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

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=S1g_S0VYvr>

**Meta-review:**

The paper received mixed reviews: R (R3), WA (R2), A (R1). AC has read the reviews, rebuttal and paper. AC is concerned about the short planning horizon, which seems like a major issue: (i) as R1 notes, most MPC algorithms use much longer horizons as they find it helps performance and (ii) the claim of the approach to be able to pick the planning horizon is moot if its dynamic range is small. Overall, the paper is very borderline. The idea is interesting but without addressing longer horizons, the contribution is limited. Under guidance from the PCs, the AC feels that the paper just falls below the acceptance threshold and thus cannot be accepted unfortunately. The work is definitely interesting however and should be revised for a future submission. \n\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** |
| --- | --- |
| (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 2 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=cFpWC6ZMtmj>

**Meta-review:**

All the reviewers found interesting the use of Shapley values to provide feature attributions for fairness, however, the reviewers brought up a number of issues, particularly in terms of presentation and clarity. While the authors' responses did clarify some of these concerns, this was not enough for the reviewers to broadly support acceptance.

**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 3 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=Bye8hREtvB>

**Meta-review:**

The paper proposes learning a latent embedding for image manipulation for PixelCNN by using Fisher scores projected to a low-dimensional space.\nThe reviewers have several concerns about this paper:\n\* Novelty\n\* Random projection doesn’t learn useful representation\n\* Weak evaluations\nSince two expert reviewers are negative about this paper, I cannot recommend acceptance at this stage.\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** |
| --- | --- |
| (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=HJeNIjA5Y7>

**Meta-review:**

Reviewers are in full agreement for 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** |
| --- | --- |
| (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 5 ###**

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=S1x522NFvS>

**Meta-review:**

This paper makes a connection between one-class neural networks and the unsupervised approximation of the binary classifier risk under the hinge loss. An important contribution of the paper is the algorithm to train a binary classifier without supervision by using class prior and the hypothesis that class conditional classifier scores have normal distribution. The technical contribution of the paper is novel and brings an increased understanding into one-class neural networks. The equations and the modeling present in the paper are sound and the paper is well-written.\n\nHowever, in its current form, as pointed out by the reviewers, the experimental section is rather weak and can be substantially improved by adding extra experiments as suggested by reviewers #1, #2. Since its submission the paper has not yet been updated to incorporate these comments. Thus, for now, I recommend rejection of this paper, however on improvements I'm sure it can be a good contribution in other conferences.

**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** |
| --- | --- |
| Thus, for now, I recommend rejection of this paper, however on improvements I'm sure it can be a good contribution in other conferences. | Meta-reviewer's knowledge in the field |
| The equations and the modeling present in the paper are sound and the paper is well-written | Information from the full paper (excluding abstract) |

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

**Source Documents:**

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

**Meta-review:**

This well-written paper introduces an improved exploration strategy by exploiting knowledge about sequences of actions that lead to the same state. The idea is straightforward and easy to understand and apply, which makes it potentially interesting. An important downside is the limited applicability of the method, as there mainly seems to be an advantage in (mostly deterministic) grid-like MDPs. In addition, priors about action-sequence equivalences have to be available. Overall, the contribution of the paper is not deemed significant enough for publication at a top-tier conference like ICLR by the majority of the reviewers as well as myself. For these reasons, I recommend 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** |
| --- | --- |
| Overall, the contribution of the paper is not deemed significant enough for publication at a top-tier conference like ICLR by the majority of the reviewers as well as myself. | Meta-reviewer's knowledge in the field |