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Interactive comment on "ELEFANT: a user-friendly multipurpose geodynamics code" *by* C. Thieulot

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Dear Reviewers, dear Editor,

I first wish to thank both reviewers for taking the time to read and comment on my manuscript. I agree with the vast majority of their comments, and I am eager and willing to undertake the work necessary to ultimately bring the paper to their satisfaction. Here are the changes I intend to bring to the manuscript (in what follows DM stands for Dave May's comments, BK for Boris Kaus' comments): - The structure will be changed so that the physics and the numerical implementation are more clearly presented in different sections (DM & BK); novelty and objectives will be stated upfront. A small review of the codes using Q1P0 in the geodynamics community will be added too. - I have been looking at various finite element topologies, have already implemented them and will report on those (which would bring something new to the table) (DM) - I

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am currently working on a benchmark in which pressure is present in the rheology so that order of accuracy tests can be carried out and hopefully show that the element is usable for non-linear pressure-dependent rheologies. (DM) - The title will be changed so as not to mislead the reader into thinking that ELEFANT is an open-source code (DM). - The subroutine where error calculations are computed has been corrected and now computes said errors at the right location. All order of accuracy tests will be re-run. - weak scaling tests for MUMPS will be carried out and reported (BK) - the algebraic multigrid solver performance will be better documented (BK, DM) - additional references to FD/spectral methods will be added (BK) - a clarification of the marker advection scheme will be added, as well as how they are injected, how their local coordinates are found (BK) - since the article submission, I have started to use the SPARSKIT solver library which has allowed me to run geodynamically relevant thermo-mechanically coupled nonlinear 3D models which should also be included to give an idea of how the code performs for such systems.

I further wish to address what I believe to be the two main points of criticism:

-> There is little new in this work (I essentially paraphrase both reviewers). BK correcty states that ELEFANT employs standard techniques which have been employed for the best part of two decades and therefore is comparable to SOPALE, DOUAR, MILAMIN_VEP, CITCOM, GALE, FANTOM, SULEC, . . . These codes have been extensively used, and are still in use today. However, I am not aware of a single publication (article or book) that documents the behaviour of Q1P0(+ALE) codes through extensive benchmarking. These codes have of course been used to run various tests, such as in Crameri et al (2012), Schmeling et al (2008), Buiter et al (geomod 2006 & 2008), Kaus (2010), Thielman et al (2014), and many many others, but the information is diffuse. Susanne Buiter has undertaken such a task with her SULEC code and has shown her benchmark results at geomod 2012, but she hasn't come around writing a paper about SULEC yet although results obtained with her code have appeared in several publications. The Gale manual offers a few examples too but is incomplete and poorly written. In a nutshell, there is no paper I am aware of which puts one single code through the test and attempts to document its behaviour with regards to solvers, plasticity, free surface, large viscosity contrasts, etc . . . (I could of course have overlooked some publications, please prove me wrong if have overlooked a key one). BK states that ‘it would be good to be more upfront about the well-known shortcomings of this kind of code, such that future generations of students can spend their time on solving those issues rather than on repeating them’. In a way, my paper is written with this future student in mind and aims at three things: - showing the limitations of this (popular) approach (which I understand need to be clarified and put upfront more) - showing that it is not that bad: direct solvers will work up to 100ˆ3 or so, simple iterative solvers with a simple preconditioner allow to overcome this limitation for moderate viscosity ratios, all benchmark passed, etc ... - document the behaviour of a code which has been used for the past two years and whose results are already in various publications submitted or in prep. This last point remains in my eyes the main reason why I carried out this work: I wish to be able to refer to a centralised source of information when ELEFANT is used in a publication and I cannot refer to the FANTOM paper because a) the codes are different from a source code point of view b) ELEFANT offers many more features than FANTOM (boundary conditions, surface stabilisation, ‘Uzawa’ scheme, etc ...), c) ELEFANT relies on different (types of) solvers.

-> BK states that the benchmarks I show are essentially too simple and never ’test the full viscoplastic thermo-mechanical code’, and therefore advises me to (for instance) run a subduction model off Taras Gerya’s book. This is of course a valid remark but such tests have been carried out, albeit not published yet (nor included in the manuscript): - I am nearly done running the Buiter et al (Geomod 2008) suite of models which you all three know very well; - I have sent more than a year ago the results of thermo-mechanically coupled visco-plastic subduction models for a benchmark paper done by phd student M. Quinquis (with Susanne Buiter); - I have sent results of a thermo-mechanically coupled visco-plastic convection models for a benchmark paper done by Nicola Tosi - I have re-done the Buiter et al (geomod 2004) experiments

SED
6, C987–C990, 2014

but chose not to show these since I had shown one already in the FANTOM paper, as stated in the text. In all four cases ELEFANT yielded results comparable to other codes. As stated hereabove in the general comments, I am also working on a pressure-dependent rheology benchmark which will be included in the new version of the paper. One could also think of adding a pressure dependency in the slab detachment benchmark I am doing so that all kinds of code can be compared on a somewhat challenging large deformation case involving free surface, pressure-dependent rheologies, multiple materials.

I thereby hope to have convinced you three of why I believe this article should be published and look forward to hearing from you.

Best regards,

Cedric, Utrecht, Sept. 22, 2014.

Interactive comment on Solid Earth Discuss., 6, 1949, 2014.