
What I Cannot Predict, I Do Not Understand: A Human-Centered Evaluation Framework for Explainability Methods

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Abstract

A multitude of explainability methods and associated fidelity performance metrics have been proposed to help better understand how modern AI systems make decisions. However, much of the current work has remained theoretical – without much consideration for the human end-user. In particular, it is not yet known (1) how useful current explainability methods are in practice for more real-world scenarios and (2) how well associated performance metrics accurately predict how much knowledge individual explanations contribute to a human end-user trying to understand the inner-workings of the system. To fill this gap, we conducted psychophysics experiments at scale to evaluate the ability of human participants to leverage representative attribution methods for understanding the behavior of different image classifiers representing three real-world scenarios: identifying bias in an AI system, characterizing the visual strategy it uses for tasks that are too difficult for an untrained non-expert human observer as well as understanding its failure cases. Our results demonstrate that the degree to which individual attribution methods help human participants better understand an AI system varied widely across these scenarios. This suggests a critical need for the field to move past quantitative improvements of current attribution methods towards the development of complementary approaches that provide qualitatively different sources of information to human end-users.

1 Introduction

There is now broad consensus that modern AI systems might not be safe to be deployed in the real world [1] despite their exhibiting very high levels of accuracy on held-out data because these systems have been shown to exploit dataset biases and other statistical shortcuts [2–4]. A growing body of research thus focuses on the development of explainability methods to better interpret these systems’ predictions [5–12] to make them more trustworthy. The application of these explainability methods will find broad societal uses, like easing the debugging of self-driving vehicles [13] and helping to fulfill the “right to explanation” that European laws guarantee to its citizens [14].

The most commonly used methods in Explainable AI (XAI) are attribution methods, but despite a plethora of different approaches [5–9, 11, 15–17], assessing the quality and reliability of these methods remains an open problem. So far the community has mostly focused on evaluating these methods using surrogate measures defined axiomatically [13, 18–21]. The two most popular approaches are: (1) using ground truth annotations [8, 9, 11, 22–25] and (2) measuring *faithfulness* using objective metrics [8, 11, 22, 23, 26, 27].

* Equal contribution † Work done before April 2021 and joining Tesla

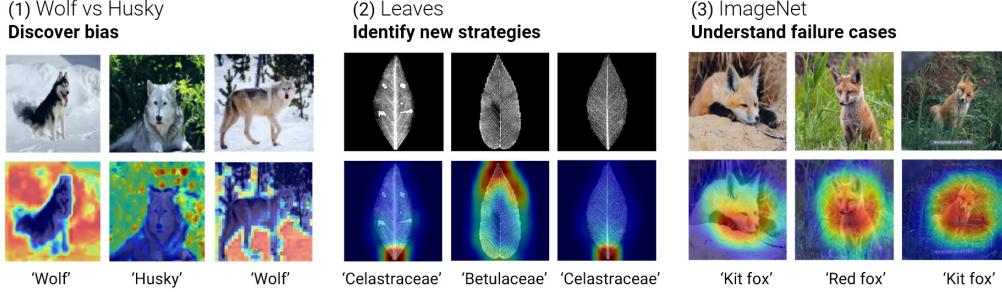


Figure 1: We study the practical usefulness of recent explainability methods in three real-world scenarios, each corresponding to different use cases for XAI. The first dataset is Husky vs. Wolf where the goal of the explanations is to help the user to identify a source of bias in a model (classification is based on the background (snow, grass) as opposed to the animal). The second dataset corresponds to a real-world leaf classification problem which is complex for non-experts. The goal of the explanations is to help the end-user identify the strategy discovered by the vision system. Finally, the third dataset is a subset of ImageNet, which consists of a collection of images where half have been misclassified by the system. The goal of the explanations here is to help the end-user understand the failure sources.

The first approach takes humans into consideration only in so far as it evaluates the alignment of an explanation with a human annotation. This metric assumes that the classifier relies on human-like features, which is an assumption that turns out to be erroneous [2–4], and unsurprisingly the results obtained on those benchmarks do not correlate with the ability of attribution methods to help humans in decision making tasks [28]. On the other hand, the second approach focuses purely on the relationship between the explanations and the model, i.e., they assess whether the explanations actually reflect the evidence for the prediction. They have emerged as the primary means of evaluating attribution methods, but because they take the human totally out-of-the-loop of the evaluation, it is not clear how well they can predict the practical usefulness of attribution methods and their possible failures.

As previously highlighted in [28], relying solely on current theoretical benchmarks to evaluate attribution methods can be a risky endeavor as they are totally detached from humans. Indeed, the seminal work of [29] has emphasized keeping in mind the end goal of XAI: to develop useful methods that help the user better understand their model. Thus, they recommend systematically having recourse to human experiences. Moreover, a large body of research from the psychology literature has endorsed the argument that the functional role of explanations is to support learning and generalization [30–36] – i.e., can an explanation help identify generalizable rules that readily transfer to unseen instances? Therefore, evaluating attribution methods on their ability to help users understand general rules to predict a classifier decisions is perfectly aligned with this vision.

To that end, [29] suggest evaluating explainability methods directly through the end goals of XAI. We subscribe to this idea and, in this work, consider three main real-world scenarios that illustrate potential applications of explainability methods: (1) identifying a potential bias in a system’s decision along with its source using the classic Husky vs. Wolf dataset [5] (previous work has documented the risk associated with a biased model [37–39]); (2) identifying novel strategies discovered by a system [40, 41] for tasks that would be nearly impossible for humans [42–45], here we consider a highly complex categorization problem for non-expert humans [46, 47]; and (3) understanding failure cases [4, 37] on a subset of ImageNet [48].

The main contributions of this paper are as follows:

- We propose a novel human-centered explainability performance measure together with associated psychophysics methods to experimentally evaluate the practical usefulness of modern explainability methods in real-world scenarios.
- Large-scale psychophysics experiments ($n = 1,150$) revealed that SmoothGrad [7] is the most useful attribution method amongst all tested and that none of the fidelity performance metrics appear to predict if and when attribution methods will be of practical use to a human end-user.
- Perceptual scores derived from attribution maps, characterizing either the complexity of an explanation or the challenge associated with identifying “what” features drive the system’s decision, appear to predict failure cases of explainability methods better than fidelity metrics.

2 Related work

Evaluations based on faithfulness measures Common approaches [8, 26] measure the fidelity of an explanation through the change in the classification score when the most important pixels are progressively removed. The bigger the drop, the more faithful is the explanation method. To ensure that the score drop in score does not come from a change in the distribution of the perturbed images, the ROAR [49] methods include an additional step whereby the image classifier is re-trained between each removal step. Because these methods do not require ground-truth annotations (i.e. object masks or bounding boxes), they are quite popular in computer vision [8, 11, 22, 23, 26, 27] and natural language processing [11, 50, 51].

Nevertheless, *faithfulness* measures have recently been criticized as they all rely on a baseline for removing important areas, a baseline that will obviously give better scores to methods relying internally on the same baseline. [52]. More importantly, they do not consider humans at any time in the evaluation. As a result, it is unclear if the most faithful attribution method is practically useful to humans.

Evaluations based on humans A second class of approaches consists in evaluating the ability of humans to leverage explanations for different purposes [5, 9, 28, 53–59]. [5] were the first to evaluate the usefulness of explanations. Their work focused on the use case of bias detection: they trained a classifier on a biased dataset of wolves and huskies and found the model consistently used the background to classify. They asked participants if they trusted the model before and after seeing the explanation for the model predictions. They found that explanations helped detect bias in this case. We use a similar dataset to reproduce those results as a verification, but our evaluation framework differs greatly from theirs as we do not ask if the participants trust the model but instead measure directly if they understand the model.

Closest to our work are [57–60]. [57–59] design their evaluation around the notion of *simulatability* [29, 61]. They introduce different experimental procedures to measure if humans can learn from the explanations how to copy the model prediction on unseen data. Some provide the explanations at test time [57, 58]. Similar to us but for tabular data, [59] proposes to hide explanations at test time, this forces the participants to learn the rules driving the model’s decision at training time where the explanations are shown. There are two limitations to their work: (1) they provide ground-truth labels associated with input images during training, (2) the participants see the same set of images without explanation, and then with explanations, always in that order. This creates learning effects that can heavily bias their results. We differ from their work by: (1) removing ground-truth labels from our framework as they serve no purpose and can bias participants, and (2) we have different participants go through the different conditions. This remove any learning effect, and more importantly, new explainability methods can be evaluated independently and still be compared to the previously evaluated methods. A very recent study [60] evaluated how AI systems may be able to assist human decisions by asking participants to identify the correct prediction out of four prediction-explanation pairs shown simultaneously. This measure reflects how well explanations help users tell apart correct from incorrect predictions. While the approach was useful to evaluate explanation in this specific scenario, it is not clear how this framework could be used to evaluate explainability methods more generally. Furthermore, when comparing different types of methods, they adapt the complexity of certain explainability methods to ease the task for participants. We argue that the complexity of explanation is an important property of explanations and that abstracting it away from the evaluation lead to unfair comparisons between methods. In contrast, we propose a more general evaluation framework that can be used for any kind of explainability method without the need to adapt them for the evaluation procedure – hence allowing for an unbiased comparison between methods.

3 Proposed evaluation framework

Before providing a rigorous definition of interpretability, let us motivate our approach with an example: a linear classifier is often considered to be readily interpretable because its inner working is sufficiently intuitive that it can be comprehended by a human user. A user can in turn build a mental model of the classifier – potentially predicting the classifier’s output for arbitrary inputs. In essence, we suggest that the model is interpretable because the output can be predicted. This concept of predicting the classifier’s output is central to our approach and we conceptualize the human user as a *Meta-predictor* of the machine learning model. This notion of Meta-predictor is also closely related to the notion of *simulatability* [22, 29, 59, 61, 62]. We will now define the term more formally.

We consider a standard supervised learning setting where f is a black-box predictor that maps an input $x \in \mathcal{X}$ (e.g., an image) to an output $f(x) \in \mathcal{Y}$ (e.g., a class label). One of the main goals

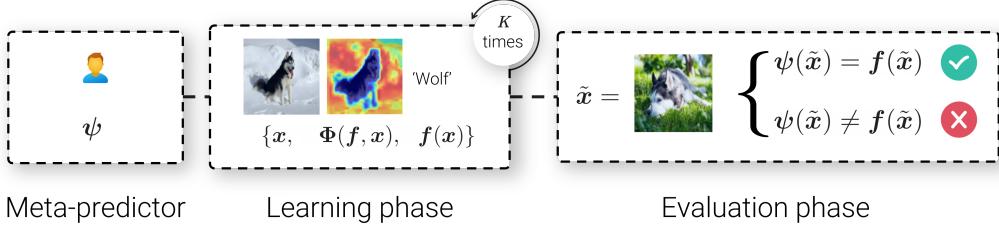


Figure 2: We describe a human-centered framework to evaluate explainability methods borrowing from the concept of Meta-predictor. The framework requires a black box model f , an explanation method Φ and a human subject ψ which will try to predict the predictor, hence, the name Meta-predictor. The first step is the learning phase where the Meta-predictor is training using K samples x , together with the associated model predictions $f(x)$ and explanations $\Phi(f, x)$. The goal of this learning phase is for the Meta-predictor to try to uncover a rule describing the functioning of the model from the triplets $(x, \Phi(f, x), f(x))$. Then, the second step is the evaluation phase where we test the Meta-predictor accuracy on new samples \tilde{x} by comparing its predictions $\psi(\tilde{x})$ to those of $f(\tilde{x})$. The better the predictions are, the more useful the rules learned during training and therefore the more relevant the method.

of explainable AI is to yield useful rules to understand the inner-working of a model f such that it is possible to infer its behavior on unseen data points. To this end, a current approach consists in studying explanations (Attribution Map, Concept Activation Vectors, Feature Visualization) for several predictions. Formally, Φ is any explanation functional which, given a predictor f and a point x , provides an information $\Phi(f, x)$ about the prediction of the predictor. In our experiments, Φ is an attribution method but we would like to remind that the framework is naturally adaptable to other explainability methods such as concept-based methods or feature visualization.

The understandability-completeness trade-off Different attribution methods will typically produce different heatmaps – potentially highlighting different image regions and/or presenting the same information in a different format. The quality of an explanation can thus be affected by two factors: *faithfulness* of the explanation (i.e., how many pixels or input dimensions deemed important effectively drive the classifier’s prediction) and the understandability of the explanation for an end-user (i.e., how much of the pattern highlighted by the explanation is grasped by the user).

At one extreme, an explanation can be entirely *faithful* and provide all the information necessary to predict how a classifier will assign a class label to an arbitrary image (by rendering all the parameters of the classifiers). However, such information will obviously be too complex to be understood by a user and hence it is not *understandable*. It should be noted that when the parameters are understandable, for a linear model, for example, it is generally the most suitable explanation. Conversely, an explanation that overly simplifies the model might offer an approximation of the rule used by the model that will be more easily grasped by the user –a more *understandable* explanation– but this approximation might ultimately mislead the user if it is not *faithful*. That is to say, just because a human agrees with the evidence pointed out by an explanation does not necessarily mean that it reflects how the model works.

Overall, this means that there is a trade-off between the amount of information provided by an explanation and its comprehensibility to humans. The most useful explanations lie somewhere in the middle of this trade-off.

The usefulness metric We describe a new human-centered measure that incorporates this trade-off into a single *usefulness* measure by empirically evaluating the ability of human participants to learn to “predict the predictor”, i.e., to be an accurate Meta-predictor. Indeed, if an explanation allows users to infer precise rules for the functioning of the predictor on past data, the correct application of these same rules should allow the user to correctly anticipate the model’s decisions on future data. Scrutable but inaccurate explanations will result in an inaccurate Meta-predictor – just like accurate inscrutable ones. This Meta-predictor framework avoids current pitfalls such as confirmation bias – just because a user likes the explanation does not mean they will be a better Meta-predictor – or prediction leakage on the explanation – in simulatability experiments, as the explanation is available during the test phase, any explanation that leaks the prediction would have a perfect score, without giving us any additional information about the model. We will now formally describe the metric build using this framework.

We assume a dataset $\mathcal{D} = \{(\mathbf{x}_i, \mathbf{f}(\mathbf{x}_i), \Phi(\mathbf{f}, \mathbf{x}_i)\}_{i=1}^K$ used to train human participants to learn to predict a classifier's output \mathbf{f} from K samples made of an input image \mathbf{x}_i , the associated predictions $\mathbf{f}(\mathbf{x}_i)$ and explanations $\Phi(\mathbf{f}, \mathbf{x}_i)$. We denote $\psi^{(K)}$ a human Meta-predictor after being trained on the dataset \mathcal{D} (see Fig. 2) using explanations. In addition, let $\psi^{(0)}$ be the human Meta-predictor after participants were trained on the same dataset but without explanations to offer baseline accuracy scores. We can now define the usefulness of an explainability method Φ after training participants on K samples through the accuracy score of the Meta-predictor normalized by the baseline Meta-predictor accuracy:

$$\text{Utility-}K = \frac{\mathbb{P}(\psi^{(K)}(\mathbf{x}) = \mathbf{f}(\mathbf{x}))}{\mathbb{P}(\psi^{(0)}(\mathbf{x}) = \mathbf{f}(\mathbf{x}))} \quad (1)$$

with $\mathbb{P}(\cdot)$ the probability over a test set. Thus, *Utility- K* score measures the improvement in accuracy that the explanation has brought. It is important to emphasize that this *Utility* measure only depends on the classifier prediction and not on the ground-truth label as recommended by [63]. After fixing the number of training samples K , we compare the normalized accuracy of different Meta-predictor. The Meta-predictor with the highest score is then the one whose explanations were the most useful as measures compared to a no-explanation baseline.

Utility metric In practice, we propose to vary the number of observations $K \in \{K_0, \dots, K_n\}$ and to consider an aggregated *Utility* score by computing the area under the curve (AUC) of the *Utility- K* . The higher the AUC the better the corresponding explanation method is. Formally, given a curve represented by a set of n points $\mathcal{C} = \{(K_0, \text{Utility-}K_0), \dots, (K_n, \text{Utility-}K_n)\}$ where $K_{i-1} < K_i$ we define our main metric $\text{Utility} = \text{AUC}(\mathcal{C})$.

4 Experimental design

We first describe how participants were enrolled in the study, then our general experimental design (See SI for more informations).

Participants Behavioral accuracy data were gathered from $n = 1,150$ participants using the Amazon Mechanical Turk (AMT) (www.mturk.com). All participants provided informed consent electronically and were compensated \$1.4 for their time ($\sim 5 - 8$ min). The protocol was approved by the University IRB and was carried out in accordance with the provisions of the World Medical Association Declaration of Helsinki (See SI for details). For each of the three tested datasets, we ensured that there was a sufficient number of participants after filtering out uncooperative participants ($n = 240$ participants, 30 per condition, 8 conditions) to guarantee sufficient statistical power. See SI for details.

General study design It included 3 conditions: an experimental condition where an explanation is provided to human participants during their training phase (see Fig. 2), a baseline condition where no explanation was provided to the human participants, and a control condition where a bottom-up saliency map [64] was provided as a non-informative explanation. This last control is critical, and indeed lacking from previous work [5, 59], because it provides a control for the possibility that providing explanations along with training images simply increases participants' engagement in the task. As we will show in Sec. 5, such non-informative explanations actually led to a decrease in participants' ability to predict the classifier's decisions – suggesting that giving a wrong explanation is worse than giving no explanations at all.

Each participant was only tested on a single condition to avoid possible experimental confounds. The main experiment was divided into 3 training sessions (with 5 training samples in each) each followed by a brief test. In each individual trial, an image was presented with the associated prediction of the model, either alone for the baseline condition or together with an explanation for the experimental and control condition. After a brief training phase (5 samples), participants' ability to predict the classifier's output was evaluated on 7 new samples during a test phase. During the test phase, no explanation was provided to limit confounding effects: one possible effect is if the explanation leaks information about the class label.* We also propose to use a reservoir that subjects can refer to during the testing phase to minimize memory load as a confounding factor which was reported in [59] (see SI for an illustration).

*Imagine an attribution method that would solely encode the classifiers' prediction. Participants would be able to guess the classifier's prediction perfectly from the explanation but the explanation per se would not help participants understand how the classifier works.

| Method | Husky vs. Wolf | | | | Leaves | | | | ImageNet | | | |
|------------------|----------------|------|------|-------------|---------|------|------|-------------|----------|------|------|------|
| | Session n° | 1 | 2 | 3 | Utility | 1 | 2 | 3 | Utility | 1 | 2 | 3 |
| Baseline | 55.7 | 66.2 | 62.9 | | 70.1 | 76.8 | 78.6 | | 58.8 | 62.2 | 58.8 | |
| Control | 53.3 | 61.0 | 61.4 | 0.95 | 72.0 | 78.0 | 80.2 | 1.02 | 60.7 | 59.2 | 48.5 | 0.94 |
| Saliency [15] | 53.9 | 69.6 | 73.3 | 1.06 | 83.2 | 88.7 | 82.4 | 1.13 | 61.7 | 60.2 | 58.2 | 1.00 |
| Integ.-Grad. [6] | 67.4 | 72.8 | 73.2 | 1.15 | 82.5 | 82.5 | 85.3 | 1.11 | 59.4 | 58.3 | 58.3 | 0.98 |
| SmoothGrad [7] | 68.7 | 75.3 | 78.0 | 1.20 | 83.0 | 85.7 | 86.3 | 1.13 | 50.3 | 55.0 | 61.4 | 0.93 |
| GradCAM [9] | 77.6 | 85.7 | 84.1 | 1.34 | 81.9 | 83.5 | 82.4 | 1.10 | 54.4 | 52.5 | 54.1 | 0.90 |
| Occlusion [16] | 71.0 | 75.7 | 78.1 | 1.22 | 78.8 | 86.1 | 82.9 | 1.10 | 51.0 | 60.2 | 55.1 | 0.92 |
| Grad.-Input [65] | 65.8 | 63.3 | 67.9 | 1.06 | 76.5 | 82.9 | 79.5 | 1.05 | 50.0 | 57.6 | 62.6 | 0.95 |

Table 1: **Utility-K and Utility scores.** Utility-*K*scores across the 3 sessions for each attribution method, for each of the 3 datasets considered, followed by the *Utility* scores. Higher is better. The *Utility* scores of attribution methods that are statistically significant are **bolded**.

Datasets and models We performed three distinct experiments in total – using a variety of neural network architectures and 6 representative attributions methods. Each of these experiments aimed at testing the usefulness of the explanation in a different context.

Our first scenario focuses on the detection of biases in AI systems using the popular Wolf vs. Husky dataset from [5] where an evaluation measure was already proposed around the usefulness of explanations for humans to detect biases. This makes it a good control experiment to measure the effectiveness of the framework proposed in Sec. 3. For this first experiment, we used the same model as in the original paper: InceptionV1 [66], and a similar dataset of Husky and Wolf images to bias the model. In this situation where prior knowledge of subjects can affect their Meta-predictor score, we balance data correctness (50% of correct/incorrect examples shown). Therefore, a subject relying only on their prior knowledge will end up as a bad Meta-predictor of the model. For this experiment, the results come from $n = 242$ subjects who all passed our screening process.

In our second scenario, we focus on a representative challenging image categorization task which would be hard to solve by a non-expert untrained human participant and the goal is for the end-user to understand the strategy that was discovered by the AI system. Here, we chose the leaf dataset described in [46]. We selected 2 classes from this dataset (Betulaceae and Celastraceae) that could not be classified by shape to reduce the chances that participants will discover the solution on their own – forcing them instead to rely on non-trivial features highlighted by the explanations (veins, leaf margin, etc). This scenario is far from being artificial as it reflects a genuine problem for the paleobotanist [47]. Can explainability methods help nonspecialists discover the strategies discovered by an AI system? As participants are lay people from Amazon Mechanical Turk we do not expect them to be experts in botany, therefore we did not explicitly try to control for prior knowledge. In this experiment, $n = 240$ subjects passed all our screening and other filtering processes.

Finally, our last scenario focuses on identifying cases where an AI system fails using ImageNet [48], also used in previous explainability work [11, 22, 28, 49, 57, 67]. We used this dataset because we expect it to be representative of real-world scenarios where it is difficult to understand what the model relies on for classification which makes it very difficult to understand these failure cases. Moreover, previous work has pointed out that attribution methods are not useful on this dataset [57], we have thus chosen to extend our analysis to this particular case. We use a ResNet50 [68] pretrained on this dataset as predictor. Because prior knowledge is a major confounding factor on ImageNet, we select a pair of classes that was heavily miss-classified by the model, to be able to show subjects 50% of correct/incorrect predictions: the pair Kit Fox and Red Fox fits this requirement. In this experiment, we analyzed data from $n = 241$ participants who passed our screening and filtering processes.

For all experiments, we compared 6 representative attribution methods: Saliency (SA) [15], Gradient ⊙ Input (GI) [17], Integrated Gradients (IG) [6], Occlusion (OC) [16], SmoothGrad (SG) [7] and Grad-CAM (GC) [9]. Further information on these methods can be found in SI. Table 1 summarizes all the results from our psychophysics experiments.

5 Results

Scenario (1): Bias detection Fig. 3 shows the *Utility-K* scores for each method after different numbers of training samples were used to train participants for the biased dataset of Husky vs. Wolf. The *Utility* score encodes the quality of the explanations provided by a method, the higher the score,

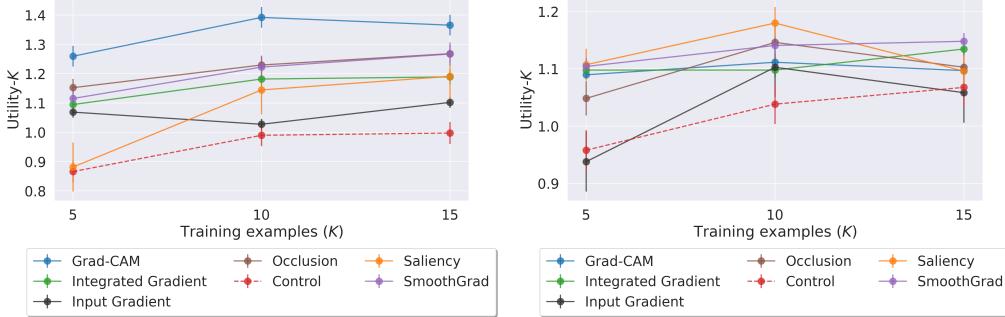


Figure 3: **Utility- K for both Husky vs. Wolf (left) and the Leaves (right) dataset.** The $Utility-K$ of the explanation, or the accuracy of the human Meta-predictor after training, is measured after each training session (3 in total) for the scenario (1) of bias detection (on the left) and the scenario (2) concerning the identification of new strategies. Concerning the first scenario, all methods have a positive effect on the score obtained - they improve the subjects' ability to predict the model - and are thus useful to understand the model. Grad-CAM, Occlusion and SmoothGrad are particularly useful for bias detection. On the Leaves dataset [46], explanations are also useful, but specifically Saliency, SmoothGrad and Integrated Gradients.

the better the method, with the baseline score being 1 (every score is divided by the baseline score corresponding to human accuracy after training without explanations).

A first observation is that the explanations have a positive effect on the $Utility-K$ score: the explanation allows participants to better predict the model's decision (as the $Utility$ scores are above 1). These results are consistent with those reported in [5]. This is confirmed with an Analysis of Variance (ANOVA) for which we found a significant main effect, with a medium effect size ($F(7, 234) = 9.19, p < .001, \eta^2 = 0.082$). Moreover, the only score below the baseline is that of the control explanation, which do not make use of the model. We further explore our results by performing pairwise comparisons using Tukey's Honestly Significant Difference [69] to compare the different explanations against the baseline. We found 3 explainability methods to be significantly better than the baseline: Grad-CAM($p < 0.001$), Occlusion($p = 0.01$) and SmoothGrad($p = 0.034$). Thus, participants who received the Grad-CAM, Occlusion or SmoothGrad explanations performed much better than those who did not receive them.

Scenario (2): Identifying an expert strategy In Fig. 3, we show results on the Leaves dataset. An ANOVA analysis across all conditions revealed a significant main effect, albeit small ($F(7, 232) = 3.88, p < .001, \eta^2 = 0.037$). This implies that explanation also had a positive effect resulting in better Meta-predictor in this use case. A Tukey's Honestly Significant Difference test suggests that the best explanations are Saliency, SmoothGrad and Integrated Gradients as they are the only ones to be significantly better than our baseline (WE) ($p = .004, p = .007$ and $p = .03$ respectively). An interesting result is that SmoothGrad seems to be consistently useful across both use cases where explanations are indeed practically useful. A more surprising result is that Saliency which was one of the worst explanations for bias detection, is now the best explanation on this use case.

Scenario (3): Understanding failure cases Table 1 shows that, on the ImageNet dataset, none of the methods tested exceeded baseline accuracy. Indeed, the experiment carried out, even with an improved experimental design, led us to the same conclusion as previous works [57]: none of the tested attribution methods are useful (ANOVA: $F(7, 233) = 1.26, p > .05$). In the use case of understanding failure cases on ImageNet, **no attributions methods seem to be useful**.

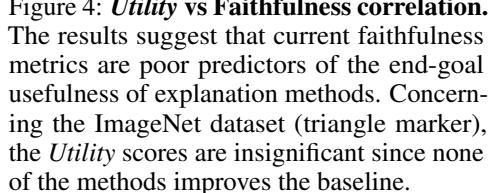


Figure 4: **Utility vs Faithfulness correlation.** The results suggest that current faithfulness metrics are poor predictors of the end-goal usefulness of explanation methods. Concerning the ImageNet dataset (triangle marker), the $Utility$ scores are insignificant since none of the methods improves the baseline.

Why do attribution methods fail?

After studying the usefulness of attribution methods across 3 real-world scenarios for explainable AI we found that attribution methods help in some cases but not others. To try to better understand why this is the case, we explore 3 possible explanations to gain insights as to why sometimes attribution methods fail to help.

Faithfulness as a proxy for Utility? Faithfulness is often described as one of the key desiderata for a good explanation [21, 70, 71]. If an explanation fails to be sufficiently faithful, the rules it highlights won't allow a user to understand the inner-working of the model. Thus, a lack of faithfulness on ImageNet could explain our results. To test this hypothesis, we use the *faithfulness* metrics: Deletion[8, 26], commonly used to compare attribution methods [8, 11, 22, 23, 26, 27]. A low Deletion score indicates a good *faithfulness*, thus for ease of reading we report the *faithfulness* score as 1 – Deletion such that a higher *faithfulness* score is better.

Fig 4 shows the linear relationship between our Utility metrics and the faithfulness scores computed for every attribution method across all 3 datasets. We observe two main trends: 1) **There does not appear to be any specific pattern regarding faithfulness that could explain why attribution methods are not useful for ImageNet**, and 2) the least useful attribution methods for both use cases for which methods help (Bias and Leaves) are some of the leading methods in the field measured by the faithfulness metric. We also found a weak, if maybe anti-correlated, relation between faithfulness and usefulness: just focusing on making attribution methods more faithful does not translate to having methods with higher practical usefulness for end-users. And, in fact, focusing too heavily on faithfulness seem to come at the expense of usefulness, resulting in explanations that are counter-intuitively less useful. This second observation may seems rather alarming for the field given that the faithfulness measure is one of the main benchmarks.

Are explanations too complex? Using the trade-off between completeness and understandability previously discussed in section 3, we formulate another plausible hypothesis: some explanations may be faithful but too complex and therefore not understood by humans. In that view, an explanation with low complexity would tend to be more useful.

As a simple measure of the complexity of visual explanations, it would be ideal to be able to compute the Kolmogorov complexity [72] of each explanation. It was shown in previous work [73] to correlate well with human-derived ratings for the complexity of natural images [74, 75]. As suggested by [72, 76] we used a standard compression technique (JPEG) to approximate the Kolmogorov complexity. Fig. 5 shows the *Utility* vs complexity score of attribution methods for each dataset. For one of the datasets where attribution methods help the results suggest the presence of a strong correlation between *usefulness* and *complexity*: the least complex method is the most useful to end-users. For the other datasets, the results are either not conclusive (Leaves), or are not relevant as methods are not useful (ImageNet). Furthermore, **across datasets there is no significant difference in the complexity of explanations** that can explain why attribution methods do not help on ImageNet. This could be because the Kolmogorov Complexity does not perfectly reflect human visual complexity, or because this is not the key element to explain failure cases of attribution methods.

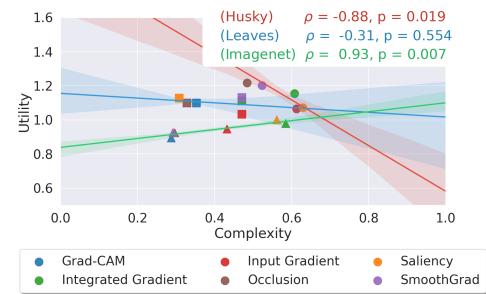


Figure 5: **Utility vs Complexity correlation.** This suggest a weak but possibly existing link between *Complexity* and *Utility* scores.

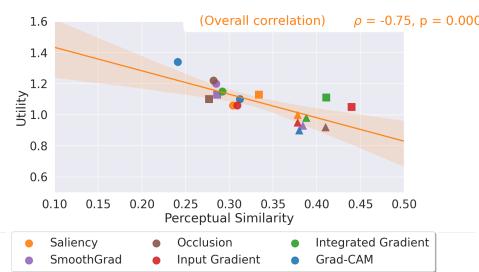


Figure 6: **Perceptual Similarity scores vs. Utility.** The perceptual similarity of highlighted regions by a given attribution method for both classes is measured, for each method, for each dataset. We observe a strong correlation between the perceptual similarity of features highlighted for both classes and the practical usefulness of methods.

8

An intrinsic limitation of Attribution methods? The role of attribution methods is to help identify “where” to look in an image to understand the basis for a system’s prediction. However, attribution methods do not tell us “what” in those image regions is driving decisions. Understanding the what information is thus left open for the end-user to infer. For visually very similar classes (such as discriminating between different breeds of dogs) and fine-grained recognition more generally, understanding what precise features are being used by the system might be very challenging for humans simply by looking at those diagnostic image regions (knowing that the system is looking at the ear regions tells us nothing about what in those ear regions is driving decisions). Our main hypothesis is that such a lack of clear “what” understanding is precisely what is driving the failure of attribution methods on the ImageNet dataset. To test this hypothesis we estimated the perceptual similarity between highlighted regions of both classes, across methods, and across datasets (more results in SI). If the perceptual similarity is high for two distinct classifications we expect attribution to not be useful. To measure perceptual similarity we use the LPIPS metric [77] as it has been shown to provide a good proxy for human perceptual similarity judgments [77, 78]. We report the perceptual similarity score as $1 - \text{LPIPS}$ score so that a high score means a high similarity. Fig. 6 shows the correlation between the perceptual similarity scores vs. our *Utility* scores on all methods and datasets studied. Our results suggest a strong correlation between perceptual similarity and practical usefulness: the more perceptually similar discriminative features of both classes are, the less useful the methods become. More importantly, the results across datasets show that on ImageNet, where attribution methods do not help, every method has a high similarity score. This result suggests that after a certain threshold of perceptual similarity, attribution methods might no longer be useful, no matter how faithful or low in complexity the explanation is. Overall, the results suggest that the perceptual similarity of discriminative features could explain why attribution methods fail on ImageNet.

6 Discussion

In summary, we conducted a large-scale human psychophysics experiment to test the utility of explainability methods in real-world scenarios. Our work shows that in two of the three tested scenarios (bias detection and identification of new strategies), explainability has indeed progressed and provided meaningful assistance to human end-users. Nevertheless, we identified a scenario (understanding failure case) for which none of the tested attribution methods were useful. This result is consistent with previous work [57] and highlights a fundamental challenge for XAI.

Further analysis of associated fidelity performance metrics used to guide the development of explainability methods revealed that they did not correlate with our empirical measure of utility – suggesting that they fail to predict such failure cases. We also investigated the possibility that the complexity of individual explanations may play a role in explaining human failures to learn to leverage those explanations to understand the model and, while we found a weak correlation between complexity and our empirical measure of utility, this correlation appears too low to explain these failure cases.

Finally, we considered the possibility that fully understanding the inner-workings of a vision system requires understanding, not just “where” to look (as revealed by attribution maps) but also “what” features in those locations drive decisions, something not currently revealed by these methods. Our assumption is that the need for finer “what” information should arise when diagnostic image locations across classes look perceptually very similar and potentially semantically related for certain classification problems (e.g., looking at the ears or the snout to discriminate between breeds of cats and dogs) and one needs to identify what aspects of these features are driving decisions. We computed a perceptual score for classification problems by estimating the perceptual similarity between diagnostic image regions (as predicted by attribution methods) and found that, indeed, when this score predicts a certain level of perceptual similarity between two distinct classes, attribution methods fail to contribute useful information to human end-users, regardless of the fidelity or complexity of the explanations. This fundamental result highlights an intrinsic limitation of attribution methods and suggests the need for a change of focus in the field – from quantitative improvements of current attribution methods to the development of novel explainability methods that communicate qualitatively different knowledge about the inner-workings of the system.

The cost of evaluating additional methods using our benchmark is relatively modest (\$50 per test scenario). By making all data and software available, we hope to help researchers evaluate future explainability methods and assess overall progress towards the development of more human-interpretable AI systems.

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A Human experiments

A.1 Experimental design

Figure 7 summarizes the experimental design used for our experiments. The participants that went through our experiments are users from the online platform Amazon Mechanical Turk (AMT). Through this platform, users stay anonymous, hence, we do not collect any sensitive personal information about them. We prioritized users with a Master qualification (which is a qualification attributed by AMT to users who have proven to be of excellent quality) or normal users with high qualifications (number of HIT completed = 10000 and HIT accepted > 98%).

Before going through the experiment, participants are asked to read and agree to a consent form, which specifies: the objective and procedure of the experiment, as well as the time expected to completion ($\sim 5 - 8$ min) with the reward associated (\$1.4), and finally, the risk, benefits, and confidentiality of taking part in this study. There are no anticipated risks and no direct benefits for the participants taking part in this study.

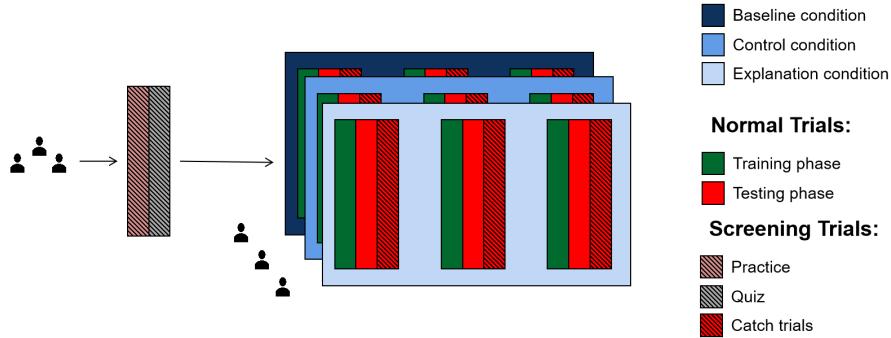


Figure 7: **Experimental design.** First, every participant goes through a practice session (fig 8) to make sure they understand how to use attribution methods to infer the rules used by a model, and a quiz (fig 9) to make sure they actually read and understand the instructions. Then, participants are split into the different conditions – every participant will only go through one condition. The 3 possible conditions are: an Explanation condition where an explanation is provided to human participants during their training phase, a Baseline condition where no explanation was provided to the human participants, and a Control condition where a non-informative explanation was provided. The main experiment was divided into 3 training sessions each followed by a brief test. In each individual training trial, an image was presented with the associated prediction of the model, either alone for the baseline condition or together with an explanation for the experimental and control condition. After a brief training phase (5 samples), participants' ability to predict the classifier's output was evaluated on 7 new samples (only the image, no explanation) during a test phase. To filter out uncooperative participants we also add a catch trial (fig 10) in each test session.

Controlling for prior class knowledge To control for users' own semantic knowledge, we balanced the samples shown to participants so that the classifiers were correct/incorrect 50% of the time. This way, the baseline (participants who try to simply predict the true class label of an image as opposed to learning to predict the model's outputs) is at 50%. Any higher score reflects a certain understanding of the rules used by the model.

A.2 Pruning out uncooperative participants

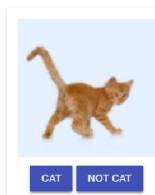
3-stage screening process. To prune out uncooperative participants, we subjected them to a 3-stage screening process. First, participants completed a short practice session to make sure they understood the task and how to use the attribution methods to infer the rules used by the model (fig 8). Second, as done in [79], we asked participants to answer a few questions regarding the instructions provided to make sure they actually read and understood them (fig 9). Third, during the main experiment, we took advantage of the reservoir to introduce a catch trial (fig 10). The reservoir is the place where we store the training example of the current session, which can be accessed during the testing phase. We added a trial in the testing phase of each session where the input image

Practice

The Artificial Intelligence (AI) focuses on certain aspects of the image to judge whether an animal, for example a cat, is present or not in the image. In this example, we use an AI trained to recognize the presence of a cat in the image. You have 4 examples of images that the AI has classified as either cat or no cat.



Next to the image is the explanation, which explains how the AI decides if there is a cat in the image. It takes the form of a heatmap. The red pixels are the ones the AI looked at the most when it decided there was a cat in the image and the blue pixels are the ones it looked at the least. To check if you understand the AI's behavior, predict whether the AI classifies the following images as cat or not cat.



CHECK

Figure 8: **Practice session.** Through a practice session, which is a simplified version of the main experiment, we evaluate if users understand how to read and use explanations. Participants that failed to predict correctly any of the 5 cat test images on the first try were excluded from further analysis.

corresponded to one of the training samples used in the current session: since the answer is still on the screen (or a scroll away) we expect participants to be correct on these catch trials. Participants that failed any of the 3 screening processes were excluded from further analysis.

Practice session is now complete.

You will now begin the main experiment.

All the images of Red fox and Kit fox that will be shown in this experiment were also shown to an AI that was trained to recognize Red fox and Kit fox in pictures.

Unfortunately, our AI is not perfect, it is not always able to predict the right animal in the picture. For every picture, the AI was asked to guess whether there was a Red fox or a Kit fox in the picture. In total, it predicted Red fox half the time and the other half it predicted Kit fox.

We hope you can help us understand the behavior of the AI.

Your task will first be to study examples of model classification, to try to understand the AI's behavior: what makes it predict Red fox vs. Kit fox. Then you will make predictions on new images, which means you will have to put yourself in the shoes of the AI based on the behavior you have captured.

Concerning these sessions, there will be 3 of them, each one composed of a training phase (viewing 5 images, their predictions and an explanation if you are in the condition with explanation) and a testing phase (on 7 images, predict the AI's prediction).

During the testing phase, you will have access to the examples you studied previously (only the last 5 so as not to flood your screen with too many images).

To make sure instructions were clear, please answer the following question, thank you.

Does the AI always make the correct prediction ?

- Yes
 No

Will you have access to the training examples as references while making prediction on new test images ?

- Yes
 No

How many block (learning + testing) is there in the experiment?

- 1
 2
 3

Figure 9: **Quiz.** Through a quiz, we make sure that users read and understood the instructions. Participants that did not answer correctly every question on the first try were excluded from further analysis.

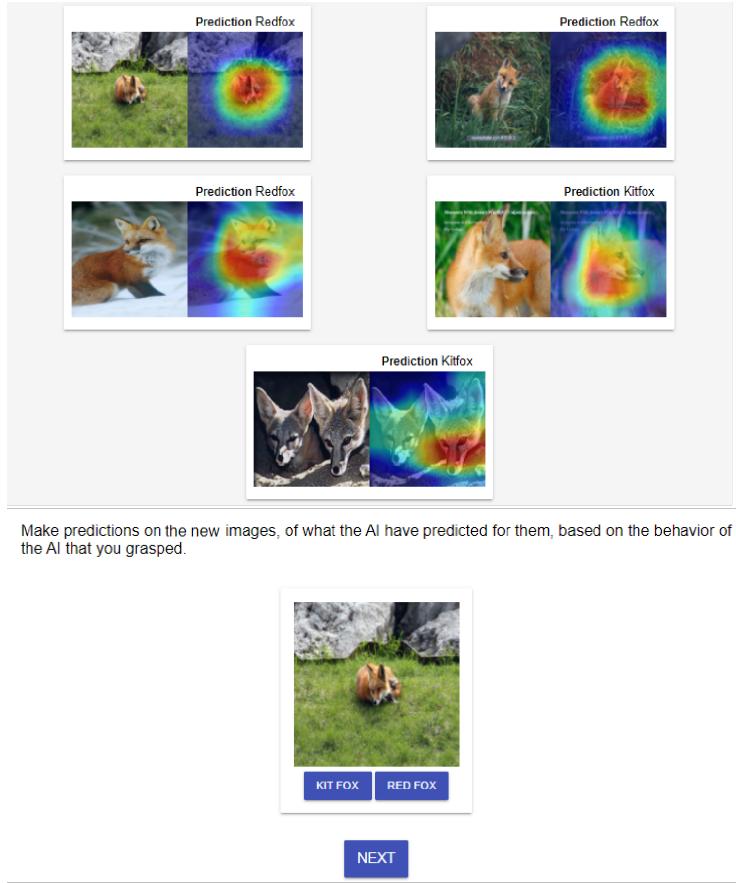


Figure 10: Catch trial. We use a reservoir (to store all the examples of the current training session) that participants can refer to during the testing phase to minimize memory load. At the top of the screen is the reservoir, at the bottom of the screen is a trial from the testing phase. We take advantage of the reservoir to introduce a catch trial. We added a trial in the testing phase of each session where the input image corresponded to one of the training samples used in the current session: since the answer is still on the screen (or a scroll away) we expect participants to be correct on these catch trials. Participants that failed any of the 3 catch trials (one per session) were excluded from further analysis.

B Why do attribution methods fail?

B.1 Faithfulness

While the Deletion[8] measure is the most commonly used faithfulness metric, for completeness we also consider 2 others faithfulness metric available in the Xplique library[80]: Insertion[8] and μ Fidelity[70]. Fig 11 shows the correlation between either measure and our *Utility*. We find them to be no better predictor of the practical usefulness of attribution methods than the Deletion measure.

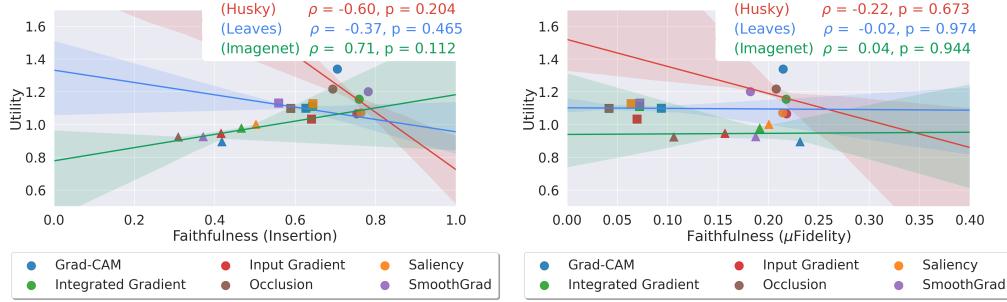


Figure 11: **Utility vs Insertion correlation & Utility vs μ Fidelity correlation** The results suggest that every faithfulness metrics tested are poor predictors of the practical usefulness of attribution methods. Concerning the ImageNet dataset (triangle marker), the *Utility* scores are insignificant since none of the methods improves the baseline.

B.2 Perceptual Similarity

| Method | Husky vs. Wolf | Leaves | ImageNet |
|------------------|----------------|--------------|--------------|
| Saliency [15] | 0.304 | 0.334 | 0.378 |
| Integ.-Grad. [6] | 0.292 | 0.411 | 0.388 |
| SmoothGrad [7] | 0.285 | 0.286 | 0.384 |
| GradCAM [9] | 0.241 | 0.312 | 0.38 |
| Occlusion [16] | 0.282 | 0.277 | 0.41 |
| Grad.-Input [65] | 0.309 | 0.44 | 0.378 |

Table 2: **Perceptual Similarity scores.** The perceptual similarity of highlighted regions by a given attribution method for both classes is measured, for each method, for each dataset. The perceptual similarity scores that are higher than 0.378 (the minimum score on ImageNet) are **bolded**. Higher is more similar.

Tab 2 shows the Perceptual Similarity scores obtained for each method, on every dataset. We observe that on ImageNet, where attribution methods do not help, the perceptual similarity scores are clearly higher than on the two other datasets, where attribution methods help.

Fig 12 shows examples of patches for each dataset using Grad-CAM.

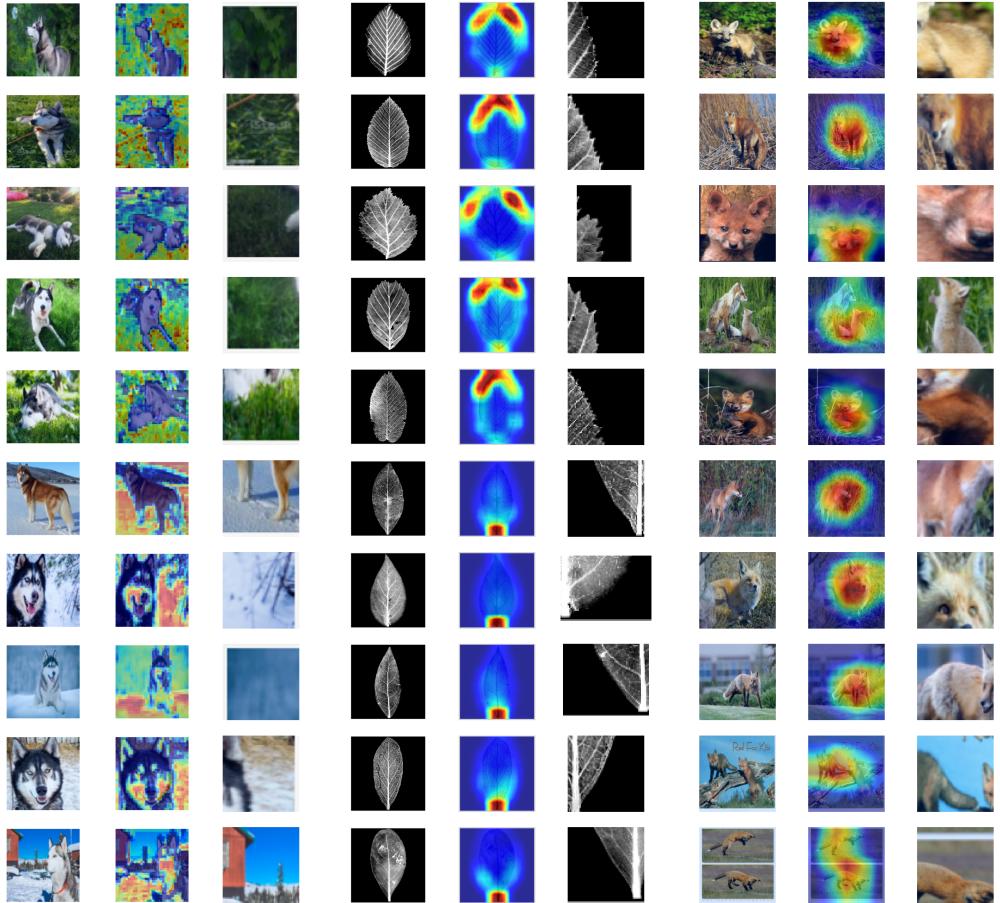


Figure 12: **Examples of extracted patches.** The perceptual similarity score is performed on the locations considered most important by the attribution methods. Examples of patches extracted for the three datasets with the Grad-CAM method.

C Attribution methods

C.1 Methods

In the following section, the formulation of the different methods used in the experiment is given. We define $f(\mathbf{x})$ the logit score (before softmax) for the class of interest. An explanation method provides an attribution score for each input variables. Each value then corresponds to the importance of this feature for the model results.

Saliency [15] is a visualization technique based on the gradient of a class score relative to the input, indicating in an infinitesimal neighborhood, which pixels must be modified to most affect the score of the class of interest.

$$\Phi^{SA}(\mathbf{x}) = \|\nabla_{\mathbf{x}} f(\mathbf{x})\|$$

Gradient ⊙ Input [65] is based on the gradient of a class score relative to the input, element-wise with the input, it was introduced to improve the sharpness of the attribution maps. A theoretical analysis conducted by [17] showed that Gradient ⊙ Input is equivalent to ϵ -LRP and DeepLIFT [81] methods under certain conditions: using a baseline of zero, and with all biases to zero.

$$\Phi^{GI}(\mathbf{x}) = \mathbf{x} \odot \|\nabla_{\mathbf{x}} f(\mathbf{x})\|$$

Integrated Gradients [6] consists of summing the gradient values along the path from a baseline state to the current value. The baseline is defined by the user and often chosen to be zero. This integral can be approximated with a set of m points at regular intervals between the baseline and the point of interest. In order to approximate from a finite number of steps, we use a Trapezoidal rule and not a left-Riemann summation, which allows for more accurate results and improved performance (see [82] for a comparison). The final result depends on both the choice of the baseline \mathbf{x}_0 and the number of points to estimate the integral. In the context of these experiments, we use zero as the baseline and $m = 80$.

$$\Phi^{IG}(\mathbf{x}) = (\mathbf{x} - \mathbf{x}_0) \int_0^1 \nabla_{\mathbf{x}} f(\mathbf{x}_0 + \alpha(\mathbf{x} - \mathbf{x}_0)) d\alpha$$

SmoothGrad [7] is also a gradient-based explanation method, which, as the name suggests, averages the gradient at several points corresponding to small perturbations (drawn i.i.d from a normal distribution of standard deviation σ) around the point of interest. The smoothing effect induced by the average helps reduce the visual noise and hence improve the explanations. In practice, Smoothgrad is obtained by averaging after sampling m points. In the context of these experiments, we took $m = 80$ and $\sigma = 0.2$ as suggested in the original paper.

$$\Phi^{SG}(\mathbf{x}) = \mathbb{E}_{\varepsilon \sim \mathcal{N}(0, I\sigma)} (\nabla_{\mathbf{x}} f(\mathbf{x} + \varepsilon))$$

Grad-CAM [9] can be used on Convolutional Neural Network (CNN), it uses the gradient and the feature maps \mathbf{A}^k of the last convolution layer. More precisely, to obtain the localization map for a class, we need to compute the weights α_c^k associated to each of the feature map activation \mathbf{A}^k , with k the number of filters and Z the number of features in each feature map, with $\alpha_k^c = \frac{1}{Z} \sum_i \sum_j \frac{\partial f(\mathbf{x})}{\partial \mathbf{A}_{ij}^k}$ and

$$\Phi^{GC} = \max(0, \sum_k \alpha_k^c \mathbf{A}^k)$$

Notice that the size of the explanation depends on the size (width, height) of the last feature map, a bilinear interpolation is performed in order to find the same dimensions as the input.

Occlusion [16] is a sensitivity method that sweeps a patch that occludes pixels over the images, and uses the variations of the model prediction to deduce critical areas. In the context of these experiments, we took a patch size and a patch stride of of 1 tenth of the image size.

$$\Phi_i^{OC} = f(\mathbf{x}) - f(\mathbf{x}_{[\mathbf{x}_i=0]})$$

C.2 Examples of explanations

Examples of explanations from the different attributions methods evaluated through our experiments on the Husky vs. Wolf dataset (fig 13), the Leaves dataset (fig 14) and the ImageNet dataset (fig 15).

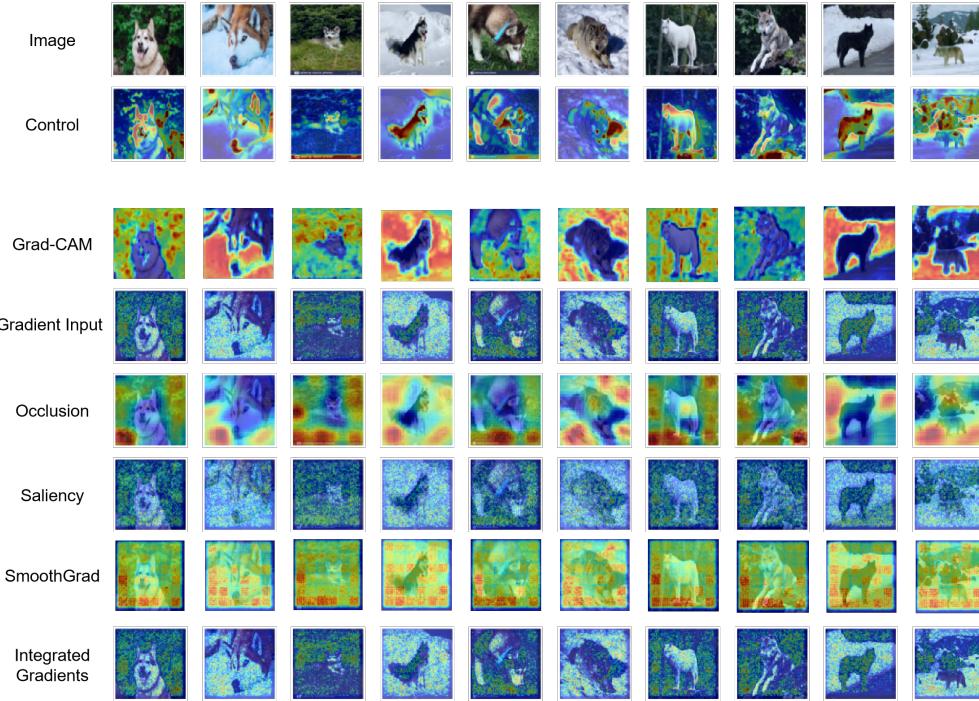


Figure 13: Examples of images from the Wolf vs. Husky experiment, alongside their respective: Control explanation (which is a non-informative explanation) as well as the different Attribution methods evaluated in our experiment.

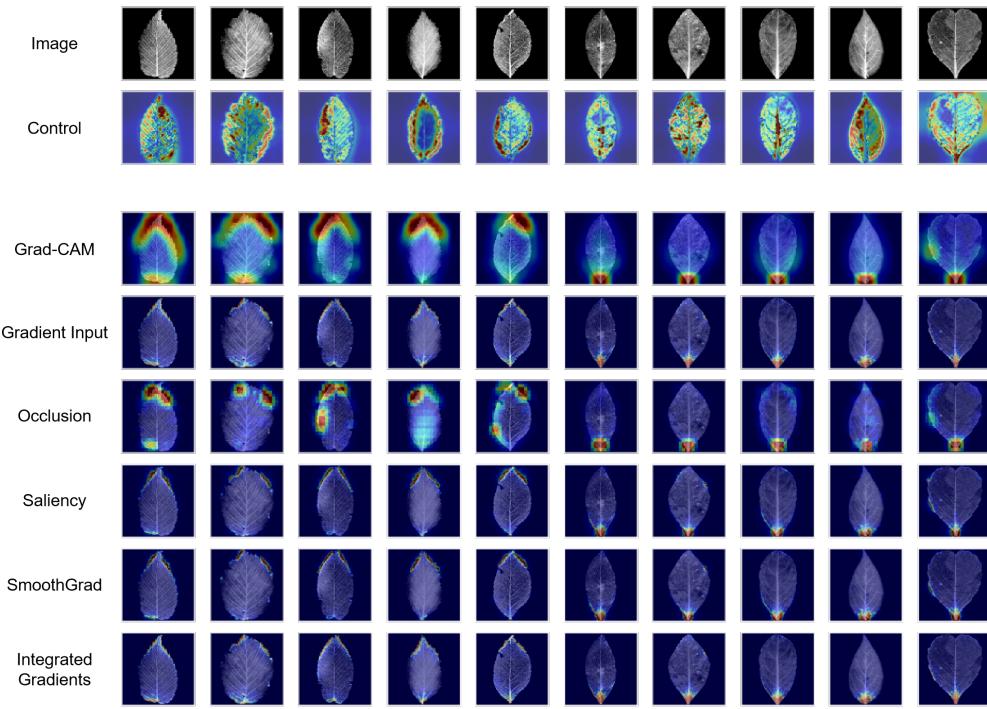


Figure 14: Examples of images from the Leaves experiment, alongside their respective: Control explanation (which is a non-informative explanation) as well as the different Attribution methods evaluated in our experiment.

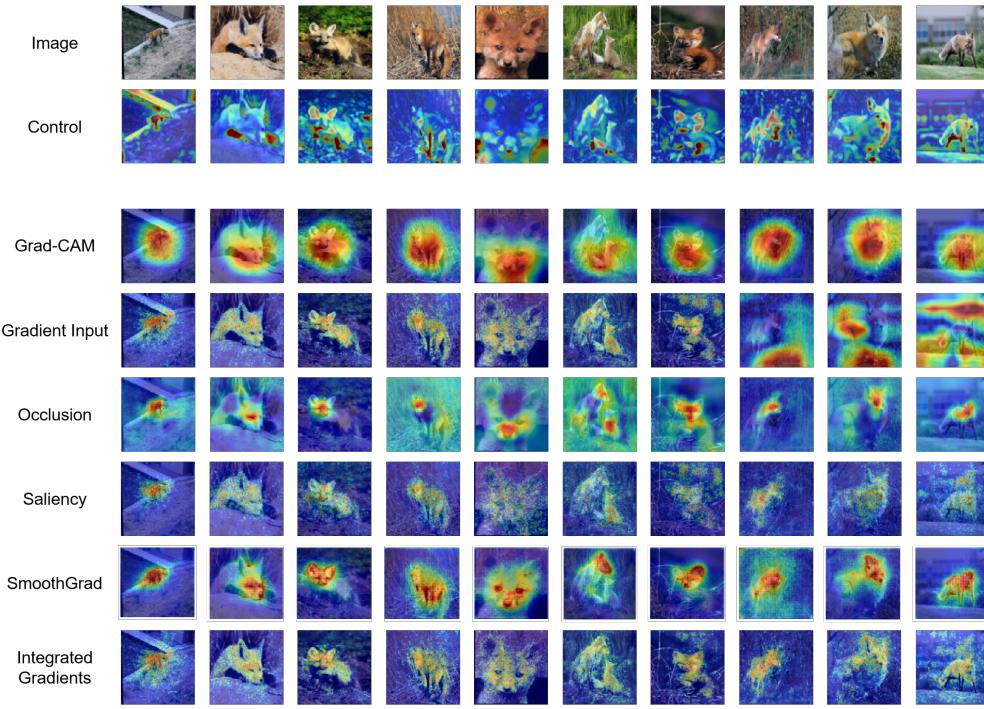


Figure 15: Examples of images from the ImageNet experiment, alongside their respective: Control explanation (which is a non-informative explanation) as well as the different Attribution methods evaluated in our experiment.