

Thanh Tran-Cong

From: Canh-Dung.Tran@csiro.au
Sent: Thursday, 22 May 2008 3:03 PM
To: Thanh Tran-Cong; David.Phillips@csiro.au
Subject: FW: Revision Requested for Manuscript

FYI.

Best regards

Tcd

-----Original Message-----

From: Tran, Canh-Dung (TFT, Geelong)
Sent: Thursday, 22 May 2008 11:21 AM
To: 'Ravi Jagadeeshan'
Subject: RE: Revision Requested for Manuscript

Dear Prof. Ravi,

Thank you for the reviews.

I will send back you our revised version with modifications and explanation in 60 days.

Best regards,

Tcd

Dr. CD. Tran, Research Scientist, CSIRO
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-----Original Message-----

From: Ravi Jagadeeshan [mailto:ravi.jagadeeshan@eng.monash.edu.au]
Sent: Monday, 19 May 2008 11:26 AM
To: Tran, Canh-Dung (TFT, Geelong)
Subject: Re: Revision Requested for Manuscript

KOREA-AUSTRALIA RHEOLOGY JOURNAL
(<http://www.rheology.or.kr/karj/karj.htm>)

MS-Type: Full Paper

Title: "Computation of dilute polymer solution using BCF-RBFN based method and Domain Decomposition technique"

Authors: Canh-Dung Tran1, David G. Phillips and Thanh Tran-Cong

Dear Tran

I enclose four reviews of your above-referenced manuscript. As you can see, while the reviewers find that your work merits to be published in the Korea-Australia Rheology Journal, the reviewers raise some issues that should be addressed. I hope that you will find their remarks helpful and look forward to receiving the revised version and a detailed

description of the changes you made. At that time I anticipate that a second round of reviewing will be necessary. We would like to receive your revision as soon as possible, 60 days at the latest.

With kind regards,

Ravi

--

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Review A

Review of the paper by Tran et al.

“Computation of dilute polymer solution using BCF-RBFN based method and Domain Decomposition technique”

Overall I think this paper is acceptable for publishing in KARJ. At the same time, however, I would like to bring the authors' attention to the points listed below.

- Authors just mentioned that their results generally agree with Laso's data. It is more helpful if both results could be compared in the same plot.

- The meaning of “wall time” in the concluding remark is not clear.

- Some abbreviations have been used without stating the full words: SDE, TPS-RBF

- Some symbols have been used without the definition:

. < > in Eq. (5)

.-< > in Eqs. (6), (7), (10)

CM_u , CM_t

- The meaning of some symbols, which are not used in the equations, is given in the text:

$p = -p_e(\nabla \cdot u)$ in Eq. (4)

$b' > 0$ in page 5, line 7

B in page 6, line 2

- The resolution of Figs. 3, 4 is too low to read the text in them.

- The symbols in Fig. 5, R_u , R_d , are not consistent with those used in the text.

Review of

Computation of dilute polymer solution using BCF-RBFN based method and domain decomposition technique

Canh-Dung Tran, David G. Phillips and Thanh Tran-Cong

This manuscript proposes an efficient numerical method for solving a micro-macro flow problem of a dilute polymeric solution. In particular, a domain decomposition method is used together with the Brownian configuration field method, and a combined radial basis function and polynomial basis function method. The algorithm is tested by simulating the flow of a FENE fluid in the 10:1 planar contraction. Results are compared with the results of Laso et al. (1999).

The manuscript should be of interest to people in the field. However, it should not be accepted for publication until the following points are fully addressed.

1. The manuscript is not well written and is confusing in many places. The authors need to read through the manuscript carefully and correct the numerous logical and technical mistakes which make the manuscript tedious to read. I will not enumerate all these mistakes here, but will give a sampling.
 - (a) Page 5: b^j is defined, but where is it used?
 - (b) Page 5, Eq. (19): Are the authors taking $\bar{m} = 3$? If so, why?
 - (c) Page 6, Line 2: B is defined after Eqs (20) and (21), but where is it used? Should this B be defined after Eqs (25) and (26) on page 7? If not, what is B in Eq. (25)?
 - (d) Page 7: The authors write “Let Ω_1 and Ω_2 be the boundaries of Ω_1 and Ω_2 ...”
 - (e) Page 7, Eq. (26): Should the subscript of 1 in the third line be a subscript of 2?
 - (f) Page 7: In the equation that defines c^i , should c^{i-1} on the right-hand side be u_2^{i-1} or u_1^i ?
 - (g) Page 8: The sets G_b and G_w are used inconsistently. For example, sometimes the authors write $k \in G_b$ and sometimes they write $\Omega_k \in G_b$.
 - (h) Page 8, Eqs (27) and (28): What does $\Gamma_{kl} \neq 0$ mean? Do they mean $\Gamma_{kl} \neq \emptyset$? If so, then it is not needed, since $\mathbf{x} \in \Gamma_{kl}$ is specified.
 - (i) Table 2: The caption reads “Axisymmetric 10:1.” Should this be “Planar 10:1”?
 - (j) Page 9: Is h really 0.003mm? Also, why write $L_u = 6H$ and $L_d = 60h$? Since h is the characteristic length and $H = 10h$, why not simply write $L_u = L_d = 60h$?
 - (k) Page 9: Eq. (29): Specify the units of λ_H .
 - (l) Page 10: Are $\Delta t = 0.005$ and $\lambda_H = 0.01$ dimensionless here? Why doesn't the value of λ_H here agree with the value of 2 specified in Eq. (29)?
 - (m) Page 11: In the last paragraph, “4” should be “8” or in Table 2, “8” should be “4.”
 - (n) Figure 10: The caption lists three profiles as being plotted, but the graph and its legend only show two profiles.
 - (o) The values of L_u , L_d , R_u and R_d in Fig. 5 appear to have dimensions (since the text cites e.g. $L_d = 60h$ and $h = 0.003\text{mm}$, which would give a value of $L_d = 0.18\text{mm}$). The authors should state the dimensions or give nondimensional values. Also, R_d and R_u should be replaced with h and H for consistency with the text.

- (p) In general, it appears that the values of x_1 and x_2 given on page 11 and in Figs. 6, 9–20 have dimensions (perhaps m?). The authors should state the dimensions or give nondimensional values for these quantities.
 - (q) Are the values of velocity and stress in the figures dimensionless?
2. Figure 6: If the fluid is shear-thinning, then shouldn't its fully-developed velocity profile be more pluggish than the parabolic profile? In particular, shouldn't the centerline velocity be lower for a shear-thinning fluid than for a Newtonian fluid? This figure seems to indicate otherwise.

Concerning the results:

3. Page 11: The authors should say how their vortex shape differs from that of Laso et al. (1999). They should also quantify the size of their vortex and the vortex produced in Laso et al. (1999), and compare the values.
4. Page 11: After reference to Figs (10) and (11), the authors state that their results “generally agree” with those of Laso et al. (1999). They should plot some data points from the graphs in Laso et al. (1999) in Figs (10) and (11) to establish a quantitative comparison.
5. The authors ran simulations with ensemble sizes of 200, 400 and 500 dumbbells (or configuration fields?). How do these compare with the ensemble sizes used in Laso et al. (1999)? Also, what is the size of the statistical errors in these simulations?
6. I see no reason to include both Figure 12 and 13, since they only differ by one curve. Likewise for Figures 14 and 15. Moreover, there is no reason to include Figure 20, since it contains no more information than that given in Figures 18 and 19.

Review C

In this paper a FENE 4:1 contraction flow is solved numerically using the numerical method described by Zerroukat et al. This numerical method is a collocation method that uses radial basis functions and random collocation points.

The first and third authors have already published a paper using this method in the KOREA-AUSTRALIA RHEOLOGY JOURNAL. The originality of the paper is i) to use domain decomposition techniques to lower the CPU time ii) to present numerical results for the 4:1 test case.

The paper deserves in principle to be published. However, the following points have to be addressed before publication, specially the last one.

- Eq. (4) corresponds to a penalty method for the pressure, this should be explicitly written.
- First sentence section 2.1 : in principle, a second order predictor-corrector scheme for an ode is not of second order for a sde !
- Second paragraph section 2.1 : please give a reference.
- After eq. (11) : what is the meaning of the word "neuron" ? Same with "training".
- Eq. (14)(15)(16) : please specify the min and max indices.
- Eq. (17)(18) : not clear, please expand.
- Eq. (22) : how is the min found ? Does this correspond to solving a linear system ? Which method is used for solving (22) ? What is the rate of convergence of the method ?
- Section 3, after the second paragraph : the text is badly placed, this should be in section 2.
- Section 5 : an experimental convergence analysis with respect to m and \bar{m} is missing here.

Review D

This manuscript reports the flows of dilute polymer solutions by combining BCF/RBFN and DD technique. BCF/RBFN was previously reported by the authors, and they combined to overcome the limited resources by applying parallel DD technique. The manuscript is well organized and relatively easy to read, and will deserve for publication in the journal. But I would suggest the authors to change the manuscript before publication. As a reader, it is important to convince how the suggested algorithm or solutions are reliable and precise. The authors refer the solutions of Laso, but there is no direct comparison. They simply mention 'in very good agreement with the results of Laso et al. generally,' or 'the present results generally agree with those obtained by Laso et al.'. The solution of Laso will not be precise because they could manage the solution with only limited number of degrees of freedom. I guess the current simulation will be more precise compared to that of Laso. However direct comparison with numbers or graphs is inevitable to have an idea how this algorithm works and how much it is precise. Also the reproducibility of the simulation needs to be provided. Error bar in one figure will be enough. I think this is a must for every simulation paper.

Minor comments:

- the total number of degrees of freedom needs to be provided
- quality of the figures is poor, and needs to be improved (very hard to read)
- in the figures (or captions), the position at x_2 is hard to read. It would be better to use x_2/H rather than x_2 . then the range will be from zero to one for example, rather than at 0.00020, 0.00047, 0.00114 etc.
- figures 12,13 and 14,15: the position at x_2 looks similar in the caption, but the curves look different.
- figure 13: the peak before $x_1=0.2$ seems strange (too narrow) compared to other figures as well as many previous reports.

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