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# *Interactive comment on* "Slate: extending Firedrake's domain-specific abstraction to hybridized solvers for geoscience and beyond" *by* Thomas H. Gibson et al.

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The authors would like to sincerely thank the referee for carefully reviewing the manuscript and providing helpful comments to improve the quality of the discussion.

1) "2.1. There are a few inaccuracies and omissions in the historical references for hybridization of mixed finite element methods, particularly in the introduction.

   2.1.1. The bibliographic reference to Brezzi and Fortin's book (from 1991, not 2012) is incorrect. The correct reference is:

   F. Brezzi and M. Fortin, Mixed and hybrid finite element methods, vol. 15



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Discussion paper

of Springer Series in Computational Mathematics, Springer-Verlag, New York, 1991.

The authors may also wish to cite the following, which is essentially an updated version of the Brezzi and Fortin book:

D. Boffi, F. Brezzi, and M. Fortin, Mixed finite element methods and applications, vol. 44 of Springer Series in Computational Mathematics, Springer, Heidelberg, 2013."

Thank you for pointing this out. We will correct the references in the introduction and include the updated book by Boffi, Brezzi, and Fortin (2013).

2) "2.1.2. Hybridization and static condensation of mixed methods was apparently first introduced in the following reference:

B. M. Fraejis de Veubeke, Displacement and equilibrium models in the finite element method, in Stress Analysis, O. Zienkiewicz and G. Holister, eds., Wiley, New York, 1965. Reprinted in Internat. J. Numer. Methods Engrg., 52 (2001), pp. 287–342."

The referee is absolutely correct. We will fix this omission in our discussion.

3) "2.1.3. Local post-processing appeared in Arnold and Brezzi (1985) for the hybridized RT method and in Brezzi, Douglas, and Marini (1985) for the hybridized BDM method. The authors frequently cite the former but not the latter when mentioning hybridization and post-processing of mixed methods. Also, the 1991 paper of Stenberg on post-processing is cited elsewhere in the paper but not in the introduction. I believe that these three papers should be cited in the relevant part of the introduction (p. 2, l. 18)."

This will be fixed in the revised paper.

4) "2.2. Section 2.1 is somewhat confusing and could be improved.

   2.2.1. The notation $a(\boldsymbol{c}; \boldsymbol{v})$ originally made me think that $a(\cdot;\cdot)$ was a bilinear form. It took a few times through before I made sense of the notation and realized how linear, bilinear, etc., forms are specified. It might be helpful to include one or two concrete examples before introducing a "general form." This is done nicely in Section 2.1 of Alnaes et al. (2014), which I suggest that the authors emulate."

This is a very good point. We will rework this part of the paper and clarify our notation on bilinear forms to avoid confusion.

5) "2.2.2. The notation for the $\mathcal{I}^c$ and $\mathcal{I}^f$ integrals is also confusing. Presumably $c$ stands for "cell" and $f$ for "facet," but at first I thought $c$ was the coefficient function $\boldsymbol{c}$ and $f$ was some source function, as in equation (10). Since the authors have been using $\mathcal{T}_h$ for cells and $\mathcal{E}_h$ for facets, perhaps a clearer notation would be to call these $\mathcal{I}^{\mathcal{T}}$ , $\mathcal{I}^{\mathcal{E},\circ}$, and $\mathcal{I}^{\mathcal{E},\partial}$. A short sentence mentioning that the three $\mathcal{I}$s correspond to the contributions from the cells, internal facets, and boundary facets, respectively, would make this easier for the reader to understand."

Another good point and an excellent suggestion for improving our notation. We will incorporate these changes and provide further explanation of the individual terms.

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