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

**Source Documents:**

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

**Meta-review:**

This paper introduces a VAE-based generative model of 3D point-clouds inspired by SPAIR that can do unsupervised segmentation, named SPAIR3D. The model uses both global and local latent variables to encode global scene structure as well as individual objects.\n\nThe proposed model is relatively complex, but the presentation is overall clear. \n\nExperimental results on simple synthetic datasets look promising. However, one might argue that for these simple tasks a direct application of a simpler mixture of VAEs (such as IODINE) might be sufficient, so it would be informative to make a direct comparison between these methods and/or show results on a problem clearly out of the scope of these simpler methods (e.g. with high imbalance in the point clouds).

**Statements that draw on external knowledge:**

Possible sources:

1. Meta-reviewer's knowledge in the field

2. Knowledge after reading the full paper (excluding abstract)

4. Other

Please feel free to add more rows to the table.

| **Text span** | **Possible source** |
| --- | --- |
| However, one might argue that for these simple tasks a direct application of a simpler mixture of VAEs (such as IODINE) might be sufficient, so it would be informative to make a direct comparison between these methods and/or show results on a problem clearly out of the scope of these simpler methods (e.g. with high imbalance in the point clouds). | Meta-reviewer's knowledge in the field |

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

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=kSqyNY_QrD9>

**Meta-review:**

The paper considers the problem of solving time-constrained multi-robot task allocation (MRTA) problems. Formulating the problem as a Markov decision process (MDP), the paper proposes Covariant Attention-based Mechanism (CAM), a graph neural network-based policy that can be trained to solve MRTA problems via standard RL methods. The encoder adapts the covariant compositional network to improve generalizability, while the decoder extends a recent combinatorial optimization architecture to the multi-agent optimization domain. Experimental results demonstrate that CAM outperforms an encoder-decoder baseline in terms of task completion, generalizability, and scalability, while also providing greater computational efficiency than non-learning baselines.\n\nThe paper considers an important topic---multi-agent task allocation is an interesting and challenging combinatorial optimization problem. The proposed CAM architecture adapts existing components in an interesting way and seems sensible for the MRTA domain. The reviewers initially raised concerns regarding the conclusions that can be drawn from the experimental evaluation, the significance of the algorithmic contributions, as well as the motivation for the proposed approach. The authors made a concerted effort to address these concerns through the addition of new experimental evaluations (e.g., comparisons to a myopic baseline and ablation studies), updates to the text, and detailed responses to each reviewer. Unfortunately, only one reviewer responded and updated their review (increasing their score). In light of this, the AC also reviewed the paper. The AC agrees with the strengths identified by the reviewers (including those noted above) and with the contributions provided by the additional evaluations. However, the paper remains unnecessarily dense, while at the same time not being self-contained (e.g., the new experimental results are relegated to the appendix rather than appearing in the main text). The paper would also benefit from a more concise motivation for learning-based solutions to MRTA and a clearer discussion of the paper's contributions.

**Statements that draw on external knowledge:**

Possible sources of external knowledge:

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

1.Judgemental knowledge (?%)

2.Extra content knowledge: (?%)

2.1. Information from the full paper (excluding abstract)

2.2. Knowledge from related publications

2.3. Other (personal?, based on pilot study)

Please feel free to add more rows to the table.

| **Text span** | **Possible source** |
| --- | --- |
| multi-agent task allocation is an interesting and challenging combinatorial optimization problem | Meta-reviewer's knowledge in the field |
| The authors made a concerted effort to address these concerns through the addition of new experimental evaluations (e.g., comparisons to a myopic baseline and ablation studies), updates to the text, and detailed responses to each reviewer. Unfortunately, only one reviewer responded and updated their review (increasing their score). In light of this, the AC also reviewed the paper. The AC agrees with the strengths identified by the reviewers (including those noted above) and with the contributions provided by the additional evaluations. | Other |
| The paper would also benefit from a more concise motivation for learning-based solutions to MRTA and a clearer discussion of the paper's contributions. | Meta-reviewer's knowledge in the field |

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

**Source Documents:**

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

**Meta-review:**

The reviewers are overall positive. The presented approach is mathematically sound and shows that grid cells can emerge from an optimization framework with minimal assumptions. The main criticism raised is whether the approach is useful beyond already existing models. The authors should clarify potential applications more explicitly in the revision, as well as include the additional experiments they cited in the rebuttal.\n

**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** |
| --- | --- |
| The authors should clarify potential applications more explicitly in the revision, as well as include the additional experiments they cited in the rebuttal. | Meta-reviewer's knowledge in the field |

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

**Source Documents:**

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

**Meta-review:**

Two reviewers are negative on this paper while the other reviewer is slightly positive. Overall, the paper does not make the bar of ICLR. A reject is recommended.

**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** |
| --- | --- |
| Overall, the paper does not make the bar of ICLR. | Meta-reviewer's knowledge in the field |

**### Task 5 ###**

**Source Documents:**

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

**Meta-review:**

This work has a lot of promise; however, the author response was not sufficient to address the concerns expressed by reviewer 1, leading to an aggregate rating that is just not sufficient to justify an acceptance recommendation. The AC recommends rejection.

**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** |
| --- | --- |
| however, the author response was not sufficient to address the concerns expressed by reviewer 1, leading to an aggregate rating that is just not sufficient to justify an acceptance recommendation | Other (review process) |

**### Task 6 ###**

**Source Documents:**

Link to OpenReview: https://openreview.net/forum?id=S1g490VKvB

**Meta-review:**

Using ideas from mean-field theory and statistical mechanics, this paper derives a principled way to analyze signal propagation through gated recurrent networks. This analysis then allows for the development of a novel initialization scheme capable of mitigating subsequent training instabilities. In the end, while reviewers appreciated some of the analytical insights provided, two still voted for rejection while one chose accept after the rebuttal and discussion period. And as AC for this paper, I did not find sufficient evidence to overturn the reviewer majority for two primary reasons.\n\nFirst, the paper claims to demonstrate the efficacy of the proposed initialization scheme on multiple sequence tasks, but the presented experiments do not really involve representative testing scenarios as pointed out by reviewers. Given that this is not a purely theoretical paper, but rather one suggesting practically-relevant initializations for RNNs, it seems important to actually demonstrate this on sequence data people in the community actually care about. In fact, even the reviewer who voted for acceptance conceded that the presented results were not too convincing (basically limited to toy situations involving Cifar10 and MNIST data).\n\nSecondly, all reviewers found parts of the paper difficult to digest, and while a future revision has been promised to provide clarity, no text was actually changed making updated evaluations problematic. Note that the rebuttal mentions that the paper is written in a style that is common in the physics literature, and this appears to be a large part of the problem. ICLR is an ML conference and in this respect, to the extent possible it is important to frame relevant papers in an accessible way such that a broader segment of this community can benefit from the key message. At the very least, this will ensure that the reviewer pool is more equipped to properly appreciate the contribution. My own view is that this work can be reframed in such a way that it could be successfully submitted to another ML conference in the future.

**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** |
| --- | --- |
| two still voted for rejection while one chose accept after the rebuttal and discussion period. | Other (review process) |
| And as AC for this paper, I did not find sufficient evidence to overturn the reviewer majority for two primary reasons | Meta-reviewer's knowledge in the field |
| no text was actually changed making updated evaluations problematic. | Other (review process) |
| and this appears to be a large part of the problem. ICLR is an ML conference and in this respect, to the extent possible it is important to frame relevant papers in an accessible way such that a broader segment of this community can benefit from the key message. At the very least, this will ensure that the reviewer pool is more equipped to properly appreciate the contribution. My own view is that this work can be reframed in such a way that it could be successfully submitted to another ML conference in the future. | Meta-reviewer's knowledge in the field |