An analysis of best-practice strategies for replay and rehearsal in continual learning

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Abstract

This study is in the context of class-incremental continual learning using replay, which has seen notable progress in recent years, fueled by concepts like conditional, latent or maximally interfered replay. However, there are many design choices to take when it comes to implementing replay strategies, with potentially very different outcomes in the various class-incremental scenarios. Some of the obvious design choices in replay are the use of experience replay (ER), the use of different generators like GANs –vs– VAEs for generative replay, or whether to re-initialize generators after each task. For replay strategies in general, it is an open question how many samples to generate for each new task, and what weights to give generated and new samples in the loss. On top of this, there are many possible CL evaluation protocols differing in the amount of tasks, the balancing of tasks or fundamental complexity (e.g., MNIST -vs- latent CIFAR/SVHN), and thus few generic conclusions about best practices for replay/rehearsal have found consensus in the literature. This study aims at establishing such best-practices by conducting an extensive set of representative replay experiments.

1. Introduction

This article is in the context of class-incremental continual learning (CL), which is considered the most challenging CL scenario among several others, see [52]. Class-incremental CL assumes that data non-stationarities take the form of abrupt switches between mutually exclusive *tasks*, see Fig. 1. One fundamental approach to tackle class-incremental CL is *replay*, which we take to mean the re-use of samples from previous tasks when tackling the current one. In *experience replay*, these samples are taken from a buffer that was populated during previous tasks, whereas in *generative replay*, they are produced by a *generator* trained during previous tasks. Replay approaches have known considerable success [54] and are actively evolving. Some of the recent additions include latent replay [41], brain-inspired replay [53], maximally interfered replay [1] and adiabatic replay [27]. However, the design space of replay methods is large, which is illustrated in Fig. 2, and it is not clear whether there is a single best-practice strategy that can guide researchers in all possible evaluation scenarios. We can identify several fundamental axes for replay strategies in general, where we omit the issue of using latent replay or not. Rather, we assume that latent replay is used only for problems where it is required.

- Number of samples to replay for each task
- Relative weighting new and generated samples

For generative replay, there are additional choices to make. We believe that the consensus of the community is to use class-conditional generators (see, e.g., [32, 33, 53]) so this is not included here. Similarly, we do not include the choice of a particular form for the involved DNNs, and rather assume that they are chosen according to the characteristics of the data they are applied to.

- Should generators be re-initialized after each task?
- What type of generator should be used, i.e., cVAE or cGAN?

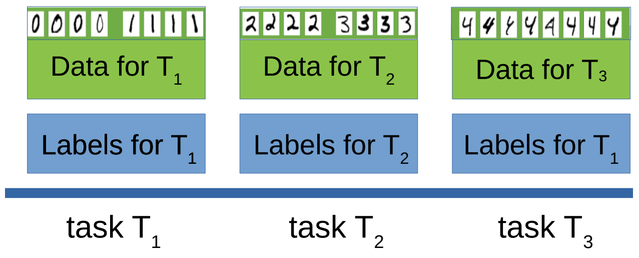


Figure 1. Class-incremental learning, consisting of distinct *tasks* that contain data from pairwise disjoint classes. Please note that not all tasks need contain the same number of classes.

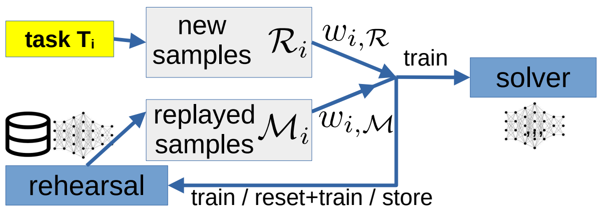

task T_i
new samples $\mathcal{R}_i$
$w_{i,\mathcal{R}}$
train
solver
replayed samples $\mathcal{M}_i$
$w_{i,\mathcal{M}}$
rehearsal
train / reset+train / store

Figure 2. A general depiction of replay approaches, regardless of whether a generator or a buffer is used. For the purposes of this study, we have also shown the different weights $w_{i,\mathcal{R}}$ and $w_{i,\mathcal{M}}$ that real and replayed samples can be assigned in the loss at task i.

And finally, the chosen evaluation scenario is relevant:

- Number of tasks (small/large)
- Task balancing, i.e., do all tasks contain the same number of classes?
- Fundamental difficulty of the CL problem (e.g., permuted MNIST –vs– CIFAR)

The common evaluation scenario seems to be what is usually termed *split-MNIST* and which we denote as D2^5, generalized to other 10-class datasets. In D2^5 CL problems, the 10 classes are grouped into five tasks of two unique classes each. Obviously, other tasks can be constructed from 10-class datasets, such as, e.g., D6-1^4 or D1^{10} which is another common (but less often used) CL benchmark. In any case, most works assume that the number of classes per task is constant and known, which is an assumption that we relax in some of the evaluation of this article.

1.1. Related Work

Many recent works perform comparison studies [12, 35, 53] between different approaches to CL. However, when it comes to rehearsal, no unified view exists w.r.t. various design choices to make. Constant-time rehearsal is used in several studies, combined with weights for replayed and new samples. In some studies [1], weights for replayed samples are chosen by cross-validation, whereas heuristics based on the number of previously seen tasks are used in others [53]. An extensive experimental evaluation of different generator types was performed in [33], with the result that conditional generators are advantageous and that GANs are more suitable than VAEs, although it is not clear how the various parameters were tuned in this study. Although it is rarely indicated in the articles, generators are usually re-initialized after each task, whereas [53] argues for keeping generators since ”preventing forgetting is easier than learning”. To the best of our knowledge, no recent study exists which systematically assesses the performance of rehearsal methods for all of the more common design choices. Concerning DGR using Denoising Diffusion Probabilistic models (DDPMS, [22]) which has been investigated in very recent studies [11, 17, 36, 57]: we did not specifically address DDPMs here but will do so in future work, once good practices for replay have been agreed upon in the community.

1.2. Contributions

This article is the first study to systematically compare different commonly used replay approaches on a wide variety of datasets and dataset splits for continual learning, including the important aspect of latent replay. Based on these investigations, we provide guidelines for using replay-based approaches to continual learning.

2. Methods

2.1. Feature encoding

The training of generative models on complex datasets like SVHN and CIFAR-10 is still challenging [1, 32]. Hence, the use of feature extractors has become a principled approach to deal with this limitation [21, 34, 40, 41, 53]. This study relies upon *supervised contrastive learning (SCL)* [24] based on SimCLR [8] to build a fixed feature extractor to tackle more complex data distributions. Usually, in contrastive learning, the encoding network is trained on large datasets such as ImageNet [47], but our empirical studies have shown that the extracted features might not be beneficial for every scenario, but ultimately depends on the compatibility between the source and target domain. While it might work for e.g., generalizing features from CIFAR-10 and use them for CL training on CIFAR-100 as shown in [53], at the same time they might be insufficient for SVHN and vice versa. We reserve a fixed portion of the original dataset for SCL and exclude these instances from being used for downstream CL, thus the data used for pretraining is identical but not the same. A ResNet-50 with randomly initialized weights is used as the encoding backbone and trained for 256 epochs with a mini-batch size of 256. Each incoming data instance is normalized and augmented by performing a random horizontal flipping and rotation in a range from $-0.02 * 2\pi$ to $0.02 * 2\pi$. The final pooling layer outputs a representation vector with the dimensionality $D = (1, 1, 2048)$. The attached projection head consists of two fully-connected layers with 2048 and 128 units respectively using ReLU activation. The n-pairs multi-class loss [49] is used with a temperature of 0.05 and optimized with Adam using $\epsilon = 0.001$, $\beta_1 = 0.9$ and $\beta_2 = 0.999$. In our experiments, the datasets are transformed prior to CL training, however, an ”on-the-fly” encoding is also feasible at the mini-batch or sub-task level, albeit with poorer runtime efficiency.

2.2. CL strategies

Experience replay (ER) uses reservoir sampling as described in [46], storing 50 samples per encountered class. The time and space complexity is thus constant w.r.t. the number of classes, even if CL breaks down at some point when the number of tasks becomes large.

The ER solver consists of 3 fully-connected layers with 512 units and ReLU activation followed by a softmax output layer.

Deep generative replay (DGR) utilizes cVAEs [25, 50] and GANs [18], whereas the latter is either implemented as a vanilla GAN (cGAN) [38] or by using the Wasserstein distance [3] combined with gradient penalty [19] (WGAN-GP). VAEs use a latent dimension z of 100 and a disentangling factor of $\beta = 1.0$, while GANs use a noise dimension z of 100. WGAN-GP uses a gradient penalty weight of 10 and performs three discriminator iterations per generator iteration. Both, VAEs and GANs are conditioned on the label space by concatenating the output of the label input mapped to a fully connected layer with units either matching the data dimension (cVAE encoder/decoder and cGAN discriminator) or noise dimension (cGAN generator). The VAE encoder consists of two fully-connected layers with 2048 units and ReLU activation, followed by a split output head for the mean vector and logarithmic variance. The decoder is composed of a dense layer chain using ReLU with 128-512-1024 units and a 2048-dimensional output layer with sigmoid activation. The GAN generator is composed of two fully connected layers with 2048 units each and an output layer with sigmoid activation. For cGAN and WGAN-GP each dense layer is followed by a batch normalization layer and LeakyReLU with $\alpha = 0.2$. The Discriminator uses two dense layers with 512 and 256 units followed by LeakyReLU with $\alpha = 0.2$ and Dropout with a rate of 0.3. DGR solvers share the same architecture as the solver used for ER while using the ADAM optimizer with a learning rate of $1\text{e}{-3}$ and $\beta_1 = 0.9, \beta_2 = 0.999$. The learning rate for cVAE encoders and decoders is set to $1\text{e}{-4}$ with $\beta_1 = 0.9, \beta_2 = 0.999$. While cGAN and WGAN-GP use a learning rate of $5\text{e}{-4}$ with $\beta_1 = 0.5, \beta_2 = 0.999$ using the ADAM optimizer. Generators and the ER solver are trained for 100 epochs per task and a mini-batch size of 128. DGR solvers are trained for an additional 50 epochs after generator training. We additionally investigate the effect of re-initializing generators before each new task, as opposed to retaining the same structure (warm-start) for training.

2.3. Replay strategies

This study focuses on three distinct approaches to replay: balanced, constant and weighted. For the current training task $i > 1$, let $\mathcal{M}_i$ denote the currently replayed samples (either from a buffer or using a generator), $\mathcal{R}_i$ the current (real) task data, and β_{ij} a training mini-batch which is uniformly sampled from $\mathcal{R}_i \cup \mathcal{M}_i$ (see also Fig. 2). Further, we define a parameter $\chi_\mathcal{M}$ that defines the proportion of replayed samples at each task, therefore also defining the proportion of replayed samples in each training mini-batch:

$$\chi_\mathcal{M} = \frac{|\mathcal{M}_i|}{|\mathcal{R}_i| + |\mathcal{M}_i|}, \qquad \chi_\mathcal{R} = 1 - \chi_\mathcal{M}. \tag{1}$$

The **balanced** strategy ensures a linear scaling of $\mathcal{R}_i$ w.r.t. previously encountered classes. Denoting the number of classes for each task j as N_j, we can ensure that amount of samples from each class in all mini-batches β_{ij} is identical by choosing:

$$\chi_\mathcal{M} = \frac{\sum_{j=1}^{i-1} N_j}{\sum_{j=1}^{i} N_j} \tag{2}$$

The **constant** strategy generates an amount of samples identical to the amount of samples in $\mathcal{R}_i$. Here, storage consumption and re-training time is bounded, and $\chi_\mathcal{M}$ is set to 0.5. There are works which replay a constant number of samples regardless of the size of $\mathcal{R}_i$, which however implicitly assumes that all tasks contain the same amount of samples. Please note that classes will generally be unbalanced in each mini-batch for this strategy.

The **weighted** strategy is a direct extension to the constant strategy, which implements an additional mechanism to ensure balancing. This approach is inspired by [53] and uses distinct weights $w_{i,R}$ for real samples and $w_{i,M}$ for generated ones, which are applied to the loss function $\mathcal{L}$ to offset imbalances. Here, the loss is split into two parts: $\mathcal{L}_\mathcal{R}$ and $\mathcal{L}_\mathcal{M}$ computed from real and generated samples, respectively (see also Fig. 2). In its original formulation the calculation of these balancing coefficients is based on the amount of encountered tasks, which we will refer to as *task-weighted loss weights* (see Eq. (3)). We additionally added an adaptation based on class counts, which we term *class-weighted loss weights* (see Eq. (4)). Assuming that the amount of samples per class is roughly similar, we compute these weights according to:

$$\mathcal{L} = w_{i,\mathcal{R}}\mathcal{L}_\mathcal{R} + w_{i,\mathcal{M}}\mathcal{L}_\mathcal{M} = \frac{1}{i}\mathcal{L}_\mathcal{R} + \frac{i-1}{i}\mathcal{L}_\mathcal{M} \tag{3}$$

$$\mathcal{L} = w_{i,\mathcal{R}}\mathcal{L}_\mathcal{R} + w_{i,\mathcal{M}}\mathcal{L}_\mathcal{M} = \frac{1}{\sum_{j=1}^{i} N_j}\mathcal{L}_\mathcal{R} + \frac{N_i}{\sum_{j=1}^{i} N_j}\mathcal{L}_\mathcal{M}. \tag{4}$$

For generative replay, loss weighting is applied to both the generator and solver losses.

3. Experiments

3.1. Evaluation protocol

Usually CIL is investigated in an artificially composed setting where tasks share the same amount of classes per task which results in approximately evenly balanced compositions [2, 33, 45, 48, 55]. We argue that this assumption seems highly unrealistic for real world learning scenarios, since novel additions to a persistent knowledge base should diminish and fluctuate over the course of facing a multitude of separate training sessions. We share the idea that artificial CIL scenarios, despite their usability for prototypical evaluation, should be adapted and expanded [10]. We aim to extend common CL benchmarks towards a more sophisticated CIL evaluation protocol to use for future research in this area. Still, some relaxation has to be accepted in order to enable tractable experimentation. Replay is investigated assuming known task-boundaries and disjoint classes, while it is assumed that data from all tasks occurs with equal probability. Data is normalized to a range of $[0,1]$ and randomly shuffled beforehand. We perform a two-staged training, with an initial run on T_1 and a sequence of replay runs $T_i, i > 1$. Furthermore, we do not allow any information about future tasks in advance, apart from knowing the current classes and the number of samples per incoming task.

Three directions for creating distinct task splits to model CIL-problems (CP) are investigated and showcased in Tab. 1. These are divided into: *usual* CIL-problems (U-CP), commonly found in CIL literature (e.g. D5^2, D2^5, D1^{10}), and the more imbalanced, *diminishing* (D-CP) and *alternating* CIL-problems (A-CP). The latter two extend U-CP by incorporating some variance in terms of the total number of tasks and classes. D-CP reflects a decrease in the amount of new data over the course of training, while the task splits for A-CP fluctuate in terms of class additions. Classes per task are randomly selected once and fixated throughout all experiments.

split ↓	**dataset**↓	
	MNIST / F-MNIST SVHN / CIFAR10	E-MNIST
U-CP1	D5^2	/
U-CP2	D2^5	/
U-CP3	D1^{10}	/
D-CP1	D4-3-2-1	D20-1^{10}
D-CP2	D5-1^5	/
A-CP1	/	D2-10-3-10-5
A-CP2	/	D10-2-10-3-10

Table 1. This table shows all task splits evaluated in the empirical study. The short-hand D2^5 describes the split used for the training procedure and is to be read as 2-2-2-2-2 for a 5-fold split, where as each number from this sequence represents the amount of unique classes in the data stream for a task $T_i, i \in 1, ..., N$.

3.2. Data

MNIST [29] consists of 60.000 28×28 grayscale images of handwritten digits (0-9).
Fashion-MNIST [56] consists of 60.000 images of clothes in 10 categories and is structured like MNIST.
E-MNIST [9] is an extended version of MNIST and contains additional letters. A total of 131.000 samples are balanced across 47 classes, and thus allows to model a CL problem where the amount of already acquired knowledge can be significantly larger than the amount of new data added with each successive task.
SVHN [39] contains 60.000 RGB images of house numbers (0-9, resolution 32×32). This dataset is imbalanced, as classes 1 and 2 are overrepresented, while classes 0 and 9 are underrepresented.
CIFAR-10 [28] contains 60.000 RGB images of natural objects, resolution 32x32, in 10 balanced classes.
Feature encoding is used for SVHN and CIFAR-10 with a pre-trained feature-extractor to reduce the complexity of the data as discussed in Sec. 2.1. For SVHN, we take half of the extra split, while we divide the CIFAR10 training split in half, reserving one part for pre-training and the other for downstream CL. No encoding was performed for MNIST, FashionMNIST and E-MNIST.

3.3. Evaluation metrics

The accuracy α_{ij} of a solver S_i after each training phase $T_j, 1, \ldots, j$ is evaluated on a corresponding held-out test set. The final accuracy α_{end} is evaluated on a joint test set composed from samples of all present classes, and reported after training on the complete task sequence. For comparison, we also provide the joint-training performance α_{base}, achieved by a default solver on the union of all classes from each distinct dataset. To measure CL capacity we define forgetting F_{ij}, as an averaged value over all tasks F_T which is defined as follows:

$$F_{ij} = \max_{i \in \{1,..,T-1\}} \alpha_{ij} - \alpha_{Tj}, \qquad \forall j < T.$$

$$F_T = \frac{1}{T-1} \sum_{j=1}^{T-1} F_{Tj}, \qquad F_T \in [0,1]. \tag{5}$$

3.4. Results

The experiments are conducted on a cluster of 25 machines equipped with single RTX3070Ti GPUs. Five randomly initialized runs were performed for all configurations on the task compositions showcased in Tab. 1. We also offer a publicly available TensorFlow2 implementation[1]. The results are presented in the following order: First, a comprehensive comparison of the investigated CL methods from Sec. 2.2 in their unmodified version and a memory-constrained (constant) scenario is presented to investigate the impact of different datasets and task splits (see Sec. 3.1). Next, the effects of applying the proposed replay modifications as described in Sec. 2.3 is assessed and a final evaluation of resetting and reusing generators for DGR is performed.

[1]The code and instructions to reconstruct the experiments can be found under the following link: https://github.com/Alexk1704/AR

		method↓			
		ER	**CVAE**	**CGAN**	**WGAN-GP**
MNIST / F-MNIST	U-CP1	.85 ±.01 /.27	**.97** ±.00 /**.03**	.93 ±.01 /.11	.95 ±.01 /.08
		.71 ±.01 /.47	**.78** ±.01 /**.34**	.64 ±.01 /.61	.69 ±.01 /.52
	U-CP2	.76 ±.01 /**.05**	**.93** ±.00 /.08	.20 ±.01 /1.0	.88 ±.01 /.14
		.60 ±.04 /.49	**.63** ±.03 /**.44**	.48 ±.01 /.63	.44 ±.01 /.69
	U-CP3	.15 ±.11 /.64	**.83** ±.01 /**.19**	.10 ±.01 /1.0	.70 ±.06 /.32
		.37 ±.03 /.81	**.67** ±.04 /**.41**	.20 ±.19 /.91	.54 ±.03 /.51
	D-CP1	.86 ±.01 /.10	**.95** ±.01 /**.03**	.10 ±.00 /.49	.93 ±.00 /.05
		.64 ±.01 /.20	**.66** ±.04 /**.18**	.55 ±.03 /.25	.59 ±.00 /.23
	D-CP2	.84 ±.00 /.21	**.91** ±.00 /**.07**	.57 ±.40 /.47	.83 ±.05 /.10
		.69 ±.01 /.36	.65 ±.01 /**.27**	.51 ±.06 /.40	.50 ±.04 /.33
SVHN / CIFAR10	U-CP1	**.81** ±.03 /**.25**	.68 ±.04 /.49	.45 ±.00 /.94	.56 ±.05 /.72
		.62 ±.01 /**.40**	.54 ±.02 /.63	.40 ±.01 /.87	.44 ±.02 /.79
	U-CP2	**.78** ±.03 /**.26**	.54 ±.05 /.53	.15 ±.00 /.98	.35 ±.03 /.78
		.62 ±.02 /**.36**	.44 ±.03 /.53	.19 ±.01 /.93	.39 ±.03 /.79
	U-CP3	**.56** ±.40 /**.39**	.43 ±.05 /.60	.06 ±.00 /1.0	.30 ±.01 /.72
		.38 ±.25 /**.63**	.34 ±.03 /.71	.10 ±.01 /1.0	.28 ±.04 /.78
	D-CP1	**.82** ±.00 /**.11**	.56 ±.04 /.28	.09 ±.01 /.48	.41 ±.05 /.41
		.60 ±.02 /**.16**	.42 ±.03 /.27	.10 ±.00 /.43	.36 ±.05 /.34
	D-CP2	**.75** ±.09 /**.26**	.58 ±.01 /.43	.20 ±.00 /.99	.44 ±.01 /.61
		.65 ±.01 /**.32**	.36 ±.04 /.57	.10 ±.00 /.97	.32 ±.02 /.67
E-MNIST	D-CP1	**.64** ±.02 /.47	.44 ±.03 /**.34**	.21 ±.15 /.70	.22 ±.01 /.37
	A-CP1	.54 ±.03 /.52	**.71** ±.02 /**.36**	.17 ±.01 /.96	.59 ±.03 /.47
	A-CP2	**.64** ±.02 /.47	.63 ±.02 /**.45**	.55 ±.03 /.52	.55 ±.03 /.52

MNIST	F-MNIST	SVHN	CIFAR-10	E-MNIST1	E-MNIST2	E-MNIST3
.98	.88	.92	.75	.89	.89	.88

Table 2. Experimental results. **Upper table** Results for all investigated CL methods in their unmodified settings while following the constant replay scenario (see Sec. 3.1). We present the final test-set accuracy α_{end} followed by average forgetting F_{end} for each CIL problem presented in Tab. 1. **Lower table** Joint-training baselines for all datasets. We used the solver network as described in Sec. 2.2 trained for 100 epochs as the classification model. E-MNIST1/2/3 refer to the joint class sets as apparent in D-CP1, A-CP1 and A-CP2. Results are averaged across $N = 5$ runs.

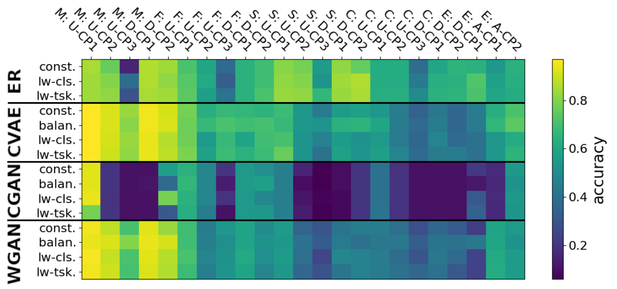
The evaluation of the memory-constrained scenario for unmodified CL methods is shown in Tab. 2. We identified cVAEs to be most effective on MNIST and Fashion-MNIST for almost every investigated task split, while ER performs better on encoded features. GAN-based DGR shows the weakest results across all datasets, especially having difficulties with longer task sequences and sequentially learning the latent feature representations. Additionally, cGANs regularly suffer from major convergence problems and mode collapse [44, 51], especially on encoded SVHN and CIFAR-10. Although, WGANs with GP show competitive results when directly compared to cVAEs, they come with the major drawback of increased training time. An epoch of generator training on U-CP3 for SVHN takes 25 seconds per epoch for cVAE, 52s/epoch for cGAN and 90s/epoch for WGAN-GP. Regarding the usage of common CIL task splits, we have identified problems like U-CP1 ($D5^2$) as vacuous to evaluate in this context due to the low number of individual replay training sessions and the inherent balance in terms of the set of classes that each task represents. We also observe this to some extent for longer and equally balanced task sequences like U-CP2 ($D2^5$) and U-CP3 ($D1^{10}$), as long as the initial capacity of the buffer or generator allows to capture the data somewhat effectively. This is reflected by the small margin in terms of accuracy and forgetting between U-CP2 and U-CP3 despite the latter objective doubling the amount of sequential learning tasks. Additionally, cVAEs for example, reach a similar performance for D-CP1 (3 tasks and 10 classes in total) as for U-CP3 (10 tasks and 10 classes in total). We also observe that all CL methods struggle to reach satisfactory results on task splits where the amount of new data to learn diminishes steadily over the course of a growing number of learning experiences, as can be seen for E-MNIST D-CP1 ($D20\text{-}1^{10}$). We also gathered more interesting results, like e.g., the poor performance of ER on MNIST/F-MNIST U-CP3. Here, the final accuracy is far off from our expectation, which again shows that minor changes in the evaluation protocol, such as randomized class orders, may show very different results than usually found in the literature.

Results for the application of proposed replay modifications are showcased in Fig. 3 to provide a comprehensive overview of their benefits for CL training. The corresponding values for forgetting can be found in Sec. 6.1. For ER, an explicit loss weighting has shown to be beneficial especially considering longer task sequences. However, we mostly couldn't distinguish a significant difference between the resulting performance of weighting on a class basis – versus – weighting on a task basis except for E-MNIST D-CP1 where balancing the loss coefficients based on the class count outperforms the weighting strategy based on the number of tasks, a training on longer task sequences like $D20\text{-}1^{20}$ could amplify this effect even more. For DGR-based rehearsal, we identified the **balanced scenario** as the most stable one, achieving the highest accuracy and least forgetting during replay training. While there are definitely some cases where explicit loss-weighting might be on par with a balancing strategy, these cases generally seem to be very rare or only occur in task splits where the replay strategy has no significant effect at all (e.g. U-CP1).

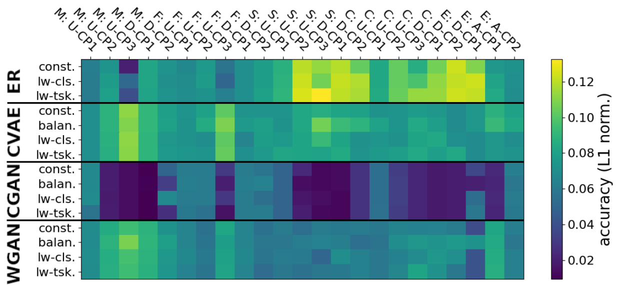
Re-using or re-starting generators makes little difference, as described by Fig. 3 (e.) and (f.). We found only small fluctuations in accuracy and forgetting across all experimental groups, which may result from statistical effects.

4. Discussion

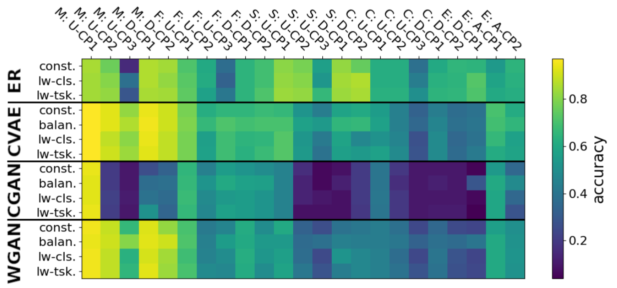
Construction of proper CIL evaluation protocols: The usage of simplified benchmarks based on a minimalist protocol may mask weaknesses of replay methods in CL. We have identified some of the relevant criteria to consider in a CIL scenario.



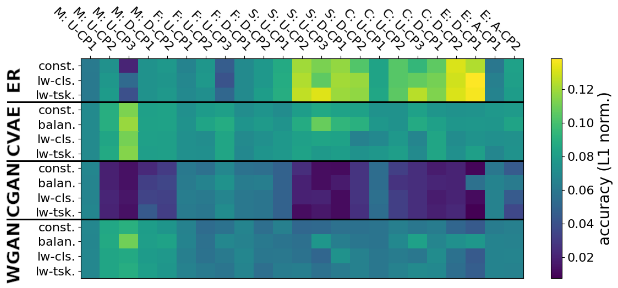
(a) Warm-start: Absolute accuracy.



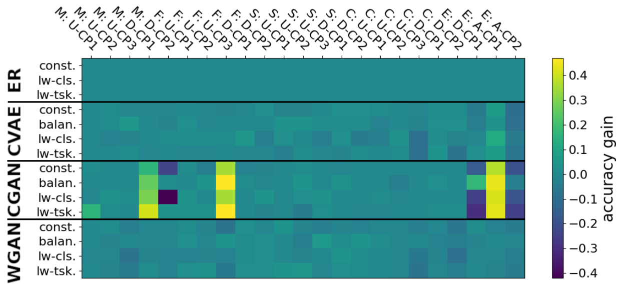
(b) Warm-start: Normalized accuracy.



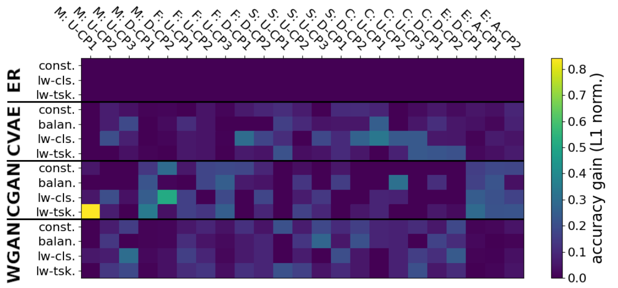
(c) Generator reset: Absolute accuracy.



(d) Generator reset: Normalized accuracy.



(e) Difference in absolute accuracy between (c.) and (a.).



(f) Difference in normalized accuracy between (c.) and (a.).

Figure 3. Final accuracy α_{end} for all investigated datasets/task splits. Each column represents a distinct task split on each dataset, whereas the first letter ("M", "F", "S", "C" stands for MNIST, Fashion-MNIST, SVHN and CIFAR-10), followed by the task split descriptor from Tab. 1. The deployed CL methods (4 groups * N rows) were modified as explained in Sec. 2.3: *const.* = constant, *balan.* = *balanced* (DGR exclusive), *lw-cls.* = loss-weighting by class count, *lw-tsk.* = loss-weighting by task count. The results for ER (top-most three rows) are re-used and serve as a baseline for the results in (c.-f.). Fig. (a.) and (b.) show the absolute and normalized (over each column vector) values for a warm-start, while Figs. (c.) and (d..) display the results when generators are reset after each task. Figs. (e.) and (f.) show the differences between resetting and re-using generators.

More interesting benchmarks should preferably involve a long sequence of tasks and impose constraints in terms of a limited computational and memory budget, as discussed in [15]. However, we have empirically confirmed that the length of the task sequence alone is not a clear indicator of the overall complexity of an objective, but rather must be considered as one piece of the puzzle when constructing appropriate CIL benchmarks. We assume that the mixture of the total amount of samples/classes, their temporal occurrence and balancing in each training phase, as well as the resulting interference between already captured and newly arriving data statistics are of central importance and must be considered holistically. We propose implementing a comprehensive CIL evaluation that considers these factors, rather than relying on commonly used but often uninformative and misleading CIL evaluation methods found in the literature. What we have not yet constructed is a natural imbalance for the number of samples per class except for SVHN, which would be a proper addition to extend our experimental framework. Furthermore, an aspect such as natural repetition [10] could be combined with the formulation of the diminishing and alternating task splits to render a CIL problem more realistic. An efficient knowledge adaptation is required here, and the CL method has to be able to deal with repetitive patterns from a previous distribution while encountering new data to add to its knowledge base. The experimental evaluation also showed that the task order plays an important role in the evaluation, see e.g. ER on ”M: U-CP3” from Fig. 3a. These results are far from the reported results of other empirical studies on exactly the same split [4]. This could be due to the fact that the solver's parameter set θ_T at the end of training resides in the low-loss region of the first task, since the same network is reused and not reinitialized, in contrast to e.g., GDumb [42]. This should underline the need for stronger randomization in CL benchmarking and its crucial role in creating meaningful CIL experiments. We also make a more practical reference to a real-time application with splits such as A-CP1, A-CP2 and D-CP1, D-CP2, since a CL model ultimately reflects the accumulated knowledge over a larger corpus of previously collected data and therefore must be able to adapt to the assumption that the amount of newly added data is dynamic or eventually represents only a small fraction of the total knowledge.

Identifying efficient generators for replay: Since factors such as memory consumption and a limited compute budget of CL methods are undeniably important metrics [13, 20, 43], we should not be guided solely by the resulting accuracy when evaluating the usability of a method. We find that cVAEs are the best-performing generative model in the context of the replay efficiency and resulting solver accuracy for DGR, as cGANs often suffer from mode collapse [5, 44, 51], while WGANs with GP are slower by a factor of three during training. When combining generative models with the presented replay strategies, we identified the balanced scenario as the best performing one. This is in line with the views of [33], where it was observed that to ensure a balanced distribution of classes, the number of generated samples must be rescaled linearly with respect to the number of tasks to ensure stable generators. However, this approach is accompanied by a worse runtime and a higher consumption of intermediate memory to compensate for the loss of knowledge. It would also be interesting to investigate whether there is a perfect timing for replaying certain aspects of the data as discussed in [26], and combine this with a dynamically balanced replay mechanism, as this could also reduce the reliance on sharp task boundaries to trigger generator re-training, which is a serious limitation in any streaming data setup.

The use of latent replay: Pre-trained (PT) models can be advantageous for several replay methods, and these advantages can vary greatly from algorithm to algorithm, while furthermore there appears to be different behavior for different types of PT models used [30]. Generative models are still limited in their capacity to model more complex distributions [2, 31] and therefore rely on PT models to be useful for datasets like SVHN and CIFAR-10/100. Our study uses supervised contrastive learning on the same data domain, but this could also be adapted to the self-supervised learning paradigm to better fit a real CL setting [6, 7, 14]. Empirical studies have shown that PT models can be combined with CL algorithms and applied to incremental batch learning [16] as well as to learning from streaming data [23].

Diffusion-based DGR: In recent years, denoising diffusion probabilistic models (DDPMs, [22]) have gained widespread attention. A few works have investigated CL with DDPMs as generators [11, 17, 36, 37, 57], reporting very promising results which comes att he cost of long training times and very large models. For most of these papers, it is not clear what replay strategy (balanced, constant, weighted) is used. In future work, will investigate DDPM-based generative replay, although we expect that the fundamental findings of this study will generalize.

5. Conclusion and take-home messages

The present study could be extended in several direction, mainly by varying more hyper-parameters, such as number of training epochs or generator/solver structure. Based on the presented results, we can formulate the following take-home messages for CL practitioners:

Balanced replay performs best This is observable across virtually all dataset splits and generative replay methods (WGAN and cVAE), but is most apparent for unbalanced task splits. The other weighting schemes we tested can be competitive for some datasets, but perform generally worse.

Warm-starting is feasible but not required Although warm-starting can improve convergence time, the final accuracies do not seem to depend on this choice at all.

ER is competitive for latent replay Although ER does not show the best performance for simple datasets, it excels when latent data are replayed. This is presumably since latent features are harder to model by generative models, and the replay of real samples gives an advantage.

Use cVAEs as generators Although generators based on WGAN-GP can be competitive, they suffer from long training times and the need to tune the number of training epochs via cross-validation which is in principle inadmissible. For cVAEs, early-stopping can be used since they minimize a loss function, which GANs do not.

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