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

**Source Documents:**

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

**Meta-review:**

This paper presents some new results on near-optimum algorithms for distributed optimization, nearly matching lower bounds. Most of the reviewers are positive about the contributions of this work. However, one issue that came up is the assumption of bounded gradient dissimilarity, which is essentially a gap between upper and lower bounds. While I am recommending to accept this paper, I believe this gap should be more prominently discussed in the abstract and introduction. \n\n

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

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=CSw5zgTjXyb>

**Meta-review:**

The paper proposes a model of agent collaboration to improve outcomes for any participating agent in a setting where every agent does not always benefit from collaborating with all other agents. The reviewers did find some of the theoretical results interesting, however, in its current (revised) form, they still argued during the discussion post-rebuttal that: (i) the game theoretic formulation of this problem is not entirely new and has been studied in various forms before and (ii) the particular application of the results to federated learning comes after making various (questionable) assumptions. I would encourage the authors to take into account (i-ii) for preparing a revised version of their paper and resubmit to another conference.

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

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=swbAS4OpXW>

**Meta-review:**

This work was the subject of significant back and forth (between authors and reviewers, but also between reviewers & myself) due to the wide range of opinions. Two of the reviewers have found this work below the bar: they have provided multiple reasonings that I would rather not repeat here. The third reviewer found this work more compelling and argued for its acceptance. My attempts at reaching a consensus have yielding the following conclusions:\n\n \* There's agreement that one-shot generation is indeed a challenging task\n \* Some of the results are indeed impressive, but many results are not compelling.\n \* The rebuttal addressed some of the concerns (e.g. visualization of latents), but some issues are unaddressed (e.g. more motivation, explanation of why the proposed method works better)\n \* One of the reviewers has argued rather forcefully that the work doesn't quite do domain adaptation in the typically understood sense. Moving beyond definitions of domain adaptation, the same reviewer was not very convinced by the quality of the results themselves.\n \* The reviewer most positive about this work agrees that this work only explores a limited form of domain transfer. They argued that some of the potential applications of this work do make the submission interesting. \n\nFundamentally, the discussion did not necessarily resolve the differences in opinion one way or another. Ultimately, all 3 reviewers believe that it would fine if this work was not accepted to ICLR at this time, despite some of the interesting results and promise. Given the discussion and this mildest consensus, I am inclined to recommend rejection too. I do think there's a substantial amount of constructive feedback in the reviews that would make a subsequent revision of this work quite a bit better.

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

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=SkgKO0EtvS>

**Meta-review:**

It seems to be an interesting contribution to the area. I suggest acceptance.

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

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=H1eKT1SFvH>

**Meta-review:**

This works presents a method for inferring the optimal bit allocation for quantization of weights and activations in CNNs. The formulation is sound and the experiments are complete. However, the main concern is that the paper is very similar to a recent work by the authors, which is not cited.

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

**Source Documents:**

Link to OpenReview: <https://openreview.net/forum?id=q1yLPNF0UFV>

**Meta-review:**

The main concerns about this work shared by the reviewers were around novelty and presentation. One reviewer felt that a key idea underlying the work has already featured in several other works in recent years, and thus the novelty of the work is limited. I am inclined to agree with this, however, I believe this work could be a useful reference to help more people understand the intricacies of OOD detection and the biases of likelihood-based models (especially autoregressive models), and the practical demonstration that a tiny local model is all you need for efficient lossless image compression, is valuable in itself.\n\nTherefore, I have decided to recommend acceptance. This is of course conditional on the authors including in the manuscript all the additional results they have provided, and reworking the parts that were deemed unclear as set out in the authors' responses.