# View Reviews

**Paper ID**
4386

**Paper Title**
Distributed Lossy Image Compression with Recurrent Networks

**Reviewer #1**

## Questions

**1. Summary. In 3-5 sentences, describe the key ideas, experiments, and their significance.**
Based on distributed source coding, the authors proposed a deep neural network based image compression framework using distributed encoders and joint decoders.

**2. What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.**
- Applied distributed source coding to deep neural networks
- compression quality is scalable

**3. What are the weaknesses of the paper? Clearly explain why these aspects of the paper are weak.**
1. The application of DSC to deep neural net is positive, but the codec compression experiment was conducted by MNIST only which is quite a simple dataset which is not normally used for codec compression.

Other papers on compression have been tested on HD RGB, such as KODAK set, but this paper only targets MNIST set which is normalized from 28x28 to 32x32 sized images. No matter how good the performance is, this is not an experiment that can show value as a codec.

2. There is no comparison of the actual codec performance. Although the proposed method shows the best PSNR value, there is no meaningful difference between Toderici using the existing recurrent neural networks. (Despite the MNIST is not the target of Toderici.)

4. Improving the performance of the codec using recurrent deep learning is also proposed in the previous paper. In this paper, it is difficult to find new ideas.

5. There is insufficient analysis of correlation such as how the network sees correlation between data sources.

**4. Paper rating (pre-rebuttal)**
Strong reject

**5. Justification of rating. What are the most important factors for your overall recommendation?**
In this paper, the authors experiment and evaluate the codec based on MNIST. This way of comparison is not fair since the other papers compared are based on other data sets, such as the KODAK test set.

Therefore, even if the authors insist that they made the best compression codec, based on MNIST only, it can not be accepted as a meaningful or useful paper.

**10. Final recommendation based on ALL the reviews, rebuttal, and discussion (post-rebuttal)**
Strong reject

**11. Final justification (post-rebuttal)**
There is no author feedback and I stick to my initial rating.

**Reviewer #2**

## Questions

**1. Summary. In 3-5 sentences, describe the key ideas, experiments, and their significance.**

This paper presents a new architecture for distributed image compression from a group of distributed data sources. The proposed method consists of encoder and decoder which are on deep recurrent models. Based on the authors' best knowledge, this is the first data-driven DSC framework for general distributed code design with Deep learning. In the evaluation, authors use MNIST dataset with multiple splits to make distributed source coding environment. The proposed method (DSC framework) achieved near performance to the theoretical upper bound (Joint training framework).

**2. What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.**

The compression quality of the proposed model is scalable. Data can be efficiently compressed at different bit rates with a single recurrent model.

The proposed method is the first attempt data-driven DSC framework for image compression based on Deep learning and it achieved near performance to the theoretical upper bound without careful data synchronization.

**3. What are the weaknesses of the paper? Clearly explain why these aspects of the paper are weak.**

First, it is not clear to me the relationship between the Distributed Source Coding and proposed model. What I understand is to model distributed source scenario authors designed multiple independent encoders and shared single decoder, but I expect the more explicit design to handle that scenario (For example, new regularization or any other specialized module).

Second, the experimental setting may have some issues for image compression task. Authors use MNIST dataset solely for evaluating the proposed model. To evaluate image compression performance, other datasets such as Kodak and RAISE are highly recommended because they have natural images. It is not clear to me whether the proposed model works well for natural images or not. Also, for designing DSC framework, the subsets of MNIST may not be suitable for representing different sources.

**4. Paper rating (pre-rebuttal)**

Weak reject

**5. Justification of rating. What are the most important factors for your overall recommendation?**

I agree with the proposed method is the first data-driven DSC framework for general distributed code design with Deep Learning, but the experimental setup for image compression is not convinced to me.

The relationship between the proposed model and DSC framework seems to be weak. The comparison result with previous methods which is not designed for DSC framework might be required.

**6. Additional comments.**

The result of more complex datasets (Kodak or RAISE) for image compression is required for validating the authors' claim.

**10. Final recommendation based on ALL the reviews, rebuttal, and discussion (post-rebuttal)**

Weak reject

**11. Final justification (post-rebuttal)**

The proposed algorithm is about image compressing, however the experimental results are limited on small and non-realistic dataset (MNIST only). For image compressing algorithm or architecture, more experimental results are required.

**Reviewer #3**

# Questions

**1. Summary. In 3-5 sentences, describe the key ideas, experiments, and their significance.**

This paper proposes a deep learning based parametric distributed lossy image compression method. In essence, a recurrent neural network at every data source is used to encode image residuals, which, after binarization, can be decoded back to images. The decoder is shared among all encoders. Experiments are carried out on the MNIST dataset. Peak signal-to-noise ratio (PSNR) is the metric used for comparing against some baseline methods and existing compression techniques, such as JPEG.

**2. What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.**

1. The authors try to establish a connection between their proposed method and the Slepian-Wolf Theorem, which provides some nice theoretical foundation for readers to understand this paper's context.

2. Sufficient technical details are provided for reproducing the results.

**3. What are the weaknesses of the paper? Clearly explain why these aspects of the paper are weak.**

1. I'm slightly confused about the claimed advantage of using distributed encoders. In the experiments, from what I understand the same recurrent neural network architecture is used in both the joint and the distributed settings, right? If so, the computation and memory consumption is multiplied by the number of encoders in the distribute setting, while the performance degrades from the joint setting. (Consider the same number of iterations.) Then how valuable is the method in practice?

2. The authors have mentioned a couple of times the correlations among data sources can be implicitly learned in their proposed method. However, no experiments corroborate this claim.

3. MNIST is such a toy dataset that testing only on this dataset isn't persuasive enough.

4. How does the proposed method fit into the Slepian-Wolf Theorem (as shown in Fig. 3)? After reading paper, I couldn't establish a clear connection between the proposed method and the Slepian-Wolf Theorem. The proposed method seems to me just a recurrent network based autoencoder.

5. For the result shown in Fig. 4, I'm not entirely convinced by its significance, since the proposed parametric method more or less overfits to the domain of MNIST images, while codecs such as JPEG are more generic.

**4. Paper rating (pre-rebuttal)**

Weak reject

**5. Justification of rating. What are the most important factors for your overall recommendation?**

If the paper could base their approach more off the Slepian-Wolf Theorem, its impact would be huge. Otherwise, I don't see sufficient novelty from this paper. And the experiments only consider the MNIST dataset, which is perhaps too simple to corroborate any claim.

**10. Final recommendation based on ALL the reviews, rebuttal, and discussion (post-rebuttal)**

Weak reject

**11. Final justification (post-rebuttal)**

My recommendation stays the same, as the authors didn't provide a rebuttal.