Evaluating Large-Vocabulary Object Detectors:  
The Devil is in the Details

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Abstract

By design, average precision (AP) for object detection aims to treat all classes independently: AP is computed independently per category and averaged. On one hand, this is desirable as it treats all classes equally. On the other hand, it ignores cross-category confidence calibration, a key property in real-world use cases. Unfortunately, under important conditions (i.e., large vocabulary, high instance counts) the default implementation of AP is neither category independent, nor does it directly reward properly calibrated detectors. In fact, we show that on LVIS the default implementation produces a gameable metric, where a simple, unintuitive re-ranking policy can improve AP by a large margin. To address these limitations, we introduce two complementary metrics. First, we present a simple fix to the default AP implementation, ensuring that it is independent across categories as originally intended. We benchmark recent LVIS detection advances and find that many reported gains do not translate to improvements under our new evaluation, suggesting recent improvements may arise from difficult to interpret changes to cross-category rankings. Given the importance of reliably benchmarking cross-category rankings, we consider a pooled version of AP (AP_{Pool}) that rewards properly calibrated detectors by directly comparing cross-category rankings. Finally, we revisit classical approaches for calibration and find that explicitly calibrating detectors improves state-of-the-art on AP_{Pool} by 1.7 points.

1. Introduction

The task of object detection is commonly benchmarked by the mean of a per-category performance metric, usually average precision (AP) [4, 18]. This evaluation methodology is designed to treat all categories independently: the AP for each category is determined by its confidence-ranked detections and is not influenced by the other categories. On one hand, this is a desirable property as it treats all classes equally. On the other hand, it ignores cross-category score calibration, a key property in real-world use cases.

Surprisingly, in practice object detection benchmarking diverges from the goal of category-independent evaluation. Cross-category interactions enter into evaluation due to a seemingly innocuous implementation detail: the number of detections per image, across all categories, is limited to make evaluation tractable [18, 6]. If a detector would exceed this limit, then a policy must be chosen to reduce its output. The commonly used policy ranks all detections in an image by confidence and retains the top-scoring ones, up to the limit. This policy naturally outputs the detections that are most likely to be correct according to the model.
However, this natural policy is not necessarily the best policy given the objective of maximizing AP. We will demonstrate a counterintuitive result: there exists a policy, which can achieve higher AP, that discards a well-chosen set of higher-confidence detections in favor of promoting lower-confidence detections; see Figure 1. We first derive this result using a simple toy example with a perfectly calibrated detector. Then, we show that given a real-world detection model, we can employ this new ranking policy to improve AP on the LVIS dataset [6] by a non-trivial margin. This policy is unnatural because it directly contradicts the model’s confidence estimates—even when they are perfectly calibrated—and shows that AP, as implemented in practice, can be vulnerable to gaming-by-re-ranking.

This analysis reveals that the default AP implementation neither achieves the goal of being independent per class nor, to the extent that it involves cross-category interactions, does it measure cross-category score calibration with a principled methodology. Further, the metric can be gamed. To address these limitations, first we fix AP to make it truly independent per class, and second, given the practical importance of calibration, we consider a complementary metric, \( \text{AP}^{\text{Pool}} \), that directly measures cross-category ranking.

Our fix to the standard AP implementation removes the detections-per-image limit and replaces it with a per-class limit over the entire evaluation set. This simple modification leads to tractable, class-independent evaluation. We examine how recent advances on LVIS fare under the new evaluation by benchmarking recently proposed loss functions, classifier head modifications, data sampling strategies, network backbones, and classifier retraining schemes. Surprisingly, we find that many gains in AP stemming from these advances do not translate into improvements for the proposed category-independent AP evaluation. This finding shows that the standard AP is sensitive to changes in cross-category ranking. However, this sensitivity is an unintentional side-effect of the detection-per-image limit, not a principled measure of how well a model ranks detections across categories.

To enable more reliable benchmarking, we propose to directly measure improvements to cross-category ranking with a complementary metric, \( \text{AP}^{\text{Pool}} \). \( \text{AP}^{\text{Pool}} \) pools detections from all classes and computes a single precision-recall curve — the detection equivalent of micro-averaging from the information retrieval community [20]. To optimize \( \text{AP}^{\text{Pool}} \), true positives for all classes must rank ahead of false positives for any class, making it a principled measure of cross-category ranking. We extend simple score calibration approaches to work for large-vocabulary object detection and demonstrate significant \( \text{AP}^{\text{Pool}} \) improvements that result in state-of-the-art performance.

2. Related work

**Large-vocabulary detection.** Object detection research has largely focused on small-to-medium vocabularies (e.g., 20 [4] to 80 [18] classes), though notable exceptions exist [2, 10]. Recent detection benchmarks with hundreds [34, 15] to over one-thousand classes [6] have renewed interest in large-vocabulary detection. Most approaches re-purpose models originally designed for small vocabularies, with modifications aimed at class imbalance. Over-sampling images with rare classes to mimic a balanced dataset [6] is simple and effective. Another strategy leverages advances from the long-tail classification literature, including classifier re-training [12, 33] and using a normalized classifier [19, 29].

Finally, recent work proposes several new loss functions to reduce the penalty for predicting rare classes, e.g., equalization loss (EQL) [19], balanced group softmax (BaGS) [16] or the CenterNet2 Federated loss [35]. We analyze these advances in large-vocabulary detection, finding that a number of them do not show improvements under our fixed, independent per-class AP evaluation, indicating that they improve existing AP by modifying cross-category rankings.

**Detection evaluation.** Average precision (AP) is the most common object detection metric, used by PASCAL [4], COCO [18], OpenImages [15], and LVIS [6]. Conceptually, AP evaluates detectors independently for each class. We show that common implementations deviate from this conceptual goal in important ways, and propose potential fixes and alternatives. Prior work analyzing AP focuses on comparisons across classes, e.g. Hoiem et al. [9] present a normalized average precision (AP\(_N\)) and Zhang et al. [33] propose ‘sampled AP’, but does not expose the issues covered in this paper. Our procedural fix for AP computation removes the impact of cross-category scores on evaluation, and thus we propose a variant, \( \text{AP}^{\text{Pool}} \), which explicitly rewards better cross-category rankings. From an information retrieval perspective, \( \text{AP}^{\text{Pool}} \) is the micro-averaging counterpart to AP [20], which evaluates macro-averaged performance, and has been used as a diagnostic in prior work [3].

**Model calibration.** A well-calibrated model is one that provides accurate probabilistic confidence estimates. Calibration has been explored extensively in the classification setting, including parametric approaches, such as Platt scaling [23] and beta calibration [13], and non-parametric approaches, such as histogram binning [31], isotonic regression [32], bayesian binning into quantiles (BBQ) [21]. While small neural networks tend to be well-calibrated [22], Guo et al. [5] show that deep networks are heavily uncalibrated. Kuppers et al. [14] extend this analysis to deep network based object detectors and show that size and position of predicted boxes helps reduce calibration error. We also apply calibration strategies to object detectors, but find that per-class calibration is crucial for improving \( \text{AP}^{\text{Pool}} \).
3. Pitfalls of AP on large-vocabulary detection

Through both toy and real-world examples, we show that cross-category scores impact AP in counterintuitive ways.

3.1. Background

The standard object detection evaluation aims to evaluate each class independently. In practice, however, this independence is broken due to an apparently harmless implementation detail: to evaluate efficiently, benchmarks limit the number of detections a model can output per image (e.g. to 100). In practice, this limit is set (hopefully) to be high enough that detections beyond it are unlikely to be correct. Importantly, this limit is shared across all classes, implicitly requiring models to rank predictions across classes.

Our analysis shows that this detections-per-image limit, when used with a class-balanced evaluation like AP, can enable an unintuitive ranking policy to perform better than the natural policy of ranking detections by their estimated confidence. The effect size is correlated with increasing the number of categories or the average instances per category.

3.2. Analysis

A toy example. Consider a toy evaluation on a dataset with two classes, as shown in Figure 2. For simplicity, suppose we have access to a detector that perfectly calibrated, when the model outputs a prediction with confidence \( s \) (e.g. 0.3), the prediction is a true positive 100 \( \cdot \) \( s \% \) (e.g. 30\%) of the time. We consider evaluating this model’s outputs under two different rankings, using the standard class-balanced AP evaluation with a limit of two detections per image.

Under this setting, consider the predictions w.r.t. two possible groundtruth scenarios in Figure 2a. The model predicts two instances for class A (A1, A2) with confidence 1.0, and one instance for class B (B1), with confidence 0.8. Since the model is perfectly calibrated (by assumption), we know A1 and A2 are true positives 100\% of the time, while B1 is a true positive 80\% of the time.

With these predictions, consider the two potential rankings depicted in Figure 2b. Ranking 1 appears ideal: it ranks more confident detections before lower confident ones, as is standard practice. By contrast, Ranking 2 is arbitrary: B1 is ranked above A2, despite having lower confidence.

Surprisingly, Ranking 2 outperforms Ranking 1 under the AP metric with a limit of two detections per image, as shown in Figure 2c and Figure 2d. While Ranking 1 gets a perfect AP of 1.0 for class A, it gets 0 AP for class B, leading to an overall AP of 0.5. By contrast, while Ranking 2 leads to a lower AP for class A (0.5), it scores an expected AP of 0.8 for class B, yielding an overall AP of 0.65.

Of course, this is a toy scenario, concocted to highlight an evaluation pitfall using an artificially low detections-per-image limit of only two predictions. We now show that a similar effect exists for a real-world detection benchmark.
Table 1. Unintuitive Ranking 2 (Figure 2b) improves LVIS AP. Artificially limiting the number of detections per class across the entire validation set leads to higher LVIS AP when using the standard limit of 300 detections per image, perhaps paradoxically. In Figure 1 we show how this ranking policy (which, again, improves AP) suppresses some higher-confidence detections in favor of detections that the model estimates are more likely incorrect.

| dets/class | dets/im | AP | AP$_r$ | AP$_c$ | AP$_t$ |
|------------|---------|----|--------|--------|--------|
| ∞ (Ranking 1) | 300 | 22.6 | 12.6 | 21.1 | 28.6 |
| 10,000 (Ranking 2) | 300 | **23.8** (+1.2) | 15.5 (+2.9) | **22.7** | 28.5 |

Table 2. Increasing the limit on detections per image significantly improves LVIS AP. AP improves by over 7 points (over 50% relative), indicating many accurate rare class predictions are ignored due to the default limit of 300 detections per image. By contrast, this limit does not significantly impact COCO, which contains a significantly smaller vocabulary.

| dets/im | LVIS | COCO |
|---------|------|------|
| 100     | 18.2 | **37.4** |
| 300     | 22.6 | 37.5 (+0.1) |
| 1,000   | 25.0 (+2.2) | 37.5 (+0.1) |
| 2,000   | 25.6 (+3.0) | 37.5 (+0.1) |
| 5,000   | 26.0 (+3.4) | 37.5 (+0.1) |
| 10,000  | 26.1 (+3.5) | 37.5 (+0.1) |

When is gameability an issue? Given the impact of the detections-per-image limit on LVIS, a natural question is whether this also affects the widely used COCO dataset. Table 2 shows that increasing this limit does not significantly change COCO AP, suggesting the limit has not negatively impacted COCO evaluation. We hypothesize that this is due to the significantly smaller vocabulary in COCO relative to the detections limit: with only 80 classes, detections beyond the top 100 per image are unlikely to impact AP.

To analyze this hypothesis, we evaluate on subsets of LVIS. Given a baseline model trained on LVIS, we restrict its predictions to a subset of classes, and report AP with a low and a high detections-per-img limit in Table 3. We find that on subsets which have small vocabularies and few labeled instances per class, the gap between AP in the two settings is small (0.4-0.5 points). However, when there are many labeled instances in the evaluation set (as with the ‘F’ subset), or the vocabulary is large (as with the second and third blocks of the table), the gap is much higher. This suggests that AP is sensitive to the detections limit on large vocabulary datasets, particularly if they contain many labeled instances per image.

4. AP without cross-category dependence

We now address this undesirable interaction between AP and cross-category scores. We have already diagnosed that this interaction is caused by the detections-per-image limit. In theory, then, the solution is simple: don’t limit the number of detections per image. Of course, this is impossible in practice, as we cannot evaluate infinite detections. How, then, can we approximate this hypothetical evaluation?

Higher detections-per-image limit. A natural option is to have a large, but finite, detections-per-image limit. Predictions beyond a very high limit are exceedingly unlikely to be correct, and thus may not affect the evaluation. Indeed, Table 2 shows that increasing the limit beyond 5,000 does not significantly affect AP. Unfortunately, this results in prohibitively slower evaluation: on LVIS validation, a baseline
model’s outputs are $15 \times$ larger using a limit of 5,000 detections than at the default limit of 300 (37GB vs. 2.4GB). Moreover, submitting such results to an evaluation server, as required for the LVIS test sets, is impractical.

**Limit detections-per-class.** We now present an alternative, tractable implementation. Rather than discarding low-scoring detections per image, we discard low-scoring detections per class across the dataset. That is, given a model’s output on the evaluation set, the benchmark would only evaluate the top $k$ predictions per class, discarding the rest.

We find that this strategy significantly reduces the storage and time requirements for evaluation. Table 4 shows that limiting detections to 10,000 per class across the dataset achieves a good balance. This limit yields 98.5% of full AP while increasing file size and evaluation time only by a factor of $2 \times$ (compared to $15 \times$ for the previous strategy), making evaluation tractable. In principle, this limit depends on the size of the evaluation set, similar to how the standard per-image limit depends on the vocabulary size and labeling density. In practice, the LVIS validation and test sets all contain 20,000 images and thus a single limit suffices.

This evaluation may appear similar to the undesirable Ranking 2 in Figure 2. However, Ranking 2 is an undesirable strategy for resolving competition across classes, while our evaluation removes this competition altogether by providing an independent detection budget per class. This evaluation has a natural appeal when viewing detection as an information retrieval task, the field from which AP originates: the detector is allowed to ‘retrieve’ up to $k$ detections (or ‘documents’) per class from the entire evaluation set (or ‘corpus’). In practice, various strategies exist for efficiently selecting the top $k$ detections over a large set of images.

We recommend this latter strategy of limiting detections per class, with no limit per image. In the remainder of the paper, we refer to the default evaluation (with a detections-per-image limit) as ‘AP$^{\text{Old}}$’ and our new, recommended version that limits detections per class as ‘AP$^{\text{Fixed}}$’.

**5. Impact on long-tailed detector advances**

We have shown that the current AP evaluation introduces subtle, undesirable interactions with cross-category rankings due to the detections-per-image limit. However, it remains unclear to what extent this issue meaningfully affects prior conclusions drawn on LVIS. To analyze this, we evaluate the importance of different design choices in LVIS detectors with the original evaluation (‘AP$^{\text{Old}}$’), with a limit of 300 predictions per image, and our modified evaluation (‘AP$^{\text{Fixed}}$’), with a limit of 10,000 detections per class across the whole evaluation set (with no per-image limit).

**Experimental setup.** The following experiments use Mask R-CNN [7]. Unless noted differently: we use a ResNet-50 [8] backbone with FPN [17] pre-trained on Im-

![Figure 3. Score distribution induced by different loss functions for LVIS rare, common, and frequent categories. Compared to the baseline softmax CE loss, BaGS, EQL, and Federated losses tilt the distribution to be more uniform, modifying ranking of detections across categories.](image)

ageNet [26] and fine-tuned on LVIS v1 [6] for 180k iterations with repeat factor sampling, minibatch of 16 images, learning rate of 0.02 decayed by $0.1 \times$ at 120k and 160k iterations, and weight decay of $1e^{-4}$. Batch norm [11] parameters are frozen. Results are reported on LVIS v1 validation using the mean of three runs with different random seeds.

**5.1. Case studies**

**Loss functions.** As discussed in Section 2, a number of new losses have been proposed in the past year. We analyze three in particular: EQL [27], BaGS [16], and a ‘Federated’ loss [35]. Table 5a (first column) shows that, under the original evaluation, the choice of loss function can robustly improve the AP of a baseline model by up to 2.4 points, from 22.3 using softmax cross-entropy (CE) to 24.7 using the Federated loss. These gains suggest the choice of loss function is important. However, under our ‘AP$^{\text{Fixed}}$’, the losses are more similar, differing by at least 0.8 points.

To gain insight into why the losses improve AP$^{\text{Old}}$ more than AP$^{\text{Fixed}}$, we plot the score distribution for the LVIS rare, common, and frequent categories (normalized so the average score for frequent categories is 1.0). Figure 3 shows that the EQL, BaGS, and Federated losses tilt the distribution to be more uniform relative to softmax CE loss. This boosts the confidence of rare category detections, making them more likely to appear in the 300 detections-per-image limit. This suggests that these losses change cross-category rankings compared to softmax CE loss in a way that AP$^{\text{Old}}$ rewards. Because AP$^{\text{Fixed}}$ is category independent, it does not reward cross-category ranking modifications.

**Classifier heads.** Next, we evaluate two common modifications to the linear classifier in detectors in Table 5b. The first modification trains a linear objectness binary classifier in parallel to the $K$-way classifier [16, 25, 29], denoted ‘Obj’. The second L2-normalizes the input features and classifier weights during training and inference [28, 19, 29], denoted
(a) Loss functions. Choosing the right loss is more important under $\text{AP}^{\text{Old}}$, providing an improvement of up to +2.4 $\text{AP}$. Under our proposed $\text{AP}^{\text{Fixed}}$, the impact of losses is reduced, to at most +0.8 $\text{AP}$. This result indicates that these loss functions may primarily modify cross-category rankings (also see Figure 3).

(b) Classifier modifications. We evaluate two ideas commonly used for improving long-tail detection: an objectness predictor (‘Obj’) [16], and L2-normalizing both the linear classifier weights and input features (‘Norm’). Once again, we find that these components improve the baseline significantly under the $\text{AP}^{\text{Old}}$, but provide minor improvements under our $\text{AP}^{\text{Fixed}}$. Nevertheless, our results indicate these components provide a strong, simple baseline that erases the impact of the training loss choice.

| Loss          | $\text{AP}^{\text{Old}}$ | $\text{AP}^{\text{Fixed}}$ |
|---------------|--------------------------|----------------------------|
| Softmax CE    | 22.3                     | 25.5                       |
| Sigmoid BCE   | 22.5 (+0.2)              | 25.6 (+0.1)                |
| EQL [27]      | 24.0 (+1.7)              | 26.1 (+0.6)                |
| Federated [35]| 24.7 (+2.4)              | 26.3 (+0.8)                |
| BaGS [16]     | 24.5 (+2.2)              | 25.8 (+0.3)                |

(c) Samplers. Category Aware Sampling (CAS) and Repeat Factor Sampling (RFS) are common sampling strategies for addressing class imbalance. While both strategies outperform the uniform sampling baseline under $\text{AP}^{\text{Old}}$, only RFS provides significant improvements under $\text{AP}^{\text{Fixed}}$.

| Sampler      | $\text{AP}^{\text{Old}}$ | $\text{AP}^{\text{Fixed}}$ |
|--------------|--------------------------|----------------------------|
| Uniform      | 18.4                     | 22.8                       |
| CAS          | 19.2 (+0.8)              | 21.5 (+1.3)                |
| RFS          | 22.3 (+3.9)              | 25.5 (+2.7)                |

(d) Classifier retraining. We evaluate the efficacy of training detectors in two phases, a common technique [12, 29]. Phase 1: the model is trained end-to-end with one sampler. Phase 2: only the final classification layer is trained, using a different sampler. This strategy improves $\text{AP}^{\text{Old}}$, but not $\text{AP}^{\text{Fixed}}$, suggesting that classifier retraining may primarily modify cross-category rankings.

| Phase          | $\text{AP}^{\text{Old}}$ | $\text{AP}^{\text{Fixed}}$ |
|----------------|--------------------------|----------------------------|
| Phase 1        | 22.3                     | 25.5                       |
| Phase 2        | 23.6 (+1.3)              | 25.6 (+0.1)                |

(e) Stronger backbones. Using larger backbones consistently improves the detector under both $\text{AP}^{\text{Old}}$ and $\text{AP}^{\text{Fixed}}$, indicating, as one might expect, that larger backbones improve overall detection quality and not just cross-category rankings. ResNeXt-101 uses the 32x8d configuration.

Table 5. Impact of various design choices on the LVIS v1 validation dataset, comparing $\text{AP}^{\text{Old}}$ to $\text{AP}^{\text{Fixed}}$. Unless specified otherwise, each experiment uses a ResNet-50 FPN Mask R-CNN model trained with Repeat Factor Sampling (RFS) for 180k iterations with 16 images per batch. All numbers are the average of three runs with different random seeds and initializations.

Classifiers are used.

‘Norm.’ We share implementation details in supplementary.

The first block in Table 5b shows that while adding an objectness predictor modestly improves $\text{AP}^{\text{Old}}$ (+0.9), it results in a slightly lower $\text{AP}^{\text{Fixed}}$ (−0.2). This discrepancy suggests the objectness predictor optimizes the ranking of predictions across classes, but doesn’t meaningfully improve the quality of the detections. On the other hand, using a normalized classifier consistently leads to higher accuracy under both $\text{AP}^{\text{Old}}$ (+0.9) and $\text{AP}^{\text{Fixed}}$ (+0.8). Finally, we find that applying both these modifications to the classifier results in a strong baseline under both $\text{AP}^{\text{Old}}$ and $\text{AP}^{\text{Fixed}}$. The second block in Table 5b further shows that under $\text{AP}^{\text{Fixed}}$, the choice of loss function is largely irrelevant when both of these classifier modifications are used.

Sampling strategies. Modifying the image sampling strategy is a common approach for addressing class imbalance in LVIS. Table 5c analyzes three strategies: Uniform, which samples images uniformly at random; Class Aware Sampling (CAS), which first samples a category and then an image containing that category; and Repeat Factor Sampling (RFS) [6], which oversamples images containing rare classes. RFS consistently and significantly outperforms the others under both $\text{AP}^{\text{Old}}$ and $\text{AP}^{\text{Fixed}}$. Surprisingly, while CAS outperforms uniform sampling under $\text{AP}^{\text{Old}}$, it hurts accuracy under $\text{AP}^{\text{Fixed}}$, suggesting that CAS improves primarily due to how it ranks predictions across classes.

Classifier retraining. A common alternative to training with a single sampler is to train the model end-to-end using one sampler, and fine-tune the linear classifier with a different sampler [12, 33]. Under $\text{AP}^{\text{Old}}$, carefully choosing the samplers for these phases appears important, improving by +1.3 $\text{AP}$. However, under $\text{AP}^{\text{Fixed}}$, this improvement disappears, indicating that on LVIS, classifier retraining primarily improves by aligning scores across classes.

Stronger backbones. Finally, we evaluate the improvements due to stronger backbone architectures. We evaluate four progressively stronger models: ResNet-50, ResNet-101 [8], and ResNeXt-101 32x8d [30]. Unlike many other LVIS-specific design choices, we find that the choice of a larger backbone consistently improves accuracy for both $\text{AP}^{\text{Old}}$ and $\text{AP}^{\text{Fixed}}$. 

| Backbone      | $\text{AP}^{\text{Old}}$ | $\text{AP}^{\text{Fixed}}$ |
|---------------|--------------------------|----------------------------|
| ResNet-50     | 22.3                     | 25.5                       |
| ResNet-101    | 24.0 (+2.3)              | 27.7 (+2.2)                |
| ResNeXt-101   | 26.2 (+3.9)              | 28.7 (+3.2)                |
Moreover, we develop a simple detector score calibration

6.1. AP

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detections across all classes to generate a single precision-recall curve, incentivizing detectors to rank confident predictions above lower confidence ones. Second, it weights all groundtruth instances, rather than classes, equally. This removes a counterintuitive effect, illustrated in Figure 2, that can occur with class averaging. Further, it reduces the impact of the detections-per-image limit, as low-confidence predictions for some rare classes do not significantly impact the evaluation. Because of this, however, the evaluation is influenced more by frequent classes than rare ones. To analyze performance for rare classes, we further report three diagnostic evaluations which evaluate predictions only for classes within a specified frequency: AP_{r} (for rare classes), AP_{f} (for frequent), and AP_{c} (common).

6.2. Analysis

How does the dets/im limit affect AP_{pool}? Table 6 analyzes how the detections-per-image limit impacts AP_{pool}. As expected, increasing this limit does not significantly affect AP_{pool}, while AP can change drastically due to a few additional true positives for rare classes. AP_{pool} treats true positives for all classes equally. Increasing the limit beyond 300 detections improves the diagnostic AP_{pool} metric, but only mildly improves AP_{pool} by 0.8 points. Nonetheless, for consistency, we evaluate models with the same detections as AP_{fixed}: the top 10,000 per class, with no per-image limit.

Do losses impact AP_{pool}? Next, we analyze various losses under AP_{pool}, though we also analyze other detector components in supp. Table 7 compares losses under AP_{fixed} and AP_{pool}. Perhaps surprisingly, while EQL and BaGS do not meaningfully impact AP_{pool}, the Federated loss improves by 2.2 points over the baseline softmax CE loss. This provides a new perspective for the Federated loss: Although it does not explicitly calibrate models, it improves cross-category ranking of predictions compared to other losses.

| Dets/im | AP | AP_{f} | AP_{c} | AP_{i} |
|---------|----|--------|--------|--------|
| 300     | 26.2 | 8.0    | 16.7   | 27.0   |
| 1,000   | 26.8 (+0.6) | 10.6 (+2.6) | 19.8 | 27.6 |
| 2,000   | 27.0 (+0.8) | 11.0 (+3.0) | 20.5 | 27.7 |
| 5,000   | 27.0 (+0.8) | 11.3 (+3.3) | 20.8 | 27.7 |
| 10,000  | 27.0 (+0.8) | 11.3 (+3.3) | 20.8 | 27.7 |

Table 6. Impact of limiting detections-per-image on AP_{pool}. As expected, AP_{pool} is less sensitive to this limit than AP_{fixed} because each instance, rather than each class, is weighted equally.

| Loss        | AP_{fixed} | AP_{r} | AP_{f} | AP_{c} |
|-------------|------------|--------|--------|--------|
| Softmax CE  | 25.5       | 18.9   | 24.9   | 29.1   |
| Sigmoid BCE | 25.6 (+0.0) | 19.4 | 24.9 | 28.9 |
| EQL [27]    | 26.1 (+0.6) | 19.9 | 26.1 | 28.9 |
| Federated [35] | 26.3 (+0.8) | 20.7 | 24.9 | 30.2 |
| BaGS [16]   | 25.8 (+0.3) | 17.9 | 25.6 | 29.5 |

Table 7. AP_{fixed} and AP_{pool} for models trained with varying losses. Federated significantly outperforms others under AP_{pool}.

5.2. Discussion: something gained, something lost

AP_{fixed} makes AP evaluation category independent by design. As a result, it is no longer vulnerable to gaming-by-re-ranking, as we demonstrate is possible with AP_{fixed} in Section 3. However, by benchmarking several recent advances in long-tailed object detection we observe evidence that several of the improvements may be due to better cross-category rankings, because the improvements that were observed with AP_{fixed} largely disappear when evaluated with AP_{fixed}. While AP_{fixed} improperly evaluated calibration, AP_{fixed} is invariant to calibration: i.e., per-category, monotonic score transformations do not change AP_{fixed}.

Neither AP_{fixed} nor AP_{fixed} appropriately specifies how detectors should be deployed in the real world, a task which requires score calibration. In the simplest example, one may want to produce a demo that visualizes all detections above a global score threshold (e.g. 0.5) and expect to see consistent results across all categories. Given this practical demand, we consider in the next section a variant of AP, called AP_{pool}, that directly rewards cross-category rankings, without the vulnerability to gaming displayed by AP_{fixed}. Furthermore, we develop a simple detector score calibration method and show that it improves AP_{pool}.

6. Evaluating cross-category rankings

An independent, per-class evaluation is appealing in its simplicity. Most practical applications, however, require comparing the confidence of predictions across classes to form a unified understanding of the objects in an image. As an extreme example, note that a detector can output arbitrary range of scores for each class for a truly independent evaluation: that is, all detections for one class (say, ‘banana’) may have confidences above 0.5, while all detections for another class (say, ‘person’) have confidences below 0.5. Using such a detector in practice requires carefully calibrating scores across classes—an open challenge that is not evaluated by current detection evaluations.

6.1. AP_{pool}: A cross-category rank sensitive AP

To address this, we consider a complementary metric, AP_{pool}, which explicitly evaluates detections across all classes together [3]. To do this, we first match predictions to groundtruth per-class, following the standard evaluation. Next, instead of computing a precision-recall (PR) curve for each class, we pool detections across all classes to generate a single PR curve across all classes, and compute the Average Precision on this curve to get AP_{pool}.

This evaluation has two key properties. First, it ranks detections across all classes to generate a single precision-recall curve, incentivizing detectors to rank confident predictions above lower confidence ones. Second, it weights all groundtruth instances, rather than classes, equally. This removes a counterintuitive effect, illustrated in Figure 2, that can occur with class averaging. Further, it reduces the impact of the detections-per-image limit, as low-confidence predictions for some rare classes do not significantly impact the evaluation. Because of this, however, the evaluation is influenced more by frequent classes than rare ones. To analyze performance for rare classes, we further report three diagnostic evaluations which evaluate predictions only for classes within a specified frequency: AP_{r} (for rare classes), AP_{f} (for frequent), and AP_{c} (common).
6.3. Calibration

We now propose a simple and effective strategy for improving $AP_{\text{Pool}}$. We re-purpose classic techniques for calibrating model uncertainty for the task of large-vocabulary object detection. Calibration aims to ensure that the model’s confidence for a prediction corresponds to the probability that the prediction is correct. In the detection setting, if a model detects a box with confidence $s$, it should correctly localize a groundtruth box of the same category $s\%$ of the time [14]. While this property is not necessary for $AP_{\text{Pool}}$, it provides a sufficient condition for improving cross-category rankings ($AP_{\text{Pool}}$ only requires that true positives are ranked higher than false positives across all classes, without requiring the scores to be probabilistically calibrated).

Following [14], we analyze various calibration strategies: histogram binning [31], Bayesian Binning into Quantiles (BBQ) [21], beta calibration [13], isotonic regression [32], and Platt scaling [23]. Prior work on calibrating detectors applies calibration strategies to predictions across all classes [14]. However, this approach does not account for class frequency: rare classes may, for example, have lower-scoring predictions than frequent classes. Instead, we propose to calibrate each class individually, allowing the method to boost scores of under-confident classes and diminish scores of over-confident classes.

Standard calibration strategies require a held-out dataset for calibration. However, in the large-vocabulary setting, many classes have only a handful of examples in the entire dataset. We instead calibrate directly on the training set. To understand the impact of this choice, we also report an upper-bound by calibrating on the validation set.

Table 8 reports $AP_{\text{Pool}}$ using various calibration approaches applied to a model trained with the Federated loss. The results show that calibrating per class improves $AP_{\text{Pool}}$ by 1.7 points, from 27.8 to 29.5, and the choice of calibration strategy is not critical. Surprisingly, calibrating on the validation set, as in the second block, outperforms training set calibration by only 0.8 points, suggesting that calibrating on the training set is a viable strategy. However, calibrating on the validation set significantly improves $AP_{\text{Pool}}$ while calibrating on the training set harms $AP_{\text{Pool}}$, indicating that calibrating rare classes remains an open challenge. Figure 4 presents qualitative examples of this improvement: calibration increases the scores of underconfident, accurate predictions from some classes (e.g. ’cowboy hat’) and suppresses overconfident predictions from others (e.g. ’horse’).

| Calibration          | $AP_{\text{Pool}}$ | $AP_{\text{Pool}}^i$ | $AP_{\text{Pool}}^c$ | $AP_{\text{Pool}}^r$ |
|----------------------|--------------------|-----------------------|-----------------------|-----------------------|
| Uncalibrated         | 27.8               | 16.1                  | 22.0                  | 28.2                  |
| Histogram Bin        | 28.6 (±0.8)        | 12.4                  | 20.6                  | 29.2                  |
| BBQ (AIC)            | 28.8 (±1.0)        | 13.6                  | 21.6                  | 29.3                  |
| Beta calibration     | 29.5 (±1.7)        | 12.8                  | 22.7                  | 30.0                  |
| Isotonic reg.        | 28.3 (±0.5)        | 14.4                  | 22.2                  | 28.7                  |
| Platt scaling        | 29.5 (±1.7)        | 13.1                  | 22.8                  | 30.0                  |
| Calibrate on validation (upper-bound oracle) |
| HistBin              | 30.1 (±2.3)        | 24.4                  | 27.8                  | 30.2                  |
| BBQ (AIC)            | 30.0 (±2.2)        | 22.9                  | 26.9                  | 30.2                  |
| Beta calibration     | 29.8 (±2.0)        | 22.4                  | 25.2                  | 30.1                  |
| Isotonic reg.        | 30.3 (±2.5)        | 24.6                  | 27.2                  | 30.4                  |
| Platt scaling        | 29.8 (±2.0)        | 22.2                  | 24.9                  | 30.1                  |

Table 8. Calibrating detection outputs on the train set significantly improves $AP_{\text{Pool}}$. The gains are due to improved rankings across categories. Calibrating on validation significantly improves $AP_{\text{Pool}}$, indicating calibration remains challenging in the tail. All models trained with the Federated loss.

7. Discussion

Robust, reliable evaluations are critical for advances in large-vocabulary detection. Our analysis reveals that current evaluations fail to properly handle cross-category interactions by neither eliminating them (as intended) nor evaluating them in a principled fashion (as potentially desired). We show that, as a result, the current AP implementation ($AP_{\text{Old}}$) is vulnerable to gaming. We propose $AP_{\text{Fixed}}$, which addresses this gameability by removing the effect of cross-category score calibration, and recommend it as a replacement for $AP_{\text{Old}}$ moving forward. $AP_{\text{Fixed}}$ provides new conclusions about the importance of different LVIS advances. Finally, we recommend a complementary diagnostic metric, $AP_{\text{Pool}}$, for applications requiring cross-category score calibration, and show that a simple calibration strategy offers off-the-self detectors solid improvements to $AP_{\text{Pool}}$. 

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Figure 4. Examples illustrating the effect of calibration. Each row shows the 20 highest-scoring predictions from the baseline, uncalibrated model (left) and its calibrated version (right). True-positives and false-positives (at IoU 0.5) are indicated with a green and red label, respectively. The calibrated model increases the rank of low-confidence but accurate predictions, such as the ‘bird’s (top row) and ‘cowboy hat’s (bottom), over incorrect predictions with artificially high scores, such as some ‘boat’s (top), and ‘horse’s (bottom).
Appendix

We first present additional analysis of our experiments in Section A. Section A.1 reports AP\textsuperscript{Pool} for all experiments in Table 5, and discusses key results. Section A.2 analyzes all variants of classifier retraining. Section A.3 analyzes losses and classifier modifications using a stronger baseline detector. Section A.4 presents the impact of non-max suppression on AP\textsuperscript{Old} compared to AP\textsuperscript{Fixed}. Finally, Section B presents implementation details, including the experimental setup for tables in the main paper, classifier modifications, and the RegNetY-4GF model used in this appendix.

A. Additional analyses

A.1. AP\textsuperscript{Pool}: Long-tail detector advances

Table 9 reports AP\textsuperscript{Pool} for all experiments in Table 5. We highlight a few results here. Table 9a reports the same results as Table 7: while most losses do not improve AP\textsuperscript{Pool}, the Federated loss provides significant improvements of +2.2 points under AP\textsuperscript{Pool}, indicating it helps to calibrate cross-category scores. Table 9b shows that the classifier modifications described in Section 5.1 provide small improvements in AP\textsuperscript{Pool} (+0.7 using both modifications). Surprisingly, the Federated loss performs slightly better without these modifications under AP\textsuperscript{Pool} (27.8 without modifications in Table 9a vs. 27.6 with modifications). Even with both classifier modifications, the choice of loss is important under AP\textsuperscript{Pool}, as the softmax CE loss significantly underperforms the Federated loss. Table 9c shows the uniform sampler performs on par with Repeat Factor Sampling, likely because AP\textsuperscript{Pool} weights all instances equally, while AP\textsuperscript{Fixed} and AP\textsuperscript{Old} weight each class equally. Table 9d shows that two-stage training does not improve AP\textsuperscript{Fixed} or AP\textsuperscript{Pool} significantly. Finally, larger backbones consistently improve AP\textsuperscript{Old}, AP\textsuperscript{Fixed}, and AP\textsuperscript{Pool} (Table 9e).

A.2. Classifier retraining

Table 5d evaluates the efficacy of training detectors in two phases, using a few different data sampling configurations. For completeness, we report results using all sampler configurations in Table 10. These results further support the conclusions from the main paper: while certain configurations improves over the baseline under AP\textsuperscript{Old} (up to +1.3AP), they provide little to no improvements under AP\textsuperscript{Fixed}, suggesting they modify cross-category rankings. However, they also do not impact AP\textsuperscript{Pool}, indicating they do not meaningfully improve the calibration of scores across categories — i.e., they may take advantage of the vulnerability in AP\textsuperscript{Old} discussed in Section 3.

A.3. Evaluating losses for stronger models

Since certain detector modifications may behave differently as model capacity varies, we re-evaluate the importance of losses and classifier modifications using a stronger model in Table 11. We use a Cascade R-CNN model with a RegNetY-4GF backbone (see Section B for details), which we refer to as the strong model. We report results using this model in Table 11, and compare with the results using a ResNet-50 model (the weak model) reported in Table 9. We highlight two key differences. First, the Federated loss provides clear, significant improvements under both AP\textsuperscript{Fixed} and AP\textsuperscript{Pool} using the strong model (Table 11a), while it provides only a minor improvement over the weak model under AP\textsuperscript{Fixed} (Table 9a). This suggests the Federated loss may be more helpful for models with higher capacity. Second, while the normalized linear layer improves both the weak model and the strong model under all metrics, adding an objectness predictor in addition to the normalized classifier hurts the strong model considerably (dropping from 33.3 to 32.2 under AP\textsuperscript{Fixed}). These results suggest that the normalized classifier is helpful even for high-capacity models, but the objectness predictor is not.

A.4. Impact of NMS on AP\textsuperscript{Fixed} vs. AP\textsuperscript{Old}

Non-max suppression (NMS) is an important, tunable component of modern detection pipelines. A key parameter in NMS is the intersection-over-union (IoU) threshold: a low-confidence prediction which has IoU greater than this threshold with a high-confidence prediction of the same class is suppressed. At low thresholds, NMS will ensure predictions are almost entirely non-overlapping by suppressing many predictions. At high thresholds, by contrast, few predictions are suppressed. We evaluate the impact of this threshold for NMS for AP\textsuperscript{Old} and AP\textsuperscript{Fixed} in Table 12. Overall, NMS strongly impacts both AP\textsuperscript{Old} and AP\textsuperscript{Fixed} significantly. We highlight one behavior that illustrates a key difference between AP\textsuperscript{Old} and AP\textsuperscript{Fixed}. At high thresholds, where NMS suppresses only a few predictions, AP\textsuperscript{Old} sees a drastic drop in rare class accuracy (−10.1), because predictions for frequent classes crowd out rare class predictions from the common detections per image limit. However, AP\textsuperscript{Fixed} sees a more modest drop in rare class accuracy (−3.3), as AP\textsuperscript{Fixed} provides a separate, per-class budget that prevents crowding out of rare class predictions.

B. Implementation details

Experimental Setup for Tables 1, 2, 4. For experiments in Tables 1 and 4, and the LVIS results in Table 2, we closely follow the setup in Section 5, but all results are from a single random seed and init. We follow the same setup for COCO results in Table 2 with two modifications: (1) We train for 270k iterations with a 0.1 × learning rate decay at 210k and 250k iterations, and (2) we use a uniform sampler. Classifier heads: Objectness. The objectness predictor in Table 5b is implemented as an additional linear layer paral-
Each experiment uses a ResNet-50 FPN Mask R-CNN model trained with Repeat Factor Sampling (RFS) for 180k iterations with 16 images.

Impact of various design choices on the LVIS v1 validation dataset, comparing AP

Table 9.

**Table 10.**

(a) **Loss functions.** Most losses perform equally under AP^Pool, with the exception of Federated loss, which significantly improves AP^Pool by 2.2 points.

(b) **Classifier modifications.** We evaluate two ideas commonly used for improving long-tail detection: an objectness predictor ('Obj') [16], and L2-normalizing both the linear classifier weights and input features ('Norm'). These components mildly improve AP^Pool, and the Federated loss still outperforms all other losses. Perhaps surprisingly, the modifications hurt the Federated loss (27.6 with vs. 27.8 without in Table 9a).

(c) **Samplers.** Category Aware Sampling (CAS) and Repeat Factor Sampling (RFS) are common sampling strategies for addressing class imbalance. As AP^Pool weights all instances equally, unlike AP^Fixed and AP^Old (which weight all classes equally), uniform sampling performs on par with RFS.

(d) **Classifier retraining.** We evaluate the efficacy of training detectors in two phases, a commonly used technique [12, 29]. In Phase 1, the model is trained end-to-end with one sampler. In Phase 2, only the final classification layer is trained, using a different sampler. This strategy provides only minor improvements on AP^Pool.

(e) **Stronger backbones.** Using larger backbones consistently improves the detector under AP^Old, AP^Fixed, and AP^Pool, indicating, as one might expect, that larger backbones improve overall detection quality and not just cross-category rankings. ResNeXt-101 uses the 32×8d configuration.

| Sampler | AP^Old | AP^Fixed | AP^Pool |
|---------|--------|----------|---------|
| Uniform | 18.4   | 22.8     | 25.7    |
| CAS     | 19.2   | (0.8) 21.5 (1.3) 21.7 (4.0) |
| RFS     | 22.3   | (3.8) 25.5 (2.7) 25.6 (0.1) |

Table 9. **Impact of various design choices on the LVIS v1 validation dataset, comparing AP^Old, AP^Fixed and AP^Pool.** We report all experiments from Table 4 of our main paper using AP^Pool in addition to AP^Fixed and AP^Old. As in the main paper, unless specified otherwise, each experiment uses a ResNet-50 FPN Mask R-CNN model trained with Repeat Factor Sampling (RFS) for 180k iterations with 16 images per batch. All numbers are the average of three runs with different random seeds and initializations.

| Phase 1 | Phase 2 | AP^Old | AP^Fixed | AP^Pool |
|---------|---------|--------|----------|---------|
| RFS     | -       | 22.3   | 25.5     | 25.6    |
| Uniform | Uniform | 19.3   | (3.0) 24.0 (1.5) 25.8 (0.2) |
| Uniform | RFS     | 21.6   | (0.7) 24.9 (0.6) 25.8 (0.0) |
| Uniform | CAS     | 23.1   | (0.8) 24.9 (0.6) 25.6 (0.0) |
| RFS     | Uniform | 20.8   | (1.5) 25.4 (0.1) 25.8 (0.2) |
| RFS     | CAS     | 23.6   | (1.3) 25.6 (0.1) 25.5 (0.1) |
| CAS     | Uniform | 17.4   | (3.9) 21.1 (4.4) 22.1 (3.5) |
| CAS     | RFS     | 18.2   | (1.3) 21.3 (4.2) 22.1 (3.5) |
| CAS     | CAS     | 19.1   | (3.2) 21.2 (4.3) 21.7 (3.9) |

Table 10. **Classifier retraining.** We report all variants of classifier retraining, a subset of which are reported in Table 5d. This strategy improves AP^Old, but only mildly affects AP^Fixed, suggesting that classifier retraining may primarily improve cross-category rankings. All results are the average of 3 runs with different random seeds and initializations.

**Table 10.**

Classifier retraining. We evaluate the efficacy of training detectors in two phases, a commonly used technique [12, 29]. In Phase 1, the model is trained end-to-end with one sampler. In Phase 2, only the final classification layer is trained, using a different sampler. This strategy provides only minor improvements on AP^Pool.

Classifier heads: *Normalized layer.* We replace the standard classifier with the following, as in [29]:

\[ f(x; w, b, \tau) = \frac{\tau}{\|w\|_2^2 \|x\|_2} w^T x + b, \]

where \( \tau \) is a temperature parameter tuned separately for each loss. For softmax CE and BaGS, \( \tau = 20.0 \); for sigmoid BCE, Federated, and EQL losses, \( \tau = 50.0 \). When using an objectness predictor with the normalized classifier, we replace the objectness predictor with a normalized layer as well. Figure 5 shows a concise PyTorch implementation.

Classifier retraining. For classifier retraining experiments in Table 5d, we train models in two phases. In Phase 1, we train the baseline model end-to-end following the setup in Section 5, using the Phase 1 sampler. In Phase 2, we randomly re-initialize the classifier weights and biases of...
Figure 5. Pytorch code for implementing the forward pass of a normalized linear layer using a standard linear layer.

```python
def forward_normalized(linear, x, temperature):
    ###
    x (torch.Tensor): Input features, shape (n, d).
    temperature (float): Temperature hyperparam.
    linear (nn.Linear): Standard linear layer.
    ###
    x_normed = F.normalize(x, p=2, dim=1)
    w_normed = F.normalize(linear.weight, p=2, dim=1)
    return F.linear(
        temperature * x_normed, w_normed, linear.bias
    )
```

1. from torch.nn import functional as F
2. def forward_normalized(linear, x, temperature):
   
   Args:
   x (torch.Tensor): Input features, shape (n, d).
   temperature (float): Temperature hyperparam.
   linear (nn.Linear): Standard linear layer.

   
   x_normed = F.normalize(x, p=2, dim=1)
   w_normed = F.normalize(linear.weight, p=2, dim=1)
   return F.linear(
       temperature * x_normed, w_normed, linear.bias
   )

Table 11. Analyzing losses and components with a higher-capacity detector using a RegNetY-4GF backbone. See Section A.3 for details.

| Loss   | AP\text{Old} | AP\text{Fixed} | AP\text{Pool} |
|--------|--------------|----------------|--------------|
| Softmax CE | 28.6        | 31.9          | 32.2        |
| Sigmoid BCE | 28.5 (−0.1) | 31.8 (−0.1)  | 32.0 (−0.2) |
| EQL [27]   | 29.4 (−0.3) | 31.8 (−0.1)  | 32.2 (0.0)  |
| Federated [35] | 31.8 (+3.2) | 33.6 (+1.7)  | 34.7 (+2.9) |
| BaGS [16]  | 30.2 (±1.6) | 31.9 (±0.0)  | 32.5 (±0.3) |

(a) Loss functions. With the stronger model, most losses perform roughly equally well under AP\text{Old} and AP\text{Fixed}, but the Federated loss shows significant improvements for all metrics.

| Loss   | Obj | Norm | AP\text{Old} | AP\text{Fixed} | AP\text{Pool} |
|--------|-----|------|--------------|----------------|--------------|
| Softmax CE | ×   | ×    | 28.6         | 31.9          | 32.2        |
| EQL [27]   | ×   | ✓    | 29.3 (+0.7)  | 31.5 (+0.4)   | 32.4 (+0.2) |
| Federated [35] | ✓     | ✓    | 29.7 (+1.1)  | 33.3 (+1.4)   | 32.4 (+0.2) |
| ✓        | ✓    | ✓    | 30.2 (+1.6)  | 32.2 (+0.3)   | 32.5 (+0.3) |

(b) Classifier modifications. While the normalized linear layer (‘norm’) improves both AP\text{Fixed} and AP\text{Pool}, the objectness predictor (‘obj’) significantly hurts AP\text{Fixed}.

Table 12. Varying NMS thresholds, comparing AP\text{Old} and AP\text{Fixed}. Varying the intersection-over-union threshold for NMS impacts both AP\text{Old} and AP\text{Fixed}, compared to the default threshold of 0.5. At the high threshold of 0.9, NMS suppresses very few boxes. For AP\text{Old}, this results in a significant drop in accuracy for rare classes (−8.1), as overlapping predictions for frequent classes fill up the detections-per-image budget. For AP\text{Fixed}, however, the effect is much smaller (−3.3), due to the independent budget per classes.

| NMS  | AP\text{Old} | AP\text{Fixed} |
|------|--------------|----------------|
| Thresh | AP | AP\text{c} | AP\text{i} | AP | AP\text{c} | AP\text{i} |
| 0.5  | 22.7 | 13.3 | 21.2 | 28.6 | 25.5 | 18.8 | 24.8 | 29.1 |
| 0.1  | 23.2 (+0.5) | 14.0 (+0.7) | 22.1 | 28.6 | 25.1 (+0.4) | 18.1 (+0.7) | 27.4 (+2.4) | 28.9 |
| 0.3  | 23.5 (+0.8) | 13.9 (+0.6) | 22.2 | 29.1 | 25.8 (+0.3) | 18.7 (+0.1) | 25.2 (+2.9) |
| 0.7  | 20.0 (+2.7) | 8.8 (+4.5) | 18.8 | 26.2 | 23.7 (+1.8) | 18.2 (+0.6) | 23.1 (+2.8) |
| 0.9  | 12.6 (+10.1) | 5.2 (+8.1) | 12.2 | 16.3 | 16.5 (+6.0) | 15.5 (+3.3) | 16.6 | 16.9 |

Table 13. Parameters tuned for each loss. See Section B for details.

| Loss | Param | Description | Default | Search | Final |
|------|-------|-------------|---------|--------|-------|
| EQL  | λ     | Frequency threshold | 1.76e−3 | [1e-4, 5e-4, 1e-3, 1.76e-3, 5e-3] | 1e-3 |
| BaGS | β     | BG sample ratio | 8.0 | [4.0, 8.0, 16.0, 32.0, 64.0] | 16.0 |
| Federated | | Neg. classes sampled | 50 | [10, 50, 100] | 50 |

RegNetY-4GF model. For the ‘RegNetY-4GF’ model in Table 9e and Table 11, we use a Cascade R-CNN model [1] with a RegNetY-4GF [24] backbone using FPN [17]. This model is trained following the Section 5 setup, with important modifications to achieve high accuracy using the stronger capacity backbone. The model is trained for 270k iterations, with a 0.1× learning rate decay applied after 210k and 250k iterations. The weight decay is set to 5e−5 to match the weight decay used for ImageNet pre-training (a standard weight decay of 1e−4 decreases AP\text{Old} by more than 3 points). Stronger data augmentation was needed to prevent overfitting for rare categories; we resize the larger size of training images randomly between 400px and 1000px, instead of the default strategy (used for all other experiments) of picking a random scale from 640px to 800px with a step of 32.

Losses. As EQL [27] and BaGS [16] were developed for LVIS v0.5, and the Federated loss [35] was tuned for a CenterNet-based detector [36], we tune key parameters for each loss in our setting. We tune parameters using a Mask R-CNN model with ResNet-50, trained on LVIS v1 closely following the setup in Section 5, except with a Uniform sampler instead of RFS, and using a single random initialization and seed instead of three. We choose hyperparameters which optimize AP\text{Old} on the LVIS v1 validation set. We detail these hyperparameters and their optimal choices in Section B. In addition to these recently proposed losses, we found it necessary to lengthen the warmup schedule for the ‘Sigmoid BCE’ loss for training stability. The default for most models is a linear warmup schedule starting with a learning rate of 1e−3, ramping up for the first 1,000 iterations. For Sigmoid BCE, we found it necessary to start with a lower learning rate of 1e−4 and ramp up for 10,000

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iterations. Our Sigmoid BCE implementation first sums the BCE loss over all $K \cdot N$ loss evaluations for the $K$ classes (e.g., 1203 in LVIS v1) and $N$ object proposals in a mini-batch and then divides this sum by $N$.

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