed.

Major comments

1. *The use of the SD model is equivalent to a shallow ice (SIA) model. I don't think that it is fair to run SIA on a 25 m mesh, which violates the basic assumptions that lead to the shallow ice equations; i.e. the thickness to length ratio is not small. Of course SIA performs poorly, its not at all the right model to be using. We didn't need a run to see this. I would propose that to be fair to poor, old SIA, some averaging over the surface be done. To achieve an thickness to length ratio of about 1:10, you should average surface elevations over about 850 m. On so short a glacier, this could lead to problems with averaging near the boundaries, but I think its possible with an asymmetric kernel. Were this done, I suspect that SIA will do nearly as well as the other models.*

Valley glaciers mostly have complicated geometry. With reference to spatial resolution over which the geometry varies (bedrock topography and valley cross-sections), small glaciers such as Haig certainly do not satisfy the fundamental criteria of SIA; neither do they have a small aspect ratio, nor an infinite width. However, SIA-based models are commonly used for simulations of such glacial bodies. In this context, our prime motive of this paper was to explore, without being judgmental, whether it is reasonable to stick with such simplified models. To fulfill this objective, we had to isolate the physics as cleanly as possible. This requires that exactly the same domain be used for each model and that the same numerical settings be employed; as the reviewer suggests, we would alter the domain considerably and lose quite a bit around the edges with an aggressive smoothing. We add a comment concerning this on page 17 (lines 1–5) as this is

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not unreasonable; however, aggressive surface smoothing would be limiting on such a small glacier and would compromise potentially interesting topographic features as a general practice in modelling valley glaciers. Note that we have smoothed the surface elevation by running a few prognostic iterations prior to present-day diagnostic simulations (see page 14, lines 6–10). This was sufficient to avoid any local geometric effects even in SIA model simulations.

The fact is that SIA lacks fundamental physics that may be required to describe the dynamics of valley glaciers. Even if bed and surface topography are smoothed as you have suggested, the bed slope plays a key role by generating large gradients in longitudinal stress, which SIA cannot capture, thereby making SIA less suitable for valley glacier applications (e.g., Le Meur et al., 2004; Leysinger Vieli and Gudmundsson, 2004; Adhikari and Marshall, 2011). On top of this, effects of lateral drag is highly pronounced, particularly in narrow valley glaciers (e.g., Nye, 1965; Adhikari and Marshall, 2012).

2. *The authors are modelers, and everyone likes a little job security. That said, in this paper there is an important case that is missing from the analysis, and that is 'no model'. Why not simply assume that velocities will continue to be similar to what they are now, and see how the time until the glacier disintegrates is changed. I suspect it is not appreciably different to the modeled cases, and that the 'model' used for wasting and retreat is irrelevant, because all of the changes are driven by surface mass balance. This is quite alright in my mind, a paper that shows the community the easiest way to correctly model alpine glacier demise is likely to be much more important than a paper that teases out subtle difference in the model output from different momentum balance approximations. This also takes us back to point 1, above. If we can get away with just SIA on alpine glaciers, we should.*

It is a good idea of being able to make predictions using 'no model', and this is in fact common in the literature (examinations of glacier evolution using only mass

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balance scenarios, or using volume-area scaling for adjustment of ice geometry under glacier retreat). However, this is obviously not better than running a proper ice flow model. What is the basis to use the same velocity as present, as it is clear that, for Haig glacier, the velocity drops by about a factor of five in retreat scenarios and increases by an order of magnitude in advance scenarios? There are similar other issues that are to be assumed in 'no model' case, and these remain mostly unknown unless we have a model solution. High-order model simulations, if possible, are always safe. Even SIA models are useful, particularly because these are numerically simple and computationally efficient, and there are a few parameterized ways to improve SIA simulations by capturing missing effects of high-order mechanics (e.g., Nye, 1965; Adhikari and Marshall, 2011, 2012). So, we think the 'no-model' concept, although it might be useful to get crude insights of glacier evolution, is not so appealing in the context of increasing availability of high-order models, computational facilities, and model input data.

3. *The assumption of a no slip basal boundary may not be entirely justified. While observed velocities are small, I don't think that we can know whether it is deformation flow, or sliding taking place during a short period during the spring when subglacial water pressures are high due to the lack of an efficient sub-glacial drainage system. This isn't something that the authors can do much about, other than to acknowledge the possibility, and that if true, the stress balance would be significantly different, depending on the time of year. This would also tend to decrease A, and that will influence the stress balance as well.*

Based on the limited observations, we have mentioned that Haig glacier, at least presently, has an efficient drainage system (see page 8 line 26 to page 9 line 5). This lends more confidence to stick with only deformational flow mechanism, rather than trying to include sliding mechanism with several unknown parameters. Now, we also acknowledge the possibility of future lake formation during the later stage of glacier retreats and its potential influences (see page 19, lines 2–9).

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Minor comments

1. *p1708 line 26 'tremendous' seems to overstate things.*
OK. We now write "...will have a large impact on..." (page 2, line 25).
2. *p1709 I'm not sure if we'll ever have "full understanding..." in the sense that a predictive framework will be developed. There are too many non-linear couplings and stochastic variables. But that's pessimistic. What I am pretty sure of is that continuing to add more and more components to Earth system's models is just giving us a greater ability to overfit what precious little data we do have, and likely to contribute to less understanding. These are philosophical matters, I don't really care what you say, but you should know that some of us disagree rather stridently.*
Absolutely true. We did not mean that a mere coupling of ice flow and mass balance models would be enough to fully understand the complicated processes associated with ice dynamics. We now rephrase the sentence (see page 3, lines 5–8).
3. *p1715: The manuscript would benefit from error estimates in the measurements of velocity.*
Unfortunately, we are unable to provide rigorous error estimates associated with velocity measurements. However, the employed GPS tool was capable of measuring the stake positions within decimetre accuracy. This is mentioned on page 8, line 20.
4. *p1716 Some readers might benefit from having the \cdot_j subscript defined for them as derivative wrt j .*
Sure. See page 9, line 17.
5. *p1719 I like the idea of calling the SD model the SIA model, because that's what it is, and that's what most of us are used to seeing it called.*

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Thank you.

6. p 1719 I don't know what is meant by 'semi-structured' mesh.

Mesh is unstructured in the plan view, i.e. xy plane, and structured in the third dimension. The footprint of glacier consists of unstructured triangular elements. The mesh is then extruded between the bedrock and surface topography with equal number of vertical layers (ten in our case), such that the vertical spacing, $dz(x, y)$, is uniform. This is explained in Sect. 3.3.1; no amendment is necessary in the manuscript.

7. p 1720 a 5m layer of fictitious ice is pretty thick for a glacier with average thicknesses of 85 m. Justify this decision.

This fictitious ice of 5 m is considered such that, ice thickness over the entire domain (including the forefield) follows $h = \max(5, h)$. This implies that no fictitious layer is added in domain where ice is more than 5 m thick. Therefore this choice of thickness has nothing to do with the average thickness of glacier. In real glacier applications, Zwinger and Moore (2009) demonstrate numerically that a fictitious ice of as large as 10 m does not affect the overall dynamics of glacier. We add the citation in the revised manuscript (see page 13, line 16).

8. p 1722 line 22: I don't know what 'realistic' means here.

This sentence is rephrased. See page 16, lines 13–14.

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To: T. Zwinger

Pages and lines indicated below are those of revised manuscript. Find **CYAN** texts in the attached manuscript, whenever pointed.

Major comments

1. *From the text I conclude that you use one and the same mesh for computing all three approximations to the Stokes equation – if not so, indicate the differences (and ignore the rest of this paragraph). I see an issue with the SIA (=SD in your paper) approach here in that sense, that SIA is based on the assumption of flatness, which basically has to hold also for your discretization. By the nature of its approximation, SIA cannot resolve any bedrock feature of length below the given flow-height divided by the aspect-ratio. In other words, running SIA on unit aspect-ratio meshes is like driving a truck on a twisting and turning go-cart track: You might get along with it – or not completely in your case, as you significantly had to reduce the time-step size (i.e., drive the large truck very carefully) – but it is just not appropriate. Smooth your DEM and enlarge your horizontal mesh size and you will see that the SD solution will get way more stable and less patchy. In that connection, the typical resolution of your DEM would be a valuable information.*

Thanks for this insightful comment and analogy. We have added a brief discussion of this to the manuscript (page 17, lines 1–5). Considering 3-D models that deal with distinct physics of deformational flow, the key objective of this paper is to evaluate the influence of high-order mechanics on simulation of climatic response of Haig glacier (see the manuscript title). To fulfill this objective, we had to isolate the physics as cleanly as possible. This requires that exactly the same domain be used for each model and that the same numerical settings be employed – no matter whether it could be computationally inefficient or whether, in

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your words, we have to “drive the large truck very carefully”. This is mentioned in the manuscript (page 14, lines 12–18). See our response #1 (major comments) to J. Johnson as well.

Note that, prior to the actual model simulations, we have smoothed both the bed and surface topography of present-day glacier domain (page 14, lines 6–10). This avoids from having patchy solutions at some regions (in SD model case). Please see also our response #2 (major comments) below.

2. *In contrary to the often applied surface relaxation pre-processing steps (i.e., short time prognostic runs allowing the errors in DEM to adjust) (e.g., Zwinger and Moore, 2009; Gillet-Chaulet et al., 2012), you seem to immediately start your diagnostic as well as prognostic results from the initial shape you obtain from the DEM. As pointed out in (Zwinger and Moore, 2009), flaws in the DEM (especially in Full Stokes) can lead to artificial local discrepancy in velocity field (in some case order of magnitude off the real value). Can you elaborate why this does not seem to be the case in your application?*

Thanks for reminding this. We overlooked it on purpose, thinking that it would be too technical to specify in the context of the manuscript. We now add a couple of sentences describing how the local features of bed and surface are filtered (page 14, lines 6–10). Minor smoothing was done during the process of mesh extrusion. Further refinement of surface elevation was done through a few iterations of prognostic simulations (see Adhikari and Marshall, AGU meeting, 2011, #C53D-0710).

3. *You are anyhow careful in your conclusions, but perhaps it would be good to even stronger point out that this is just a single glacier study and generalizations should be taken with precaution.. Nevertheless, I agree with some of the principal findings. From an unintentional mistake made during the runs of a paper you cite in this article (Zwinger and Moore, 2009), I can confirm that at low flow speeds*

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and retreat scenarios the 'no model at all' option actually on shorter timescales (a few years) does not make too much of a difference, as surface mass balance is in charge. But on longer time scales, especially looking at Fig. 13, I am not fully convinced that with Haig you just got lucky and the geometry with its over-deepening part makes the area and volume change just accidentally look the same for all three approximations while you have that significant discrepancy between the thicknesses obtained with the different approaches. The deviating thickness tells me, that dynamics actually does matter. In other more provocative words: I do not think that Haig (nor another ice body) is the mother of all glaciers. And, especially as we now start to understand the strong non-linearities and seasonal changes introduced by hydrology, like you, I emphasize that more of these studies shall be made on different type of glaciers – with the nice side-effect that we "Full-Stokers" have a perspective to keep our jobs. Not to forget about the field glaciologists bringing us the valuable data the modelers depend on.

We believe we have been transparent enough to warn that the conclusions are solely based on Haig glacier and precautions are warranted to generalize for other glaciers (page 24, lines 16–27). As you have advised, we highlight it even strongly by rewording a bit in the abstract (page 2, lines 13–14; lines 17–18).

Our results suggest that influence of high-order mechanics is evident even in glacier retreat scenarios; models predict unique distribution of ice thickness although they simulate similar area and volume evolutions. This has been acknowledged in our conclusion #4 (page 24). Without having a clear discrepancy in volume evolution (from practical point of view, ice volume is perhaps the most important entity of glaciers), we are a bit reluctant to highlight it. Anyway, as we have mentioned in several places (e.g., page 24, lines 25–27), similar studies on other glaciers are needed to make concrete conclusions regarding the importance (or not) of high-order mechanics.

4. *What influence does it have to ignore the approximation corresponding contri-*

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butions of the strain-rate tensor in the effective strain rate (the square-root of second invariant of strain rate) within the viscosity? Or are you always taking the full blown strain rate?

Model approximations are applied to the definition of strain rate tensor as well (see, for example, page 12, lines 4–5). Therefore, each model has unique “effective strain rate tensor”. See pages 191–195 of Adhikari (2012) for mathematical details. Unfortunately, we have not particularly assessed the influence of “effective strain rate tensor” associated with each 3-D model on ice viscosity. For 2-D PS and SD models, however, we have analyzed this (see Adhikari and Marshall, 2011; Adhikari, 2012, p. 46–47).

Minor comments

1. *page 1711, line 9: The derived flowline. Indicate how you derived (visual, some algorithm, only at surface?) the flowline, also with respect to the fact that actually there is a reported discrepancy between τ_d and the SD result (which should match).*

We digitized the central flowline manually, following the main valley axis. We also ensured that the flowline is approximately perpendicular to contours. See Fig. 2a. This is now clarified in the manuscript (page 5, lines 4–5).

2. *page 1716, line 1: As a general information, you might want to mention that your equations are presented in index notation as well as that Einstein's convention for summation over same indexes is applied.*

Sure. See page 9, lines 15-16.

3. *page 1716, line 11: What is the basis of your iso-thermal assumption? Usually, we can assume that if we know that we are dealing with a complete temperate*

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glacier – but then the freeze-on condition at the bedrock gets questionable. Nevertheless, your “tuning” of A , which can be interpreted as a tuning with respect to temperature, as A usually is expressed as an Arrhenius-factor, reveals $T < T_{pm}$ (so below pressure-melting). I think you are fine, but some words justifying that assumption would be good.

This is partly because there are no data of ice temperatures or of geothermal heat flux to constrain the thermal model. We now insert a few sentences to elaborate on this (page 10, lines 4–8). In fact the mean annual air temperatures at the site and the extent of meltwater indicate that temperature conditions are likely, and the tuned value of A is within the range of uncertainty of temperate ice values (Cuffey and Paterson, 2010). We believe that the glacier does not slide based on the velocity measurements (see Sect. 2.3) and as a result of being very well-drained (poorly lubricated at the base), despite being temperate. This behaviour is not uncommon.

4. *page 1719, line 12: $\tau_{ij}(s) - p(s)\delta_{ij} \approx 0$. This is wrong – at least for Full Stokes. First of all, the r.h.s. should still be a second-order tensor, but apart from that, the correct boundary condition you solve – actually it is the natural boundary condition for the Stokes-Solver in Elmer/Ice – is $\sigma_{ij}n_j = (\tau_{ij}(s) - p(s)\delta_{ij})n_j = t_i = p_{atm}n_i \approx O_i$, with n_i being the surface normal, p_{atm} the atmospheric pressure, σ_{ij} the Cauchy stress and O_i the zero vector. In other words, you do not have a in all its components vanishing Cauchy stress tensor (check the output on the surface), but rather a in all its components vanishing stress vector t_i .*

Thanks for this. We now revise it a bit to include the outward unit normal of the surface, \hat{n} (see page 12, lines 24–25). However, for simplicity, we opt not to express zero as the second order tensor; 0 is scalar, and so is $(\tau_{ij}(s) - p(s)\delta_{ij})\hat{n}$ for a given combination of i, j .

5. *page 1719, line 22: Zwinger & Moore (2009) – as stated on page 219 of their*

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paper – used the commercial pre-processor Gambit, and not Gmsh for meshing the footprint. If you want to have an explicit reference (and increase my citation index) you might refer to e.g. Gillet-Chaulet et al. (2012), where the initial mesh (although unreported in the paper) before refinement has been made with Gmsh.

Sorry for the misunderstanding. We now rephrase the sentence (page 13, lines 9–11).

6. *page 1720, line 9: The mesh is then extruded using ElmerGrid... If the version has not changed dramatically since I last viewed it, to my knowledge ElmerGrid (for the reader: ElmerGrid is an auxiliary program in the Elmer package to manipulate meshes) does not have any DEM-interpolating extrusion features. I guess you meant ExtrudeMesh. Then the reader might be also interested by the method used for approximating/interpolating the DEM data onto the footprint mesh (in case of ExtrudeMesh that would be the inverse-distance approximation). Hint: you could use a low exponent and a larger cut-off radius for the inverse radius to smoothen out the finer bedrock structures for your SIA runs.*

Yes, you are right that we use ExtrudeMesh instead of ElmerGrid. Thank you! This is now corrected (see page 13, lines 9–11). We also discuss about the interpolation scheme used during the mesh extrusion. See page 14, lines 1–6.

7. *page 1720, line 10: ...with ten vertical layers. This does not sound like a lot, although it could be sufficient. Did you make any mesh-sensibility analysis, especially to the vertical resolution?*

Yes we have run a test with five, ten and 15 vertical layers. For Haig glacier moving slowly (surface velocities on the order of a few meters per year), we find that (through diagnostic simulations of FS3 model) the effects of vertical resolution on englacial velocity and stress fields are minimal (velocity errors within a few decimetres per year). This is a bit discussed in Adhikari (2012, p. 102). Finer vertical resolution demands really small value of Δt for reduced models,

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particularly SD, to converge. Choice of ten vertical layers (page 13, line 22) and $\Delta t = 0.1$ years (page 14, line 17) is some sort of compromise between the spatial and temporal resolutions. We now include a few words on page 13, lines 22–23.

8. *page 1722, line 5: . . . non-local stresses What makes longitudinal and transversal stresses less local than vertical shear stress? I guess you rather refer to the local (in columns) confinement of variable dependencies within SIA equations.*

We think non-local is common and a fine way to describe this, as driving and shear stresses truly are 'local', in a plan view. Longitudinal and transverse stresses are non-local in that they introduce horizontal bridging of strain rates at a given point on the glacier.

9. *page 1736, Fig. 1: Where are the annotations a), b) and c) you are referring to? Plus, perhaps annotate Haig glacier in the photo.*

We will provide figure labels in the TC version of the paper.

10. *page 1737, Fig. 2: This is more of a matter of taste, so not a demand. I know you describe the units in the caption, but why not also put them directly to the color bar? Same applies to all figure-panels showing areal results (Figs. 3, 6, 9, 10, 12 and 13)*

Yes, it would have been great to have units attached with colorbars. Placing these in each subplot might make figures too messy. So, we want to keep as they are.

11. *page 1741, Fig. 6 : If you compare results between different models, in my opinion you should apply the same data-range to each of the columns. Same argument counts for Fig. 9 and Fig. 12, eventually also Fig. 13.*

Agreed. But, it is really difficult to identify the spatial distribution in some plots (e.g., velocity distribution for FS model would be hard to see in Fig. 6a if we fix

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'caxis' to [0,20], which is the velocity range of SD solution). In their current form, color codes show the spatial pattern and colorbars give the range of values. So, we opt to stick with the current form of figures.

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References

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Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/7/C1035/2013/tcd-7-C1035-2013-supplement.pdf>

Interactive comment on *The Cryosphere Discuss.*, 7, 1707, 2013.

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