



EGUsphere, author comment AC2
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Reply on RC2

Sébastien Gardoll and Olivier Boucher

Author comment on "Classification of tropical cyclone containing images using a convolutional neural network: performance and sensitivity to the learning dataset" by Sébastien Gardoll and Olivier Boucher, EGU sphere,
<https://doi.org/10.5194/egusphere-2022-147-AC2>, 2022

Section 1

- In abstract, clarify that this work is based on TCs in the North Atlantic only.
- Line 10: this sentence sounds incorrect
- The motivation of this study is not clear. I think the main purpose of this study is not simply showcasing the performance of CNN in TC detection—but examining the sensitivity of CNN-based TC detection algorithm to input and training datasets (as written in L49-50 and L53-54) and/or presenting a new way to preparing the training datasets for TC detections (as shown in figures 2 and 3). However, this point is not effectively highlighted in the introduction; in fact, it is not mentioned at all. My suggestion is to restructure the introduction to make clear the purpose of this study and also state the main purpose of this study clearly in the abstract.

We have restructured and strengthened the introduction to better explain the motivation of the study.

We also restructured the abstract:

"Tropical cyclones (TCs) are one of the most devastating natural disasters [...] This study compares the performance and sensitivity of a CNN to the learning dataset. For this purpose, we chose two meteorological reanalysis, ERA5 and MERRA-2, and used a number of meteorological variables from them to form TC-containing and background images..."

- (L27-29; L347-389) The authors claim this work contributes towards "automatic detection of TC in climate simulations without the need to retrain the CNN for each new climate model or climate model resolution." However, this point needs to be further justified. First, the cross examination (e., training on dataset A and evaluating on dataset B) would give more consistent results for two same-generation reanalyses than for two different climate models. Reanalysis datasets are at least grounded by observations; but two different climate models can wildly differ (e.g., dynamical core, subgrid schemes, and coupling between components). Second, the current study examined only 0.25° and 0.5° resolutions which is much finer than the majority of CMIP-class climate models (~1°). The authors test interpolation from a finer to a coarse resolution, and accordingly, some high-resolution information is still carried over to interpolated low-resolution fields. However, the native resolution of common climate

models is already too coarse ($\sim 1^\circ$) and their output variables calculated on a coarse grid are likely to miss finer dynamics/physics. In this regard, the author's CNN algorithm may not work well for climate model applications.

The Reviewer is right that we do not demonstrate that we can train the CNN on reanalysis and successfully retrieve TC in climate model simulations. This is certainly fraught with difficulties but this remains our objective in the near future.

We agree that TC are not simulated or at least not well simulated at resolutions less than 100 km (see Strachan *et al.*, 2013). However climate models running at resolution of 10 to 50 km are now commonplace (see e.g. HighResMIP and references in our manuscript). We do not argue that the CNN algorithm should be applied to 1° resolution climate models but only to the high-resolution models. Data augmentation methods (as discussed in the response to Reviewer #1's comments) may also help in this respect. We have strengthened the discussion section of the revised manuscript in that respect. The additional text reads:

"... Such [augmentation] techniques could however become very relevant in future work when we seek to detect TC in climate model simulations with a CNN trained on a reanalysis dataset. Indeed different climate models may simulate TC imperfectly and there is probably some value in offering a larger variety of TC structures to the training dataset. It is expected that the simulation of TCs increases in quality with the climate model resolution (Strachan *et al.*, 2013) and climate models running at resolutions of 10 to 50 km are now commonplace. Likewise we would need to augment the number of images with very intense TC or TC migrating outside their usual domains because there are indications that such situations may become more frequent with global warming and we want to ensure these can be detected adequately in climate simulations."

Furthermore we have started to collaborate with colleagues (S. Bourdin and S. Fromang) in our institute who have applied a physical detection of TC in a high-resolution version of the IPSL climate model (see Bourdin *et al.*, 2022).

Additional references:

Bourdin, S., S. Fromang, W. Dulac, J. Cattiaux, and F. Chauvin, Intercomparison of four tropical cyclones detection algorithms on ERA5, *EGUsphere* [preprint], <https://doi.org/10.5194/egusphere-2022-179>, 2022.

Strachan, J., P.L. Vidale, K. Hodges, M. Roberts, and M. Demory. Investigating global tropical cyclone activity with a hierarchy of AGCMs: The role of model resolution, *Journal of Climate*, 26(1), 133-152, 2013. doi: 10.1175/JCLI-D-12-00012.1

- (L30-40) The physical algorithms require preset thresholds. It provides intrinsic weakness because the performance of a given algorithm would depend on those thresholds. However, CNN classification (and, generally, any ML approaches) also suffer from similar issues. The performance of CNN is sensitive to the choice of hyperparameters. Unlike the thresholds used in physical algorithms (which can be physically interpreted), we do not know how each of these hyperparameters affects the performance of neural networks. As the thresholds in a physical algorithm is tuned, the hyperparameters of neural networks are tuned. In this view, the ML approach does not solve the problems related to thresholds in a physical mechanism. Besides, the authors point out that physical algorithms are *usually* applied in a limited domain. I wonder if it is due to the incapability of such algorithms to be applied in a wider domain—or if it is simply that people have not tried to apply it for a wider domain. If the latter is the case, the ML approach does not really address the issue of limited domains.

The Reviewer is right that ML approaches do not bypass the need to “tune” the retrieval models. Like for physical algorithms, some of this tuning can however be done in a more or less objective way (see our response to Reviewer #1).

Both physical and ML approaches can be applied to wider domains, which is relevant if the expected location of TC changes with global warming (see our response to Reviewer #1).

We have modified the text of the revised manuscript to make this clearer. The physical algorithms used to find cyclone tracks are based on thresholds calculated using reanalysis data (Bourdin *et al.* 2022). These algorithms are currently applied to simulation data, without changing the value of their thresholds. Of course, this practice is debated in the community and some argue that they should be adapted. It is clear that reanalysis data and climate model simulations represent cyclones in different ways because of model imperfections. We plan to compare deep learning models and physical algorithms that are trained and/or tuned on reanalysis data and applied on climate simulations. In particular we want to know if a deep learning model is able to generalize better than physical algorithms, when using simulation data. However, we felt it was important to study the effect of the learning dataset as a preliminary step towards answering this question. In addition to the changes previously presented, we have added these lines in the Introduction:

“It is common practice that data from climate simulations are first produced and stored, and then analyzed. However the climate modelling community is also moving in the direction of “on-the-fly” (also called in situ) data analysis in order to reduce the volume of data to be stored and the environmental impacts of such storage. This paradigm change implies the development of more efficient analysis methods. Both physical algorithms (i.e., trackers) and DL models are legitimate approaches to study TCs but it is useful to understand if one generalizes better than the other. However, it is important to understand that both approaches do not necessarily achieve the same thing. Indeed, trackers search for the trajectory of a cyclone by detecting its different positions in time, whereas the DL models listed in Table 1, derived from computer vision, detect cyclones on an image frozen in time. Other DL approaches are available for object tracking.”

- (L41-54) Several ML studies has been cited in this paragraph. However, the current manuscript does not clearly present the weakness (or strength) of these previous studies over this study. Since this study is extending the ML approach side of TC detection algorithms, the previous studies (in terms of their algorithms and performance) need to be introduced with more details such as which aspect of these studies are limitations and how this study improves on those points.

This point was raised by Reviewer #1 as well. We have summarized in a Table the neural network architectures that have been used to perform TC classification, detection and segmentation over the last 8 years.

Since Liu *et al.* (2016), whose deep learning model only classifies pre-existent images, various subsequent studies have focused on improving the detection of all cyclones at once present in unidimensional or multidimensional meteorological images (e.g., Ebert-Uphoff and Hilburn, 2020) and climate model data (e.g., Matsuoka *et al.*, 2018). The detection can be either “coarse” by drawing rectangular envelopes around the cyclones ((the studies are flagged as detection in the Purpose column of the Table), or “precise” by drawing the contours of the cyclones including their internal structure (studies flagged as segmentation). The main idea of these more recent studies is to apply new deep learning model architectures coming from computer vision research (e.g., U-Net, DeepLabv3, YOLOv3, Single Shot Detector, etc.) to the analysis of meteorological features such as cyclones.

This has given us the opportunity to clarify our motivations in the Introduction section of the revised manuscript. To our knowledge, no study has investigated the impact of interpolation on the performance of a CNN or compared the performance of a CNN trained on different image datasets with a statistically more robust method.

We have chosen a cyclone image classification approach mainly to avoid the difficult problem of data labeling required for cyclone detection or segmentation and the overfitting of these methods when the dataset is of modest size.

It is also interesting to note that we provide a new hint on the superior information richness of ERA5 over MERRA-2 for cyclone detection, which was strongly suggested by the study of Malakar *et al.* (2020) who conclude that ERA5 provides the best representation of the TC structure in terms of intensity, out of five reanalysis including MERRA-2.

We have added the following lines in the Section 1:

“There is also a wealth of studies on the detection of TCs in satellite imagery, reanalysis and climate model outputs based on machine learning (ML) approaches. Table 1 summarizes notable studies published in the last eight years that implement neural architectures based on convolution layers. It is not surprising that this approach was favored because TCs have very distinct features which make them relatively easy to detect with convolutional neural networks. Since Liu *et al.* (2016), whose deep learning (DL) model only classifies pre-existent images, various subsequent studies have focused on improving the detection of all cyclones at once present in unidimensional or multidimensional meteorological images (e.g., Ebert-Uphoff and Hilburn, 2020) and climate model data (e.g., Matsuoka *et al.*, 2018). This latter work focuses on the detection of cyclones using a CNN image classifier which operates on a sliding window of output from the Nonhydrostatic Icosahedral Atmospheric Model (NICAM) and studies the system performance in terms of detectability. The detection can be either “coarse” by drawing rectangular envelopes around the cyclones (the studies are flagged as detection in the Purpose column of the Table) or “precise” by drawing the contours of the cyclones including their internal structure (studies flagged as segmentation). The main idea of these more recent studies is to apply new DL model architectures coming from computer vision research (e.g., U-Net, DeepLabv3, YOLOv3, Single Shot Detector, etc.) to the analysis of meteorological features such as cyclones.”

Authors	Year	Purpose	Dataset	Variables	NN Archite cture	Performanc e at best
Liu <i>et al.</i>	2016	Cyclone image classification	ERA-interim (rea) CAM5.1 (mod) NCEP-NCAR	Pressure sea ad hoc level, wind vectors at 10 meters and at 850 hPa, temperature at 200 and 500 hPa, total column water	ad hoc CNN	Accuracy: 99 %

(rea) vapour

20th
Century
Reanalysis

Racah *et al.* 2016 Cyclone segmentation CAM5 (mod: 25 km) The 16 channels of CAM5 ad hoc autoencoder mAP@IoU=0.1: 52.92 %

Hong *et al.* 2017 Eye detection COMS-1 (sat) 4 IR channels GoogLeNet RMSE: 0.02

Matsuoka *et al.* 2018 Detection of cyclone by sliding window and cyclone image classifier NICAM (mod: 14 km) + NSW6 + HadISST Outgoing longwave radiation ad hoc CNN Probability of detection: 79.9–89.1 %

false alarm ratio: 32.8–53.4 %

Kumler-Bonfanti *et al.* 2020 Cyclone segmentation GFS (mod) Total precipitable water U-Net Accuracy: 0.991

Dice coeff: 0.763

Tversky coeff: 0.75

Shakya <i>et al.</i>	2020	Cyclone image classification	KALPANA-I (sat) MOSDAC (sat)	IR, Vis	ad hoc CNN	Accuracy: 97 %
Shakya <i>et al.</i>	2020	Cyclone detection and path prediction	KALPANA-I (sat) MOSDAC (sat)	IR, Vis	RetinaNet and polynomial regression	RMSE: 5-15.55 %
Prabhat <i>et al.</i>	2020	Cyclone segmentation	CAM5.1 (mod: 25 km)	Integrated vapor transport, integrated water vapor, vorticity, wind vectors at 10 meters and at 850 hPa, sea level pressure	DeepLabv3+	IoU: 0.2441
Pang <i>et al.</i>	2021	Cyclone detection	Satellite images from NII	Vis	DCGAN and YOLOv3	Accuracy: 97.78 % mAP@IoU=0.5: 81.39 %
Shi <i>et al.</i>	2022	Extratropical cyclone detection	ERA5 (rea)	Top net thermal radiation, mean sea level	Single Shot Detector	mAP@IoU=0.5: 79.34-86.64 %

pressure,
vorticity

Table. Summary of previous studies aiming at the detection or the segmentation of TC in satellite or model data. coeff: coefficient; IoU: intersection over union; IR: infrared; mAP: mean average precision; mod: model; rea: reanalysis; RMSE: root mean square error; sat: satellite; vis: visible.

Additional reference:

Malakar, P., Kesarkar, A. P., Bhate, J. N., Singh, V., & Deshamukhya, A. (2020). Comparison of reanalysis data sets to comprehend the evolution of tropical cyclones over North Indian Ocean. *Earth and Space Science*, 7, e2019EA000978. doi: 10.1029/2019EA000978

- L49: "lack sufficient details": elaborate "details"

As far as we can see, the published studies do not especially describe in detail the data engineering involved for training and evaluating models in deep learning applied to climate sciences. Moreover, not all publications describe the evaluation methods of the metrics used to measure the models. Thus, we have shed light on the preparation of the image sets, described the background image generation algorithm which seeks to avoid at all costs any correlation with the cyclone images and background images already generated, while seeking to maximize the classification decision boundaries. We also described precisely our iterative cross validation method that allowed us to evaluate the uncertainty on the values of the model performance metrics during our numerical experiments. Finally, we would like to caution against the commonly used accuracy metric, which has a bias introduced by its class threshold.

We have modified the manuscript to discuss this:

"While such techniques are now mainstream, they are not always well documented and their description may lack sufficient details which are often key in ML, like the data engineering involved in the preparation of the training dataset, the hyperparameters of the CNN and the evaluation methods of the metrics used to measure the performance of the models."

- L75-76: Explain why tropical depressions are not included as TCs. Is the performance of the current CNN algorithm sensitive to the inclusion of tropical depressions as TCs?

We chose to exclude tropical depressions (corresponding to the HURDAT2 status "TD") as well as the other weather events of lower intensity, because we wanted to focus on intense cyclonic events (> 34 knots) which are responsible for the largest impacts when they reach land. Our final objective is to detect cyclone in climate simulations.

This said we believe that the CNN would be sensitive to the characteristics of tropical depressions. As mentioned in the section "potential future work", a ternary classification experiment can be envisaged by adding the tropical depression class in the training dataset.

We have modified the manuscript to explain why we choose to exclude TDs in Section 2.1 on TC dataset:

"We chose to exclude tropical depressions (corresponding to the HURDAT2 status TD) as well as the other weather events of lower intensity, because we wanted to focus on intense cyclonic events (> 34 knots) which are responsible for the largest impacts when they reach land."

We also discuss a multiclass classification (including extra tropical cyclones) in the Discussion section:

"We have chosen a binary approach for the classification (i.e., TC or background), but it is quite possible to design a classifier predicting the range of HURDAT2 status of the images. Such a classifier would use nine neurons with the soft max activation function as the last layer of the CNN. However, training it would probably face an acute problem of image set imbalance. Indeed, four classes out of nine have a number of occurrences smaller than 400 in HURDAT2 (see Fig. A1). To improve the situation, it would be possible to merge some classes (e.g., WV with DB and SD with SS) in order to mitigate the problem."

Section 2

- Section 2.2: The difference and similarity between ERA5 and MERRA2 need to be presented in more detail. The atmospheric models and assimilation schemes are surely different, but the observation dataset they use might have good overlaps. How different these two datasets are is important to interpret the results of cross examination. That is, if they are only slightly different, the robust results from cross examination could be rather trivial.

The Reviewer is right that we expect two high-quality reanalyses to be "relatively similar" because they ingest similar observational data. At least the representation of TCs should be more similar in two different reanalyses than in two different climate models. This is probably why we do see robust results from our cross training methods. However there are also differences as we show in the manuscript. The literature also provides some elements for a more thorough discussion.

The representation of TC by ERA5 and MERRA-2 was studied at least by two studies: Malakar *et al.* (2020) and Hodges *et al.* (2016).

The study of Malakar *et al.* (2020) analyzes the error in the location of the center, maximum winds and minimum pressure at sea level in six meteorological reanalyses including ERA5 and MERRA-2 for the evolution of 28 TCs occurring between 2006 and 2015 over the North Indian Ocean, with respect to the observations of the Indian Meteorological Department (IMD). The authors of this study show, among other things, that the ERA5 dataset captures the evolution of these TCs in a more realistic way than MERRA-2 (i.e. smaller errors in the previous variables). They also show that ERA5 and MERRA-2 can capture the intensity of the TCs from the depression stage to the very severe cyclonic storm stage but not the last stage which is the extremely severe cyclonic storm stage: the intensity of the TCs is underestimated. However, they conclude that of the six datasets, ERA5 provides the best representation of the TC structure in terms of intensity.

The study published by Hodges *et al.* (2016) shows that 95 % of the Northern Hemisphere TC tracks, from the IBTrACS database that includes HURDAT2, are present in MERRA-2. Unfortunately, this study does not include ERA5. It also confirms the underestimation of cyclone intensity in MERRA-2 compared to observations.

The conclusion has been modified as in the response to Reviewer #1's comments:

"Applying an ERA5-trained CNN on MERRA-2 images works better than applying a

MERRA-2 trained CNN on ERA5 images, which suggests that ERA5 has a larger information content in the framework of our CNN.

This is also consistent with the findings of Malakar et al. (2020) who analyzed the error in the location of the center, maximum winds and minimum pressure at sea level in six meteorological reanalyses including ERA5 and MERRA-2 for the evolution of 28 TCs occurring between 2006 and 2015 over the North Indian Ocean, with respect to the observations of the Indian Meteorological Department (IMD). The authors of this study show, among other things, that the ERA5 dataset captures the evolution of these TCs in a more realistic way than MERRA-2 (i.e., smaller errors in the previous variables). They also show that ERA5 and MERRA-2 can capture the intensity of the TCs from the depression stage to the very severe cyclonic storm stage but not from the extremely severe cyclonic storm stage for which the intensity of the TCs is underestimated. However, they conclude that of the six datasets, ERA5 provides the best representation of the TC structure in terms of intensity. Finally, the study published by Hodges et al. (2017) shows that 95 % of the Northern Hemisphere TC tracks, from the IBTrACS database that includes HURDAT2, are present in MERRA-2. Unfortunately, this study does not include ERA5. It also confirms the underestimation of cyclone intensity in MERRA-2 compared to observations.”

Additional reference:

Hodges, K., Cobb, A., & Vidale, P. L. (2017). How well are tropical cyclones represented in reanalysis datasets?, *Journal of Climate*, 30(14), 5243-5264.

Section 3

- Section 3.2.6: I assume we are using bi-linear interpolation here?

Indeed, we applied a bi-linear interpolation. This information will be added in the revised manuscript.

- L226-240: Rewrite. The authors' choice (iterative cross-validation) was introduced in a confusing way (after hold-out and k-fold). Consider starting the paragraph with mentioning iterative cross-validation and then explaining its benefit over k-fold.

We have rewritten the description of the evaluation of the metrics as following:

“For the evaluation and comparison of the metrics (developed in Section 4.2), we wanted to be able to calculate the expected value and the uncertainty of the metrics, without bias. To that end, we applied an iterative cross validation method which consists in repeating 20 times a cross validation method. We chose the k-fold method (Bishop, 2006), with k equal to ten, as the cross validation method. We obtained a mean of the metrics for each k-fold iteration. By applying the central limit theorem on this set of metric means, we could compute the expected value and the uncertainty of the metrics.

In order to avoid any bias, we took care to check if the central limit theorem can be applied, by testing the normality of the distribution of the metric means using the Shapiro-Wilk statistical test (brief non-mathematical presentation given in Appendix B1). Moreover, images coming from a time series of tracks from the same cyclone may be found in both the training and test datasets, which would induce some dependance between the training and test datasets due to the autocorrelation within individual cyclone tracks. In order to avoid such a bias, the k-fold split is based on sampling the years randomly and balancing the folds as much as possible. The partitioning combinations are calculated in advance in order to guarantee the uniqueness of their composition. Scale bias is also avoided by standardizing the channels of the images online, just before

training the CNN.

Finally, for the comparison of the metric means, we chose to apply the Kruskal-Wallis statistical test (brief non-mathematical presentation given in Appendix B2) for an alpha level of 1 %, because the Shapiro-Wilk test was negative for most distributions of metric values of our experiments, invalidating the use of the Student's t-test.

For the experiment of highlighting the problem with the accuracy (point developed in Section 4.1), we applied the classical hold-out method, avoiding the autocorrelation between images belonging to a same cyclone track, with the following partitioning: 70 % of the data for the training dataset and 30 % of the data for the test dataset.”

New reference added:

Bishop, C. M., *Pattern Recognition and Machine Learning, 1, Springer International Publishing, 2006, 32-33*

- [3] Lack of details on the methodology of statistical tests and metrics: I do appreciate the authors' efforts to use more robust statistical metrics and tests. I believe the following metrics and tests are less common (unlike 'accuracy'): AUC, AUPRC, Shapiro-Wilks test, and Kruskal-Wallis test. Including brief mathematical descriptions for those metrics and tests—maybe in the appendix—would be a great service to readers (including myself) who are not familiar with them.

We have briefly described the usefulness of the AUC and AUPRC metrics in Section “3.4 Evaluation of metrics”. As the Reviewer points out, it is necessary to add some explanations of these notions with some Figures. Here is what we have added to the revised manuscript:

“The AUC measures the power of a model to discriminate between the two classes for a variety of decision threshold values. The AUC is, as the acronym indicates, the area under the ROC curve which is plotted by the values that recall (the ability of a model to identify all occurrences of a class) and false positive rate of a model take (definitions are given in Appendix B1). The recall and false positive rate values are calculated according to the ground truth and the classifier responses for a given test dataset and for all possible decision thresholds (or a set of discrete values). A perfect classifier has an AUC equal to 1, recall all the images of cyclone with a null false positive ratio.

The AUPRC measures the recall of a model while minimizing the precision (prediction errors). The AUPRC follows a similar approach to the AUC: it is the area under the curve which is plotted by the values that the precision and recall of a model take (for all possible decision thresholds, etc.). A perfect classifier has an AUPRC equal to 1, recall all the images of cyclone without wrongly classifying any background image as a cyclone image.”

We also have explained in Appendix the usefulness of the Shapiro-Wilks and Kruskal-Wallis statistical tests, as well as a simple explanation of how they work, without going into mathematical details that we find a bit irrelevant:

“Let us consider a group, a set of values of a random variable that we observe, for example during an experiment, and its population, the set of all the values that the variable can take for a particular experimental context. The group is a subset of the population.

The Shapiro-Wilks test poses the so-called null hypothesis (H_0) that the group of values that a given quantitative variable takes, comes from a normally distributed population. For a non-technical explanation of the test, we can say that the Shapiro-Wilks test quantifies

in a single metric, the p-value, the dissimilarities between the distribution of the values of the group and the distribution of the population of the variable if it was normal.

For a risk of error called alpha level, commonly fixed at 1 % or 5 %, H_0 null is rejected if the p value is lower than the alpha level and is accepted if the p value is higher than the alpha level. The latter represents the risk of accepting H_0 when it is not true: a false positive. If H_0 is accepted, random sampling of the group can explain the dissimilarities between the distribution of the values of the group with a normal population distribution. If H_0 is rejected, it can be stated that the population of the variable is not normally distributed.

The comparison of the mean of the values of a variable from two different groups is usually done by the Student's t-test (or two-sample ANOVA) or the Kruskal-Wallis test. The Student's t-test is a parametric statistical test, in this case it requires that the distribution of the given variable is normal. The Kruskal-Wallis test is non-parametric and does not require the assumption of the population distribution of the variable. Indeed, it is not based on the value that the variable takes, but on its rank in the classification of the observed values of the variable. As for the Shapiro-Wilks test, we propose a non-technical explanation of the Kruskal-Wallis test: this test quantifies in a single metric, the p-value, the dissimilarities between the mean ranks computed for two or more groups of values, with, as H_0 , random sampling of the group being able to explain the differences between the medians of the two groups, because they may come from the same population."

- Insufficient details of the authors' ML methodology: I suggest adding more details about how the authors implemented in the *main* text.
- Move Tables A2 and A3 to the main text. These are key information for ML implementations. Since this paper evolves around ML, these bits of information should be included in the main body of the manuscript.

We agree with the Reviewer and have moved Tables A3 and A2 to Section 3.3.

- Table 1: Add information about stride and padding for convolution and pooling layers. Readers can infer them from the values in the table, but it is better explicitly shown. Also, specify pooling was done using either 'max' or 'averaging'.

We agree with the Reviewer: we will add in the caption of the Table that there was no padding added and that the stride was (1, 1). We will also specify that pooling is max pooling.

- Table A3: Add a column that shows the search space for hyperparameter search.

We agree with the Reviewer: we have added the following information as a new column in Table A3:

Hyperparameter	Value	Search space
Loss function	Binary cross-entropy	-

Training metric	Loss computed on test set	-
Maximum number of epoch	100	-
Early stopping number of epoch	10	-
Batch size	256	from 32 to 256, step of 32
Optimizer	Adam	Adam; SGD
Learning rate	0.0001	0.0001; 0.001; 0.01

- The hyperparameter optimization done in this study does not include some of the important hyperparameters, *g.*, the number of layers, the number of neurons per layer, and the number of filters (convolution). It is likely that these hyperparameters would have impact on the performance of the CNN model more strongly than those listed in table A3. Provide justification on why these hyperparameters were omitted during the hyperparameter search process.

The authors agree with the Reviewer: our hyperparameter optimization does not include the architecture of the CNN. First of all, we would like to clarify our intention regarding the optimization of the hyperparameters: our manuscript is based on the paper written by Liu *et al.*, but these authors do not mention the values of their training hyperparameters, i.e. batch size, optimizer and learning rate. Instead of fixing these values in an arbitrary way, we search for local optimal values of these hyperparameters to maximize the performance of the CNN.

The following lines have been added to Section 3.1 Classification model:

“We did not try to optimize the architecture of the CNN proposed by Liu *et al.* because we believe they have already optimized it and the modifications we have made do not require any further optimization, since the performance of our CNN is very close to that of Liu *et al.* Finally, as explained above, we prefer to focus on the performance and the sensitivity of the CNN to the learning dataset instead of obtaining better performances.”

- In Section 3.1 (or somewhere in Section 3), explicitly write how the input vectors were normalized and/or transformed. This information is presented vaguely later in the manuscript (L241-243), but it should be written more clearly, considering input vector normalization is one of the critical factors that determines the performance of ML algorithms.

We agree to mention in Section 3 on model training how the data of each channel taken separately were standardized, as it is a necessity for the training of the CNN and we recall it at line 241. We have added the following section:

“3.2.7 Data standardization

Neural network models learn a mapping from input variables to an output variable. The input variables have nearly always different scales and large scale differences are detrimental to the learning process of neural networks. In order to ensure that each variable is equally important, whatever its range of values, input variables are rescaled to the same scale. There are several methods such as standardization (or Z-score normalization) which consists in recalculating the values of the variables so that their mean and standard deviation equal to zero and one, respectively. In our study, we have systematically standardized each channel of the images, by calculating the means and standard deviations of the channels on all the images of the training set. The validation and test image datasets are excluded from the calculation of the mean and standard deviation, to avoid that information about the validation and test datasets leak into the training phase. However the validation and test datasets are also scaled using the mean and standard deviation of the training dataset.”

- L176: Specify which task was for the 135-min CPU time.

It was for the extraction of all the channels of the images. We have added this information: “The CPU time was 135 minutes for the extraction of all the channels of the images.”

Section 4

- Insufficient comparison of the CNN model performance: Section 4 presents the performance of the author’s ML detection algorithm. However, it is hard to tell if the authors’ algorithm is good or not because the benchmark comparison with other state-of-art algorithms is not provided. Only benchmark comparison provided is “simple metrics” in Figure 7; even then, the details about “simple metrics” are not included in the manuscript. Regardless, *simple* metrics are not a fair benchmark for the current study. A better (and more useful) benchmark should be state-of-art TC detection algorithms—including both physical and ML algorithms—that has been presented in recent years (*e.g.*, some of previous studies listed in L31 and L42). In particular, Liu *et al.* (2016) which this study is based on, would provide an objective baseline. The suggested comparison will further enhance the merit of the authors’ work.

We agree with the Reviewer and have added the previous Table that sums up the experiences of detecting cyclones in meteorological images. It can be noted that the accuracy of our CNN is in the same range as Liu *et al.* (2016) with 99 % and Shakya *et al.* (2020) with 97 %.

We also have added these explanations about the simple metrics:

“Indeed, the usefulness of a model is measured by the difference between its performance and that of models based on simple rules or a domain specific baseline. For instance, we implement the following simple models (from the software library scikit-learn): “most

frequent” which always predict the most frequent class observed in the training dataset (i.e., background), “stratified” which generates randomly predictions at probabilities that respect the class distribution of the training dataset (i.e., 1/3 cyclone, 2/3 background), “uniform” which generates predictions uniformly at random background or cyclone with equal probability.”

We removed the “constant” and “prior” models that behave identically to “most frequent” in our experiment.

About the performance of algorithms based on physical equations, we can cite the prepublication by Bourdin *et al.* (2022) who performed an intercomparison of four cyclone trajectory detectors - called trackers - on the same ERA5 data. The authors report that the algorithms have a recall that ranges between 75 and 85 % and a false discovery rate between 9 and 36 % after post processing. However, it is important to understand that trackers do not have the same objectives as the DL models of classification, detection and segmentation listed in the previous Table. Indeed, trackers search for the trajectory of a cyclone by detecting its different positions in time, whereas DL models, derived from computer vision, detect cyclones on an image at a given point in time.

The following lines have been added in the Introduction:

“Bourdin *et al.* (2022) performed an intercomparison of four cyclone trajectory detectors –called trackers– on ERA5 reanalysis. [...] Both physical algorithms (i.e., trackers) and DL models are legitimate approaches to study TCs but it is useful to understand if one generalizes better than the other. However, it is important to understand that both approaches do not necessarily achieve the same thing. Indeed, trackers search for the trajectory of a cyclone by detecting its different positions in time, whereas the DL models listed in Table 1, derived from computer vision, detect cyclones on an image frozen in time. Other DL approaches are available for object tracking.”

- L216: What was the main criticisms, offered by Provost *et al.* (1997) and Ling *et al.* (2003), to the accuracy metric?

The articles by Ling *et al.* (2003) and Provost *et al.* (1997) mainly concern a comparison between AUC and accuracy and suggest favoring AUC over accuracy.

The main criticism of Ling *et al.* (2003) towards the accuracy is about the loss of information it induces during the transformation of the probability, returned by the classifier, into a class identifier: as soon as this probability exceeds the class decision threshold, the response of the classifier takes the class identifier while the information of the difference between the value of the probability and the threshold is lost. The authors also report a loss of discrimination power on an unbalanced dataset. Note that while AUC and AUPRC also transform probabilities into class identifiers, these metrics retain the probability information, as they are computed for all threshold values.

Provost *et al.* (1997) propose two fundamental justifications to explain the use of accuracy as a machine learning model metric and show that both justifications are questionable at best. The first justification states that “The classifier with the highest accuracy may well be the classifier that minimizes cost, especially when the classifier tradeoff between true positive and false positive predictions can be resolved.” By cost, the authors mean the cost of missed classification, i.e., producing false positives and false negatives. As a reminder, depending on the context of the classification, a false negative can have very strong consequences (e.g. a medical test). The authors recall that accuracy assumes equal costs of misclassification. The second justification states that “The induction algorithm that produces the most accurate classifiers can also produce minimum cost classifiers by training it differently.”

For the first justification, the authors argue that in real world cases, the distribution of classes is generally not known. On the other hand, the authors also point out that it has been proven that it is virtually impossible to assign the misclassified costs precisely. Thus, the authors show that justification 1 is generally not verified, because it is impossible to optimize the misclassified rate.

For the second justification, the authors use the properties of ROC curves whose area is the value of the AUC. ROC curves allow to visually show that a classifier "dominates" another classifier with its ROC curve strictly above the other classifier's. The authors state that "a dominating model is at least as good as all other models for all possible cost and class distributions". The authors demonstrate on some examples that ROC curves, produced by the averaging 10-fold cross-validation method, are statistically intertwined without being able to declare one classifier dominating the others, while their accuracy is not significantly different.

We have summarized this explanation into this following sentence added in Section 4.1 on "Accuracy and its threshold":

"Accuracy is a convenient measure, but according to Provost et al. (1997) and Ling et al. (2003), the class threshold makes it non objective. Provost et al. (1997) argue that in real world cases, the use of accuracy as an ML model metric is questionable at best, because the distribution of classes is generally not known so it is impossible to optimize the misclassified rate. Furthermore, the authors demonstrate in some examples that classifiers, having their accuracy not significantly different, are not equivalent concerning the AUC. Ling et al. (2003) argue that the accuracy losses information during the transformation of the probability, returned by the classifier, into a class identifier: as soon as this probability exceeds the class decision threshold, the response of the classifier takes the class identifier while the information of the difference between the value of the probability and the threshold is lost."

- L256: Provide the usefulness of Youden's index, as done for AUC and AUPRC in L217-219

We have added some hints about the usefulness of Youden's index:

"Then we calculated the decision threshold for which the number of misclassified predictions is minimal. For this purpose, we use the Youden's index which is a measure of the tradeoff between sensitivity and specificity. Maximizing the index means minimizing the false positives and false negatives according to its equation described in Appendix C6, knowing that Youden's index varies according to the class decision threshold."

- Figure 8: The convention of box plots is standard, but I suggest including a brief explanation about what whiskers, boxes, and diamond markers stand for in the figure legend.

We have included a short explanation of the whisker plots in the caption:

"Box plots are a synthetic representation of a data distribution. They are composed of a box and two whiskers. The bottom of the box corresponds to the first quartile (Q_1) of the studied dataset, below which 25 % of the data are located. The middle line of the box is the median of the dataset, and the top line of the box corresponds to the third quartile (Q_3), below which 75 % of the data are located. The interquartile range is represented by the extent of the box: $IRQ = Q_3 - Q_1$. The bottom whisker is calculated according to the IRQ: $Q_1 - 1.5 \times IRQ$ and the top whisker: $Q_3 + 1.5 \times IRQ$. The data outside the whiskers are considered as outliers, and represented as diamond markers."

Section 5

- L288-290: The authors' conclusion seems premature. There are other possibilities, for example, the current CNN architecture might be somehow more suitable for ERA5 dataset.

This point was also made by Reviewer #1 and we agree that our conclusion should be slightly qualified as it may not generalize to other CNN architectures. We would like to clarify our conclusion about MERRA-2 having less information content for TC image classification than ERA5:

"Applying an ERA5-trained CNN on MERRA-2 images works better than applying a MERRA-2 trained CNN on ERA5 images, which suggests that ERA5 has a larger information content in the framework of our CNN.

This is also consistent with the findings of Malakar *et al.* (2020) who analyzed the error in the location of the center, maximum winds and minimum pressure at sea level in six meteorological reanalyses including ERA5 and MERRA-2 for the evolution of 28 TCs occurring between 2006 and 2015 over the North Indian Ocean, with respect to the observations of the Indian Meteorological Department (IMD). The authors of this study show, among other things, that the ERA5 dataset captures the evolution of these TCs in a more realistic way than MERRA-2 (i.e., smaller errors in the previous variables). They also show that ERA5 and MERRA-2 can capture the intensity of the TCs from the depression stage to the very severe cyclonic storm stage but not from the extremely severe cyclonic storm stage for which the intensity of the TCs is underestimated. However, they conclude that of the six datasets, ERA5 provides the best representation of the TC structure in terms of intensity. Finally, the study published by Hodges *et al.* (2017) shows that 95 % of the Northern Hemisphere TC tracks, from the IBTrACS database that includes HURDAT2, are present in MERRA-2. Unfortunately, this study does not include ERA5. It also confirms the underestimation of cyclone intensity in MERRA-2 compared to observations."

Technical comments

We have implemented these technical corrections:

- L45: This later work □ This latter work
- L83-84: Specify if 0.25x0.25 and 0.5x0.6 is either native or interpolated resolutions. If it is the latter, specify which interpolation scheme was used to regrid.
- L130-131: Rewrite the sentence: "thought", "those", and "they" are unclear.
- L134: Provide a proper citation for UML2.
- L203: Specify N. Is it 10 (Table A3)?
- Figures 4, 5, and 10: include colorbars.
- Figure 6: Specify the y-axis uses a logarithmic scale in the figure legend.
- Figure7: Define the x-axis tick labels in the figure legend, as done in Figure 9
- Figure 9: 'purple' looks more similar to red.
- L324: Figure number is missing.