RETRACTED ARTICLE: A limit analysis of Mindlin plates using the cell-based smoothed triangular element CS-MIN3 and second-order cone programming (SOCP)

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Upon investigation carried out according to External reviewers and Editorial Board Committee on Publication Ethics guidelines, it has been found that the authors of this article had published some inadequate information related to using name of third co-author and research contents. Among two of three authors, they have duplicated with only minor changes of the following article that has been previously submitted to the other journal:

T. Nguyen-Thoi, P. Phung-Van, M.H. Nguyen-Thoi, H. Dang-Trung, An upper-bound limit analysis of Mindlin plates using CS-DSG3 method and second-order cone programming, Journal of Computational and Applied Mathematics, 281:32-48, 2015

Criticisms from the external reviewers

Reviewer #1: After a quick review of the paper published on APJCE, I’ve the following simple remarks.

General comments:

- The paper can be hardly followed by the reader, due to the flow misalignment between the text and the figures/tables referring to the numerical samples, the latter being anticipated in ‘Methods’.

- Bounding features of the formulation should have been clarified, as well as the theoretical reason of shear locking overcome declared by the Authors.

- The paper seems to lack of a sound originality and it is also a fact that reveals ‘almost identical’ to the one previously submitted to J. of Comp. and Appl. Math.

Specific comments:

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- I confirm that a factor $\frac{1}{2}$ was missing in Eq. 33 (in fact, is the same formal error appearing in Eq. 22 of Capsoni & Corradi), but I feel that the authors haven’t introduced this error into the numerical algorithms, as otherwise one should obtain a doubling of the limit multipliers.

- I agree that i.e. Figure 12 should displace doubled dimensions ($2L*2H$, instead of $L*H$), in order to be guarantee coherence the mentioned results from e.g. Capsoni & Corradi or Capsoni & Da Silva.

- Controversial results arise from the numerical section, as i.e. in the trend of collapse multiplier CS-MIN3 in fig.11 (with a suspect ‘locking-like behavior’ for increasing slenderness) and, over that, the trends depicted in fig. 5&6, exhibiting some unexplainable sensitivity on the number of Gauss point and a monotonically reducing trend of the calculated limit loads for both MIN3 and CS-MIN3.

As a conclusion, I think that some explanations referring to these three latter Specific Comments must be acquired from the authors, as they will allow to understand what they really did and finally about the true Scientific relevance of their work.

**Reviewer #2:** The expression (33) of dissipation after a Gauss's quadrature integration through the thickness is incorrect; a factor $\frac{1}{2}$ is missing. Actually, this error is already present in the original paper of Capsoni and Corradi (1999) (equation 22). It is possible that this error may be absent in the code otherwise every result should be impacted by this factor, even in uniaxial simple bending.

One main issue with the strain smoothing technique is that it does not ensure the upper bound status of the computed solution. The authors do not stress this point very often and one can sometimes question the generality of some results. For example, in Figure 5, it may be interesting to see if increasing the number of Gauss points after 7 lead to an asymptotic regime which stays beyond the reference solution of Capsoni or goes below due to the non-bounding but only approximate status of the method. Figure 6 is then a striking example of that. What happens for higher values of the number of Gauss points? The results should reach an asymptotic regime for a high value of Gauss points, this is not the case up to 7 and throw serious doubts on the validity of the results (why then choose 6 Gauss points?). Besides, from my practice of this kind of problem it is extremely surprising that using only 1 Gauss point leads to results that are higher by more than a factor 2!

One big issue is that the paper never defines what $L$ is in the numerical results for the square plate, especially in Figure 4. Does it define the plate whole side length or only the half of it? Inspecting the convergence presented in Figure 6 and 7, it seems that the authors used the same definition as for Capsoni and Corradi, that is $L$ is the half of the plate side, leading to a reference value of 11.546 for the clamped square plate (slenderness ratio of 100) for a basic load of $p=Mp/L^2$ in their paper, so there is no problem when comparing the present results with theirs if this definition is adopted. However, when mentioning the Kirchhoff solution from [33], in this reference $L$ designate the whole side of the plate and the value of $45.07Mp/L^2$ of Figure 11 should be replaced by $45.07Mp/(2L)^2$ to be consistent with the previous definition, leading to a numerical value of 11.27 with the previous basic load. The brown line in Figure 11 is hence completely false.

The problem here is that the authors use it to conclude that their element does not lock in the thin plate limit (infinite slenderness ratio) as it converges to the Kirchhoff solution. First, it does not converge to
this false limit since it did not reach an asymptotic regime for a slenderness ratio of 5000, besides, with the previous remark, it can be now concluded that it clearly diverges from the thin plate solution in the thin plate. Thus, the presented element suffers from shear locking, although less pronounced than the MIN3 element.

Therefore, one main conclusion of this work stating that « As expected, the solutions of the CS-MIN3 converge to the reference Kirchhoff solution [33] when the slenderness ratio is increased to the limit of the thin plate. This hence shows that the CS-MIN3 is free of shear locking in the limit analysis of thin plates. » is very misleading.

For all these reasons, I throw serious doubts on the accuracy of the presented results. It seems to me that the sentences stating that the element is free from shear locking should be erased. The authors have continued publishing the same results in the paper entitled:

T. Nguyen-Thoi, P. Phung-Van, M.H. Nguyen-Thoi, H. Dang-Trung, An upper-bound limit analysis of Mindlin plates using CS-DSG3 method and second-order cone programming, Journal of Computational and Applied Mathematics. Both papers are almost identical from the beginning to the end, the only difference being the type of finite element which is used. No clear comparison is made between those two elements and the same errors are reproduced. This seems quite unacceptable.

On the other hand, the co-author (Canh Le Van) would like remove his name because he it had been put inside without his approbation. Another co-author (P.Phung-Van) recognized the mistakes and accepts retiring the paper.

The Editor-In-Chief wrote a letter to the first author (T. Nguyen-Thoi) for this matter and he agreed to withdraw the paper.

By the way, the Editor-in-Chief had realized a campaign of consultation after his colleagues, specialists of the present matter (Pastor Joseph, Schrefler Bernhard, and de Saxcé Géry…) and they were all agreed with the Editor-in-Chief’s decision.

Finally, it appears to Editor-in-Chief and his Deputy that they have not else but to proceed to the withdrawing of this paper.