Reviewer’s report

Title: The Role of Osteomodulin on Osteo/odontogenic Differentiation in Human Dental Pulp Stem Cells

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Reviewer: Hidefumi Maeda

Reviewer's report:

This study entitled as 'The Role of Osteomodulin on Odontoblastic Differentiation in Human Dental Pulp Stem Cells' suggested the involvement of osteomodulin in odontoblastic differentiation of human pulp stem cells. Authors tried to clarify the new aspect of osteomodulin in dentinogenesis. Data were so interesting. However, suggestions are offered to make this manuscript more convincing.

Major comments

1. In Abstract, although authors described 'little is known about the intracellular effect of OMD', they had not examined its intracellular role in this study. Thus this description should be rewritten.

2. In p6, line 46-48, 'Puromycin treatment for 2 days almost completely eliminated uninfected hDPSCs without affecting the growth rate and morphology of hDPSCs' was confusable. Did the latter 'hDPSCs' mean hDPSCs survived after treatment with puromycin? If so, this should be rewritten.

3. In p7, line 31, 'the identification of genes reprogramming the hDPSCs into odontogenic fate…' is elusive. Does this mean the odontoblastic differentiation of hDPSCs needs 'reprogramming'? If so, authors need to indicate the appropriate references.

4. In Fig.1, did authors use control antibodies whose isotype was matched with fluorescein-conjugated antibodies used for flow cytometry? In addition, as shown in B and C, each differentiation activity appeared low as positive areas were limited while they isolated using the STRO-1 antibody. Thus authors need to discuss this results. Moreover, in C, authors need to show lipid droplets by the magnified view to confirm adipocytic differentiation.

5. In Fig.2, why did authors examine gene expression by qPCR and semi-quantitative PCR? Rather, they should show protein expression by western blotting using the same cells and immunohistochemical staining using rat or mouse tooth germ.
6. In Fig.3, they should describe whether the cells were cultured in GM or OM and the culture period. If cultured in OM, why did control lane in A show very week expression of OMD, compared with Fig.2? If in GM, why did intense expression of OMD was seen in control lane in B? Authors need to explain this discrepancy. In addition, did authors transfec non-targeting control shRNA to control cells? This is very important point. If not, authors must examine such cells as a control. Moreover, authors need to perform the rescue experiment by adding exogenous OMD to shOMD-hDPSC culture system to confirm the role of OMD.

7. Discussion of the mechanism why and how OMD led to the present results was lacked.

Minor comments

1. In p5, line 33, authors need to describe the company of ECL system and an X-ray films used in this study.

2. In p7, line 4, 'Fig. 5' is 'Fig. 4'.

3. In p7, line 50-52, 'hDPSCs were treated with odontoblastic induction medium which contained dexamethasone, L-ascorbic acid and beta-glycerophosphate' should be moved to Materials and Methods.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No
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