**### Task 1 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=rJgDb1SFwB>

**Meta-review:**

The problem of introducing interpretability into sepsis prediction frameworks is one that I find a very important contribution, and I personally like the ideas presented in this paper. However, there are two reviewers, who have experience at the boundary of ML and HC, who are flagging this paper as currently not focusing on the technical novelty, and explaining the HC application enough to be appreciated by the ICLR audience. As such my recommendation is to edit the exposition so that it more appropriate for a general ML audience, or to submit it to an ML for HC meeting. Great work, and I hope it finds the right audience/focus soon.

**Statements that draw on external knowledge:**

Possible sources:

1. Meta-reviewer's knowledge in the specific field

2. Information from the full paper (excluding abstract)

3. Knowledge from related publications

4. Other

Please feel free to add more rows to the table.

| **Text span** | **Possible source** |
| --- | --- |
| one that I find a very important contribution | Meta-reviewer's knowledge in the specific field |
| As such my recommendation is to edit the exposition so that it more appropriate for a general ML audience, or to submit it to an ML for HC meeting. | Meta-reviewer's knowledge in the specific field |

**### Task 2 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=Hye-p0VFPB>

**Meta-review:**

This paper presents an energy-efficient architecture for quantized deep neural networks based on decomposable multiplication using MACs. Although the proposed approach is shown to be somehow effective, two reviewers pointed out that the very similar idea was already proposed in the previous work, BitBlade [1]. As the authors did not submit a rebuttal to defend this critical point, I’d like to recommend rejection. I recommend authors to discuss and clarify the difference from [1] in the future version of the paper. \n\n[1] Sungju Ryu, Hyungjun Kim, Wooseok Yi, Jae-Joon Kim. BitBlade: Area and Energy-Efficient Precision-Scalable Neural Network Accelerator with Bitwise Summation. DAC'2019\n

**Statements that draw on external knowledge:**

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2. Information from the full paper (excluding abstract)

3. Knowledge from related publications

4. Other

Please feel free to add more rows to the table.

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**### Task 3 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=Skgxcn4YDS>

**Meta-review:**

This paper proposes a new method for lifelong learning of language using language modeling. Their training scheme is designed so as to prevent catastrophic forgetting. The reviewers found the motivation clear and that the proposed method outperforms prior related work. Reviewers raised concerns about the title and the lack of some baselines which the authors have addressed in the rebuttal and their revision.

**Statements that draw on external knowledge:**

Possible sources:

1. Meta-reviewer's knowledge in the field

2. Information from the full paper (excluding abstract)

3. Knowledge from related publications

4. Other

Please feel free to add more rows to the table.

| **Text span** | **Possible source** |
| --- | --- |
| (Remove this text when you need to fill the table. If there is no text span to highlight, please keep this and highlight this line of text.) |  |

**### Task 4 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=kK3DlGuusi>

**Meta-review:**

Reviewer rRp9 expressed concerns regarding the theoretical results included in Appendix A. In the discussion (not visible to the authors), the AC and Reviewer zn4a agree that the exposition in the original manuscript was confusing and could lead readers to assume these results were valid for the proposed algorithm. Also, in the original manuscript the presentation of the theoretical results in the appendix was quite poor (e.g. Proposition A.1). Having said that, the contributions and main points of the work are not affected by these observations as it is mainly an empirical study.\n\nFollowing from the previous point, Reviewers rRp9 and zn4a pointed out that the overall presentation of the method, particularly the mathematical presentation could be improved. \n\nReviewer zn4a points out that the method is not particularly novel, this was also indicated as a weakness by Reviewer iyVU. The main contributions of the work are to simultaneously solve the tensor factorization and vector quantization problems usinga form of projected gradient descent (with hard-thresholding). While the empirical results seem promising, are somewhat limited. The authors could make them stronger by studying other applications on top of image classification (e.g. semi-supervised setting, object detection or segmentation).\n\nIn the discussion (not visible to the authors), Reviewer iyVU stated in light of the other reviews, he/she does not oppose rejecting the work.\n\nOverall, the method is technically sound and produces promising results. In its current form, however, the paper is not yet ready for publication. The AC encourages the authors to incorporate the feedback and resubmit the work to a different venue.

**Statements that draw on external knowledge:**

Possible sources:

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2. Information from the full paper (excluding abstract)

3. Knowledge from related publications

4. Other

Please feel free to add more rows to the table.

| **Text span** | **Possible source** |
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**### Task 5 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=rJx2slSKDS>

**Meta-review:**

This paper proposes to improve VAE/GAN by performing variational inference with a constraint that the latent variables lie on a sphere. The reviewers find some technical issues with the paper (R3's comment regarding theorem 3). They also found that the method is not motivated well, and the paper is not convincing. Based on this feedback, I recommend to reject the paper.

**Statements that draw on external knowledge:**

Possible sources:

1. Meta-reviewer's knowledge in the field

2. Information from the full paper (excluding abstract)

3. Knowledge from related publications

4. Other

Please feel free to add more rows to the table.

| **Text span** | **Possible source** |
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**### Task 6 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=gJLEXy3ySpu>

**Meta-review:**

Thank you for your submission to ICLR. The reviewers ultimately have mixed opinions on this paper, but reading in a bit more depth I don't feel that the critical comments raised by the sole negative reviewer really raise valid points. Specifically, the fact that this reviewer directly asks e.g. for comparisons to Levine and Feiz 2019, when the paper (before its revisions) contains an entire section devoted to exactly this comparison, strikes me as not sufficient for a thorough review.\n\nHowever, while I'm thus going to recommend the paper for acceptance (it does present a notable, if somewhat minor, advance upon the state of the art in randomized smoothing), I also feel the paper is generally rather borderline for more straightforward reasons. Specifically, given the \_very\_ narrow focus of the proposed improvements (improvements to the bounds of randomized smoothing, for L0 perturbations, for Top-k accuracy), I ultimately don't think the paper presents that significant an advance in the field. The paper could go other way, thought definitely not doing so due to the issues that the sole critical reviewer takes.

**Statements that draw on external knowledge:**

Possible sources:

1. Meta-reviewer's knowledge in the field

2. Information from the full paper (excluding abstract)

3. Knowledge from related publications

4. Other

Please feel free to add more rows to the table.

| **Text span** | **Possible source** |
| --- | --- |
| (it does present a notable, if somewhat minor, advance upon the state of the art in randomized smoothing) | Meta-reviewer's knowledge in the field |