ESAIM: Mathematical modelling and numerical analysis

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Article title: Surface elevation errors in finite element Stokes models for glacier evolution

Author: Ed Bueler, Ph.D., University of Alaska Fairbanks

1 Summary

The author considers a mathematical model for variation of ice elevation distributed over a land mass. The article presents some analysis and theoretical estimates. The primary goal of the article is to establish the well-posedness of a semidiscrete backward Euler variational inequality applied to the surface election function $s(t, x)$.

The article has a certain novelty. Indeed, the author succeeds to some extent in combining a practical problem with core PDE analysis. However, the results rely heavily on two important conjectures at the fundamental level analysis (Conjecture 2.2 and 3.1). The author could explore some methods of analysis to get around these. Otherwise, the article is not theoretically strong enough to merit publication in ESAIM:M2AN. Furthermore, the article is very difficult to navigate through. A structural modification of the article in necessary.

2 Major comments

1. *Structural organisation of Section 1 (Introduction).* The introduction is not well written and organised. The four and a half page introduction is filled with various information that could have been categorised as motivation, model presentation, notations, etc. Instead, the current introduction presents a continuous monologue of data. This makes it difficult to read and understand the ideas. Furthermore, the introduction does not properly address the author's original contributions, which are, however, loosely outlined towards the end of the third and fourth pages.

2. Does the equality after (17) hold? How does $\epsilon^p|\Lambda|$ appeared? One more step is necessary here.

3. The inf-sup condition is not directly clear in Theorem 1.1. A few lines on the proof (at least in the appendix) is due.

4. The backward Euler scheme in (24) is curious. Why the author denotes the function at n^{th} step by $s(x)$ and at $(n-1)^{\text{th}}$ step by s^{n-1}? Please avoid such notational mish-mash without proper justification. Also, where $\boldsymbol{u}_s| \cdot \boldsymbol{n}_s$ is evaluated (n^{th} step or $(n-1)^{\text{th}}$ step or average), is not immediate. Reader needs to traverse two lines down from there to understand the implicitness of the scheme.

5. How (27)-(29) follows is not immediately obvious. Moreover, the difficulty of reading is increased by the cumbersome notations. Please add more information to guide the reader through the ideas.

6. *Conjecture 2.2* This is a worrying part. Conjecture 2.2 is a major assumption on which the rest of the analysis is based. So far there is no sound justification for it. Even the argument that the conjecture is based on Lemma 2.1 is not direct. This is a very serious flaw in the article. Similar comment is applicable to Conjecture 3.1

3 Minor comments

1. It may be good practice to use boldface for vectors to avoid confusion. For instance, in page 2, the map–plane coordinates are denoted as $x = (x_1, x_2)$. The same typeface for vectors and scalars may render, identifying some inadvertent error of mixing symbols difficult, at a later stage.
2. Please duly emphasise the time–dependent domain $\Lambda(s)$. The current notation and figures give the impression that $\Lambda(s)$ is static, which is not the case. The author may consider replacing the notation with $\Lambda(s(t, \cdot))$.
3. The notation is $\langle -\nabla s, 1\rangle$ is confusing. Does the author mean the coordinate vector $(\partial s/\partial x_1, \partial s/\partial x_2, -1)$? It may be better to avoid this notation; in Sobolev space context, $\langle \cdot, \cdot \rangle$, is often used to represent duality pairings.
4. The dependent and independent variables in the model needs to be explicitly defined earlier for better clarity; currently it is summarised in page 4.
5. The scaling factor $[H]$ intuitively makes sense. However, a remark on the necessity of this is essential. Also, the notation is not motivated. Why $[H]$ is used; why not H? Is it a constant?
6. Consider adding a figure to explain the 'intermediate value theorem' argument after (10).
7. The trace $|Tv|^p$ in the proof of Lemma 1.4 became $|v|^p$ in the statement. Please add a line that zero$^{\text{th}}$ order trace is discussed here.
8. Please refrain from denoting sequences as sets; e.g, $\{t_n\}$ in page 8. Sets do not come with an a priori order. When discretisations are concerned, a definite order prevails. Use $(t_n)_{0\le n\le N}$ to denote the discretisation $0 = t_0 < t_1 < \cdots < t_N$.
9. Why the source term in (25) does not include $(\boldsymbol{u}_s| \cdot \boldsymbol{n}_s)\Delta t$?
10. The article is filled with an abundance of acronyms (NCP, VI, FE, etc.). A table in the introduction is necessary for quick reference, as these acronyms are scattered throughout the article.

4 Punctuational/Typographical/Grammatical/Style comments

1. Page 6: Change "pth-power" to "p$^{\text{th}}$ power".
2. The author unnecessarily capitalises otherwise unnecessary text. For example: 'Introduction' (page 9), 'Section' (several passages) etc. In continuous text, if no indices are used (e.g. Section 1.2), please avoid capitalising in the middle of a sentence.

This list is not exhaustive. Author is requested to carry out a through reading again.

5 Conclusion

The article as such is not of sufficient quality to be published in ESAIM:M2AN in its present form, which in the reviewer's opinion is loosely and vaguely written. The following two points needs to addressed in the case of a resubmission.

1. Conjectures needs to be completely removed in the analysis. Otherwise, a strong justification needs to be presented.
2. The author may consider the above comments and improve the overall structure and presentation of the ideas in the article.