From: "Journal of Computational Physics" <jcp@elsevier.com>
Subject: JCOMP-D-14-00585R1
Date: March 5, 2015 10:13:11 PM CST
To: quaife@ices.utexas.edu

Re: JCOMP-D-14-00585R1

Dear Dr. Quaife,

The Editorial Office has received the decision on the paper entitled "Adaptive Time Stepping for Vesicle Suspensions".

The reviewers' comments are as follows:

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Dear Authors,

The reviews regarding your revised manuscript have been submitted. As you will see below, reviewer 1 suggests to reject the manuscript. Reviewer 2 suggests some more revisions. For the article to be published, it has to be clear that the new method is really an improvement at least compared to your own previous BDF based fixed dt method that you can very easily compare to, and also that the time step selection is effective. As you will see, reviewer #2 is not convinced about your argument as to why the fixed time step simulation can require many fewer time steps than the adaptive one (same method it seems?), and I do not understand this argument either. Also, I do not understand your reply to Reviewer 1 on a comment on the original manuscript. Here he asks you to show results for examples in your previous work to see that you actually get a reduction in computational time. Your

answer talks about the examples not being robust and the error behaving unpredictably. But these
are indeed the kind of problems that you want to use your new method for? I think it seems most reasonable that you present results for the problems that you have considered with your previous method.

I send this back to you now with a label of "major revisions". I want you to think about if you can really show the advantages of this new method in a convincing way. If so, please revise. If not, then withdraw your article and resubmit when the work is more mature.

Best,

Anna-Karin Tornberg

Reviewer #1: While all suggestions have been commented on, almost non is truly addressed. To design a high-order adaptive scheme inorder to have such a scheme, without demonstrating an improvement to existing schemes, does not qualify for publication in JCP.

Reviewer #2: This revision addresses most of the main criticism raised in the first review
and it much improved. However in a couple of instances, the revisions
should still be improved. I quote below the responses with *, the text with " and my comments in plain text.

* We now mention in the introduction that order reduction is

observed in [29] (used to
be [26]) and that this order reduction was better understood and resolved in [21] (used
to be [18]). This is now discussed at multiple points locations in the manuscript.

*The challenge with using methods described in [21] is that an entire history of the
iterates must be saved since SDC is being used as a preconditioner of a GMRES
iteration ...

I think it is important to separate the cause of order reduction from the Krylov
method in [21]. Order reduction happens because the solution has not
converged to the collocation scheme. This could be remedied by simply doing
many SDC iterations until convergence (as in [47] for stiff problems).
The Krylov method in [21] is simply a way of accelerating that convergence,
but there are other options including recent
multigrid-type ideas from Speck and colloborators.
While the revisions have improved the discussion of order reduction, the
text now suggests that one must either have order reduction or use the method
in [21] which is not the case. The choice made here is to used a fixed
number of iterations and try to manipulate the time step to meet an error
criteria. The alternative is to use more iterations (or acceleration) to

improve the error per step. The fixed iteration choice is defensible given the results,
but the text inappropriately implies that the only other option is to use
the Krylov method. The question of which is more efficient (more time steps with fewer iterations
or fewer time steps with more iterations) is a much more difficult problem.
This should be made clear.

* In order to guarantee that the quadrature error has a negligible effect on the overall
error, we took p sufficiently large that it has an additional three orders of accuracy
than the most accurate time integrator used in each example. This amounts to using
p = 5 for the relaxation flow (the most accurate runs have fifth-order accuracy), and
p = 4 for the remaining flows (the most accurate runs have fourth-order accuracy).

In the text it states:
"For computational efficiency, the accuracy of the quadrature formula should not
exceed the expected rate of convergence."

This is exactly the point I was making. Given your statement, why are you choosing
p so that the quadrature is so accurate? The error is generated not by the quadrature
rule, but by the fact that the thing being integrated is not accurate.
The footnote on page 19 adds confusion
"Recall that p – 1 = 3 additional time steps are required because of the intermediate Gauss-Lobatto quadrature points".

p-1=3 is the number of additional substeps in the SDC formulation and not the
number of time steps (or I am really confused).

* While this seems counterintuitive, this results are correct. The reason is that with
a fixed time step size, almost no error is committed for the entire simulation, except
when the vesicle passes through the constriction. However, with an adaptive time
step size, a much larger time step is taken when the vesicle is not in the constriction,
but an extremely small time step size is required when the vesicle passes through the
constriction.

This makes no sense. Clearly if the adaptive method was forced to never take a time step finer than the fixed time step, the error should be the same in the constriction. If a larger time step can be used elsewhere, then there is a savings. The authors obviously have a
lousy time step selector for this configuration. It is a first order "straw man" argument used to make the second order results seem more
impressive.

NOTE: Additional comments by the reviewers may be available in Elsevier Editorial System (EES). You can find these comments in EES by clicking on "view review attachments". Please contact jcp@elsevier.com if you have any problems opening the reviewer comments in EES.

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In view of these comments made the Associate Editor who guided your article, Professor Anna-Karin Tornberg, has decided that the paper can be reconsidered for publication after major revisions. Therefore we look forward to receiving the revised version of the paper together with a reply to the reports and a summary of the revisions made.

If the revised version is submitted within three months of receipt of this e-mail, the manuscript will retain the original submission date. After three months, your paper might be treated as a new submission and may be sent to new reviewers.

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When submitting your revised paper, we ask that you include the following items:

Response to Reviewers (mandatory)

This should be a separate file labeled "Response to Reviewers" that carefully addresses, point-by-point, the issues raised in the comments appended below. You should also include a suitable rebuttal to any specific request for change that you have not made. Mention the page, paragraph, and line number of any revisions that are made.

Manuscript and Figure Source Files (mandatory)

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Highlights consist of a short collection of bullet points that convey the core findings of the article and should be submitted in a separate file in the online submission system. Please use 'Highlights' in the file name and include 3 to 5 bullet points (maximum 85 characters, including spaces, per bullet point). See the following website for more information
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Graphical Abstracts should summarize the contents of the article in a concise, pictorial form designed to capture the attention of a wide readership online. Refer to the following website for more information: http://www.elsevier.com/graphicalabstracts

The revised version of your submission is due by May 05, 2015.

Yours sincerely,

Soniya Deepak
on behalf of the Editors of Journal of Computational Physics

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