ON THE SHELF LIFE OF FINETUNED LLM-JUDGES: FUTURE PROOFING, BACKWARD COMPATIBILITY, AND QUESTION GENERALIZATION

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ABSTRACT

The LLM-as-a-judge paradigm is widely used in both evaluating free-text model responses and reward modeling for model alignment and finetuning. Recently, finetuning judges with judge-specific data has emerged as an often preferred choice over directly prompting frontier models as judges, as the former achieves better performance with smaller model sizes while being more robust to common biases. However, the standard evaluation ignores several practical concerns of finetuned judges regarding their real world deployment. In this paper, we identify and formalize three aspects that affect the *shelf life* of these judges: *future proofing* and *backward compatibility* – how well judges finetuned on responses by today's generator models perform on responses by future models or past models, as well as *question generalization* – how well judges generalize to unseen questions at test time. We study these three aspects in the math domain under a unified framework with varying train and test distributions, three SFT- and DPO-based finetuning algorithms and three different base models. Experiments suggest that future-proofing is challenging for most models, while backward compatibility is relatively easy, with DPO-trained models consistently *improving* performance. We further find that continual learning provides a more balanced adaptation to shifts between older and newer response distributions than training solely on stronger or weaker responses. Moreover, all models observe certain degrees of performance degradation when moving from questions seen during training to unseen ones, showing that current judges do not fully generalize to unseen questions. These findings provide insights into practical considerations for developing and deploying judge models in the face of ever-changing generators.

1 INTRODUCTION

Automatic evaluators have become a central part of the large language model (LLM) development cycle. They serve both as reward models during training (Stiennon et al., 2020; Ouyang et al., 2022; Yuan et al., 2024) and as verifiers in inference-time compute scaling (Zhou et al., 2025; Kim et al., 2025; Singhi et al., 2025). In the LLM-as-judge paradigm, a generative language model evaluates the outputs of other models for a given input question, providing a scalable approach to automatic evaluation. Past work on LLM-as-judges began with zero-shot prompting of capable LLMs (Liu et al., 2023; Dubois et al., 2023). However, such judges have been shown to be prone to various biases, such as stylistic bias (Zeng et al., 2024; Raina et al., 2024), length bias (Zheng et al., 2023; Zeng et al., 2024), and positional bias (Wang et al., 2023; Pezeshkpour & Hruschka, 2024). As a result, recent efforts have finetuned specialized evaluators Li et al. (2024b); Kim et al. (2024a); Vu et al. (2024), which have been shown to be more robust to common forms of bias (Zhu et al., 2025; Wang et al., 2024a; Park et al., 2024a) while matching the performance of larger prompted models.

Although recent advances in judge model finetuning have largely focused on developing training methodology Chen et al. (2025a;c), little attention has been devoted to understanding how these models behave as a function of their training inputs. In this work, we investigate this gap by asking three key questions: First, can judge models trained on fixed datasets of input questions, model responses, and ground-truth verdicts accurately evaluate the responses of newer models, i.e., are judges *future proof*? Second, if we train a judge on up-to-date responses from newer models, can it

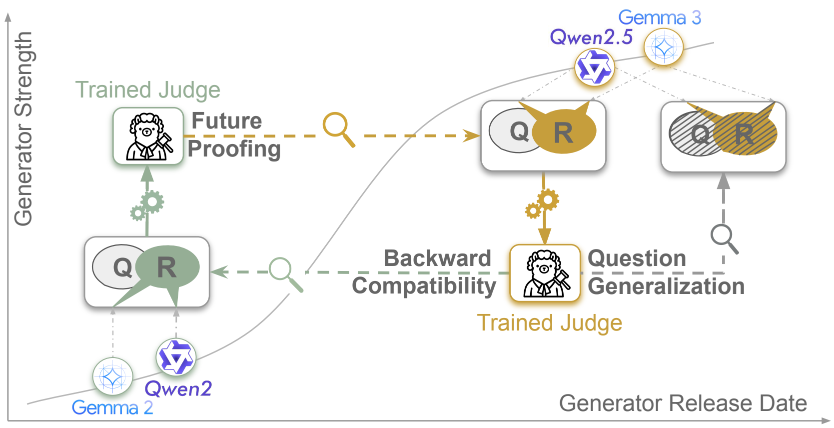


Figure 1: High-level overview of our setup for studying *Future Proofing*, *Backward Compatibility*, and *Question Generalization* through the lens of generalization and robustness to input distribution shifts. Q and R represent questions and responses, respectively, with responses generated by the shown generator models (Gemma2, Qwen2, Gemma3, Qwen2.5). *Future Proofing* evaluates how well judges trained on responses from weaker, older generators (green: Gemma2, Qwen2) assess responses from stronger, newer generators (yellow: Gemma3, Qwen2.5). *Backward Compatibility* examines the reverse direction. *Question Generalization* measures performance on in-distribution questions and corresponding responses that were both not included (dashed Q and R) in the training.

reliably evaluate responses from older models, i.e., is the trained judge *backward compatible*? Third, fixing the response generating models, how reliably can judges assess questions that differ from those seen during training, i.e., do they *generalize to new questions*? We examine these questions, as illustrated in Figure 1, through the lens of generalization and robustness, aiming to understand the *shelf life* of trained judges.

In this work, we propose a *dual-distribution* formulation of automatic evaluation. Concretely, we model the judge's input as comprising elements drawn from two distinct distributions: the *question distribution*, which characterizes the input questions to be evaluated, and the *response distribution*, which characterizes the responses to be judged. We study the performance of trained judges when responses are drawn from relatively weak and strong generators, henceforth referred to as weak responses and strong responses. We also examine how well trained judges evaluate questions that are (1) seen during training but paired with new responses, and (2) completely unseen during training. By focusing on weak and strong generators and novel questions, we gain insights into the shelf life of trained judge models through four practical questions:

- **Future-proofing.** Given a judge trained on responses from older ("weak") models, how accurately can it evaluate responses from newer ("strong") models? If the goal is to evaluate strong responses, how much benefit do practitioners gain by training on strong responses rather than weak ones?
- **Backward-compatibility.** Given a judge trained on responses from newer ("strong") models, can it reliably assess responses from older ("weak") models? If the goal is to evaluate weak responses, does training a judge on strong responses provide any benefit?
- **Continual learning.** Compared to judges trained only on weak or strong responses, how well does a continually trained judge adapt to distribution shifts between the two response distributions?
- **Question generalization.** Does judge performance depend on whether a question was seen during training? Even for seen questions, can a judge reliably assess new responses?

Using the mathematics domain, we set up a suite of controlled experiments to analyze the shelf life of judge models, training across three base models of varying sizes and capabilities and three popular judge-training recipes. Our findings reveal that fine-tuned judges struggle to evaluate newer, stronger model responses and therefore require training with up-to-date response distribution. Once trained on newer, stronger responses, judges exhibit some degree of backward compatibility. Continual training provides a more balanced adaptation to shifts between older and newer response distributions than training solely on stronger or solely on weaker responses. Finally, we find that fine-tuned judges struggle to generalize to new questions. In all, our findings inform the development and deployment of future generations of finetuned judge models.

2 Background and Related Work

2.1 An overview of finetuned judges.

LLM-based judges are automatic evaluators that evaluate LLM outputs given some evaluation criteria. While many judges accommodate different evaluation tasks, such as single rating ("Rate this response on a scale of 1-5") (Hu et al., 2024) or classification ("Is this response appropriate?") (Vu et al., 2024), the dominant evaluation paradigm LLM-based judges are deployed with is *pairwise evaluation.* Here, a judge is given a question and two candidate responses, and tasked with selecting the "better" response according to some criteria. Formally, the judge performs the transformation

$$(Q, R_1, R_2) \quad \longrightarrow \quad (C, \hat{V}), \quad C \text{ optional}, \tag{1}$$

where Q is the question, R_1, R_2 are the two candidate responses, C is an optional chain-of-thought explanation, and $\hat{V}$ is the verdict of which response is better. We denote $x = (Q, R_1, R_2) \sim \mathcal{X}$ to be the judge input and $y = (C, \hat{V})$ to be the judge output. Pairwise judges are typically evaluated using accuracy or consistent accuracy, the latter accounting for response-order bias as detailed in Appendix C. Due to its popularity and practicality, pairwise evaluation forms the focus of our study.

Past work in judge finetuning uses supervised finetuning (SFT) (Li et al., 2024b; Kim et al., 2024b; Zhu et al., 2025), preference optimization methods, like direct preference optimization (DPO) (Wang et al., 2024a; Ye et al., 2024; Saad-Falcon et al., 2024), or more recently, reinforcement learning with verifiable rewards (RLVR) (Chen et al., 2025a;c; Whitehouse et al., 2025; Xu et al., 2025b). Starting from a dataset of $(x, V^\star(x))$ pairs, where $V^\star$ denotes the ground-truth verdict/label, each approach constructs training samples differently: SFT and DPO approaches sample judge outputs from a *teacher model*, then use $V^\star(x)$ to categorize judge outputs as either correct outputs y^+ or incorrect outputs y^-. Then, the judge is trained on (x, y^+) pairs for SFT and (x, y^+, y^-) triplets for DPO. On the other hand, RL approaches directly make use of the $(x, V^\star(x))$ pairs, omitting the need for teacher model explanations.

2.2 Related work

Distribution Shifts and Generalization. Distribution shift, the mismatch between training and evaluation data, is a long-standing challenge in machine learning (Hendrycks & Dietterich, 2019; Koh et al., 2021). Early computer vision studies demonstrated significant accuracy drops under minor perturbations (Hendrycks & Dietterich, 2019), and WILDS extended this to real-world domain shifts (Koh et al., 2021). In LLMs, the problem is amplified as both data and model capabilities evolve over time (Shi et al., 2025). Recent frameworks explore how models transfer across distributions. *Easy-to-hard generalization* examines whether training on easier tasks transfers to harder ones (Sun et al., 2024), which relates to scalable oversight where only easy tasks can be reliably supervised (Amodei et al., 2016); task-difficulty can be estimated using either model or data-centric measures (Swayamdipta et al., 2020). *Weak-to-strong generalization* investigates improving strong models using supervision derived from weaker ones (Burns et al., 2024). Our setting complements these efforts by focusing on distribution shifts that arise from an *evolving population of generators* and by evaluating how judge models adapt to both weak-to-strong and strong-to-weak shifts.

Analyzing LLM-as-Judge. Prior work analyzes systematic judge biases such as positional (Wang et al., 2023; Li et al., 2024b), length (Zeng et al., 2024; Park et al., 2024b), and self-preference (Panickssery et al., 2024; Chen et al., 2025b). Prompt design, instructions, and scoring format strongly affect reliability (Murugadoss et al., 2025; Li et al., 2024a), with pairwise judgments often reducing noise and aligning better with human preferences than pointwise scores (Tripathi et al., 2025; Jeong et al., 2024). Other works have emphasized the importance of carefully selecting reference answers (Krumdick et al., 2025), linking to how generator capabilities influence the judge's inputs (Tan et al., 2025). While most studies consider *static* judges on *fixed* datasets, we instead analyze judges in a dynamic setting where generators change over time, introducing response-distribution shifts that motivate our metrics for *future-proofing*, *backward compatibility*, and *question generalization*.

3 Automatic Evaluation as a Dual-Distribution Problem

We propose a novel formulation of the automatic evaluation problem in terms of two distributions: the question distribution and the response distribution. Concretely, let $\mathcal{Q}$ denote the distribution of questions Q, and let $\mathcal{R}$ denote the distribution of responses R. For pairwise judges, the input distribution $\mathcal{X}$ therefore takes the form

$$\mathcal{X} = \mathcal{Q} \times \mathcal{R} \times \mathcal{R} \tag{2}$$

The question distribution is defined by characteristics such as semantic content (e.g., domains like medical, legal, finance, scientific, or math) and question difficulty (e.g., difficulty can be defined by pedagogical levels, such as high school vs. olympiad-level math problems). For example, we can consider all questions in GSM8K (Cobbe et al., 2021) to come from the same question distribution, as they are all arguably of similar difficulty and semantic content. The response distribution defines the characteristics of the model responses being evaluated, such as style, capability-specific content, or model-family-specific quirks. We denote the training and test input distributions to be

$$\mathcal{X}^{train} = \mathcal{Q}^{train} \times \mathcal{R}^{train} \times \mathcal{R}^{train} \quad \text{and} \quad \mathcal{X}^{test} = \mathcal{Q}^{test} \times \mathcal{R}^{test} \times \mathcal{R}^{test} \tag{3}$$

respectively. Notably, the two responses come from the same generating model, as described in the data construction details in Section 4.2. Separating the *question distribution* $\mathcal{Q}$ from the *response distribution* $\mathcal{R}$ reflects two real-world sources of shift: (1) the emergence of more capable generators (an evolving $\mathcal{R}$), and (2) the introduction of new questions (an evolving $\mathcal{Q}$). This decomposition allows us to isolate and quantify the impact of each factor on judge performance. In Section 5, we instantiate this framework using the weak response distribution $\mathcal{R}_{weak}$ and the strong response distribution $\mathcal{R}_{strong}$ to simulate a model-development timeline (older, weaker vs. newer, stronger responses and LLMs), along with question splits Q drawn from $\mathcal{Q}$ that are either seen or unseen during training. Informally, weak (strong) responses are drawn from LLMs with relatively low (high) accuracy on questions Q; we precisely describe generator strength in Section 4. This instantiation enables us to investigate the four practical questions mentioned in Section 1 regarding the *shelf life* of judges. The specifics of how dual-distribution formalization supports our analysis are detailed in Section 5, with a concise connection provided in Appendix D.

4 Experimental Setup

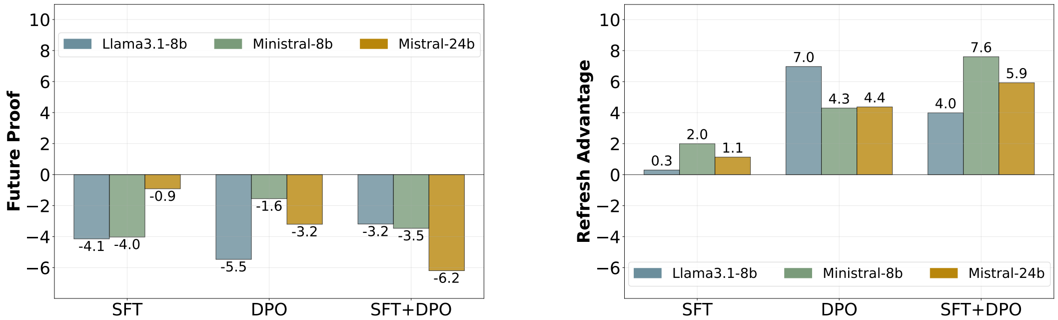
4.1 Gauging Generator Strength.

We ground our study in mathematics, a domain with verifiable solutions, and use the DeepScaleR dataset, which contains 40K Olympiad-style problems with gold answers. For each generator, we sample 20 responses per question and measure its strength using pass@1. The pass@1 metric captures the probability that a uniformly sampled attempt is correct. This yields two clear tiers, as shown in Figure 7 in Appendix B: recent or larger models attain pass@1 scores of about 0.45, while smaller or older models cluster near 0.25. We therefore group generators into *weaker* and *stronger* strength and use these groups to define our response-distribution shifts. Further details on the choice of generators and how their strength is gauged are provided in Appendix B.

4.2 Training Setup.

Dataset Construction. To create the training and evaluation splits, we first construct pairwise input samples for the judge, following prior work (Tan et al., 2025; Wang et al., 2024b). For each question, we sample multiple responses from each generator, and each response is then labeled as "correct" or "incorrect" according to the ground-truth answer $A^{\star}$. We then form response pairs, where each pair consists of one correct response and one incorrect response, resulting in a pairwise sample with an objectively correct answer. Importantly, responses in a pair are drawn from a single generator only. Based on the generator strengths defined above, we construct datasets of aggregated pairwise samples consisting exclusively of either weak or strong responses, which we refer to as our *weak dataset* and *strong dataset*, respectively.

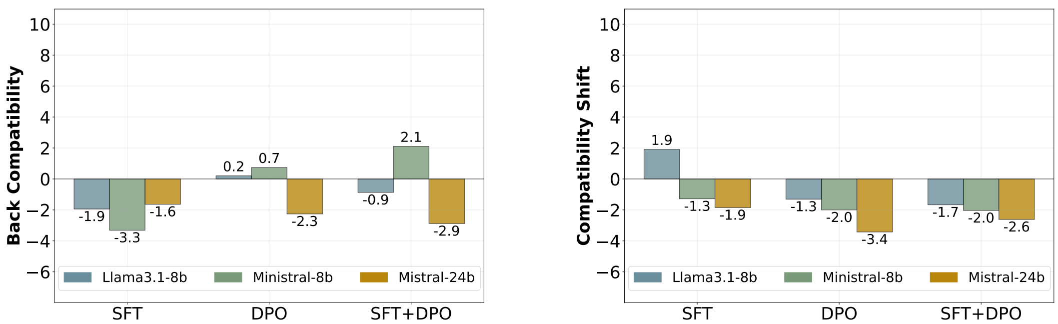
Judge Data Distillation & Training Objectives. We train judges using three commonly adopted recipes: supervised fine-tuning (SFT) (Li et al., 2024b; Kim et al., 2024a; Vu et al., 2024), direct



(a) Future Proof

(b) Refresh Advantage

Figure 2: (a) Future-proofing measured by `FutureProof`; negative values show degraded performance on stronger responses. All models and recipes performance degrade, indicating poor evaluation of newer, stronger responses. (b) Benefits of re-training on strong responses, measured by `RefreshAdvantage`. Re-training consistently improves performance, with the largest gains under DPO.



(a) `BackCompatibility`.

(b) `CompatibilityShift`.

Figure 3: (a) `BackCompatibility` of judges trained on strong responses when evaluating older responses; positive values indicate improved performance relative to older-judge baselines. Judges trained on newer responses show good `BackCompatibility`, with minimal drops—or even absolute *gains*. (b) Despite strong absolute performance, newer judges still face a distribution shift, reflected by `CompatibilityShift`, with performance drops relative to evaluating strong responses. (c) Compared with future-proofing metrics in Section 4.2, backward-compatibility metrics are smaller, indicating that strong-response–trained judges are more backward-compatible than weak-response–trained judges are future-proof.

preference optimization (DPO) (Hu et al., 2024; Wang et al., 2024b), and a combined SFT and DPO objective (Wang et al., 2024a; Ye et al., 2024; Saad-Falcon et al., 2024). As these recipes require supervision, specifically, the CoT explanation C (Sec. 2), we adopt the common *teacher model* convention (Li et al., 2024b; Wang et al., 2024a). Based on the ground-truth verdict V^*, we categorize responses as correct (positive) samples y^+ or incorrect (negative) samples y^-. Positive samples are then used for SFT, whereas positive-negative pairs are used for DPO-based recipes.

Training and Evaluation Splits. To analyze the four practical questions described in Section 1 using the dual-distribution framework from Section 3, we split the weak and strong datasets into training and test sets. For testing, we construct two distinct splits: an *unseen-questions* split and a *seen-questions* split. The unseen-questions split contains questions not present during training, while seen-questions split reuses training questions but samples *new* responses, with pairs constructed following the same process as described above. Unless otherwise specified, we use the unseen-questions split for evaluation. We choose three models to train: Llama-3.1-8B, Ministral-8B, and Mistral-24B, covering a range of model sizes and intrinsic strengths.

We provide more details on different aspects of the training setup in Appendix E.

5 EXPERIMENTAL RESULTS

In this section, we present our analysis setup and findings on future-proofing, backward-compatibility, and question-generalization of judge models. Our analysis builds on the dual-distribution framework introduced in Section 3, where judge inputs are factorized into a question distribution $\mathcal{Q}$ and a response distribution $\mathcal{R}$. We instantiate the response distribution at two levels of generator strength: $\mathcal{R}_{weak}$ (older, less capable models) and $\mathcal{R}_{strong}$ (newer, more capable models). The question distribution $\mathcal{Q}$ remains fixed but varies in whether a question was seen or unseen during training. In this way, our setup simulates model development timelines. We measure judge performance using consistent accuracy, as defined in Appendix C. Raw consistent accuracy scores are reported in Table 2 of Appendix C, and serve as the foundation for the results below.

Notation. For clarity, we denote the consistent accuracy of a judge J_t trained on response distribution t as $\mathrm{Acc}_e(J_t)$, where $t \in \{\text{weak}, \text{strong}\}$. The subscript e indicates the evaluation distribution, with $e \in \{\text{weak}, \text{strong}\}$. Thus, $\mathrm{Acc}_e(J_t)$ ties back to our dual-distribution formalism: it measures the accuracy of a judge trained on distribution t when evaluated on responses from distribution e.

5.1 HOW FUTURE-PROOF ARE JUDGE MODELS?

Experimental Setup. To study *future-proofing* in our simulated model development timeline, we design the following setup: weak generators serve as proxies for existing LLMs, and judges are trained on their responses. Strong generators represent newly released LLMs with greater capabilities. By future-proofing, we refer to how well weak-response-trained judges can evaluate responses from newer, stronger LLMs. Specifically, we quantify future-proofing using the following metrics:

`FutureProof` is defined as the difference in the performance of a weak-response-trained judge between the weak and strong evaluation sets:

$$\texttt{FutureProof} = \mathrm{Acc}_{strong}(J_{weak}) - \mathrm{Acc}_{weak}(J_{weak}). \tag{4}$$

This measures the change in performance when the evaluation distribution shifts from $\mathcal{R}^{test}_{weak}$ to $\mathcal{R}^{test}_{strong}$, i.e., a *weak-to-strong* response distribution shift. A positive value indicates relatively better performance on strong responses, while a negative value indicates degradation; thus, higher values correspond to more future-proof judges.

`RefreshAdvantage` is defined as the gain from re-training judges with strong responses:

$$\texttt{RefreshAdvantage} = \mathrm{Acc}_{strong}(J_{strong}) - \mathrm{Acc}_{strong}(J_{weak}). \tag{5}$$

This can be viewed as the *data advantage* from changing the training response distribution from $\mathcal{R}^{train}_{weak}$ to $\mathcal{R}^{train}_{strong}$ when evaluating on $\mathcal{R}^{test}_{strong}$. Higher values indicate greater benefit from re-training judges with the latest and stronger responses.

`FutureProof` Findings: For all models and training recipes, we plot the `FutureProof` values in Figure 2a. Across all settings, we do not observe any instance where judges generalize to new or stronger responses, with all `FutureProof` values being negative. Interestingly, no discernible trend emerges across training recipes or model families. Generally, we find that SFT leads to higher degradations in smaller models, but a smaller degradation in the large judge. In all, our results show that current judge training approaches do not produce judges capable of reliably generalizing to new, more capable model responses. Beyond lack of generalization, current judge recipes do not exhibit consistent trends across base models and scales. In the absence of recipe-specific or base-model-specific trends, we recommend evaluating `FutureProof` on a model-by-model basis.

`RefreshAdvantage` Findings. Our results, presented in Figure 2b, indicate that re-training with up-to-date responses consistently leads to performance gains. In particular, across all training recipes and base models, we observe positive `RefreshAdvantage` values. Training recipes also follow a clear trend: retraining with SFT yields minimal but positive gains, whereas DPO yields the largest improvements, providing up to 7.6 absolute percentage points for larger models. The combination of DPO and SFT losses provides additional benefit over DPO alone for couple of models. We further observe that as judge model size increases, updating training data has a larger impact for DPO-based approaches. For example, with DPO, Mistral-24B exhibits an absolute gain of 7.6 percentage points compared to its 8B counterpart, Mistral-8B, which improves by 4.3 points. Overall, these results reveal that to evaluate the most capable models, evaluators must be trained on their outputs; relying on stale training data leaves significant performance gains unrealized.

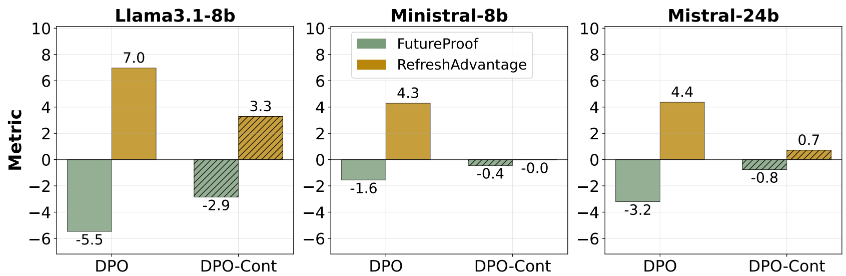


Figure 4: Changes in future-proofing metrics when replacing a weak-response-trained judge (solid) with a continually trained judge (dashed). We observe a decrease in `RefreshAdvantage` and an increase in `FutureProof`, with values approaching zero for a couple of models. This suggests that continual training enables judges to evaluate strong responses more effectively than weak-trained judges, as well as strong-trained judges, and adapts better to the weak-to-strong response shift.

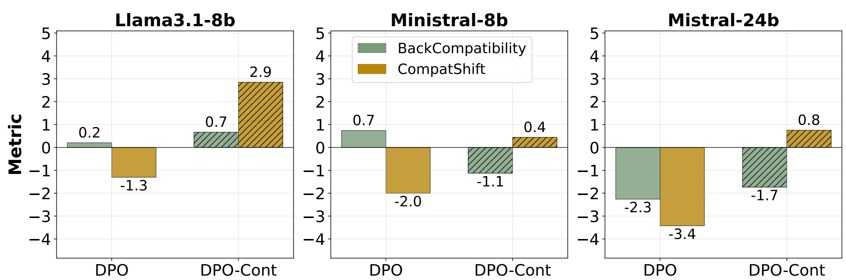


Figure 5: Changes in backward-compatibility metrics when replacing a strong-response-trained judge (solid) with a continually-trained judge (dashed). We see an increase in `BackCompat` for a couple of models, suggesting that continual training can help models better evaluate weak responses than purely strong-trained judges. We also observe an increase in `CompatShift`, showing that continually trained judges adapt better to the strong-to-weak response shift.

5.2 How backward-compatible are judge models?

Experimental setup. Now, we extend our setup for *future-proofing* in Section 5.1 to study *backward-compatibility* in a simulated model development timeline. A judge trained on strong or newer generator responses represents the current judge, which is adept at evaluating new responses, while weak generators represent older LLMs with lower capabilities. By *backward-compatibility*, we refer to how well strong-response-trained judges can evaluate the responses of older, weaker generators. Specifically, we quantify backward-compatibility using the following metrics:

`BackCompatibility` measures the performance gap when evaluating older, weaker responses with the refreshed strong-response-trained judge instead of the weak-response-trained judge:

$$\texttt{BackCompatibility} = \text{Acc}_{weak}(J_{strong}) - \text{Acc}_{weak}(J_{weak}). \tag{6}$$

This setting is particularly important for established evaluation pipelines: if an old judge is replaced by a new one while the task remains the same, how much does performance differ? We view this as the *data disadvantage* from changing training data from $\mathcal{R}_{weak}^{train}$ to $\mathcal{R}_{strong}^{train}$ when evaluating on $\mathcal{R}_{weak}^{test}$. A positive `BackCompatibility` indicates that the strong-trained judge outperforms the weak-trained judge on weak responses (good backward compatibility), while a negative value reflects performance degradation (poor backward compatibility).

`CompatibilityShift` quantifies the weak-to-strong distribution shift when evaluating older, weaker responses with a strong-response-trained, refreshed judge. As noted in the previous section, the reverse shift (strong-to-weak) can strongly affect judge performance. Here, we measure how the out-of-distribution nature of backward compatibility impacts the newly trained judge:

$$\texttt{CompatibilityShift} = \text{Acc}_{weak}(J_{strong}) - \text{Acc}_{strong}(J_{strong}). \tag{7}$$

This captures the response-distribution shift opposite to `FutureProof`, i.e., from $\mathcal{R}_{\text{strong}}^{\text{test}}$ to $\mathcal{R}_{\text{weak}}^{\text{test}}$ or *strong-to-weak*. It measures how far a strong-trained judge falls below its potential under in-distribution evaluation. A positive value indicates better relative performance on weak responses, while a negative value indicates degradation.

`BackCompatibility` Findings. In Figure 3a, we visualize the backward compatibility of judge models trained on strong responses. When evaluating on weak responses, there is little drop in absolute performance between judges trained on strong responses and those trained on in-distribution weak responses. While methods involving SFT consistently cause small performance drops, our results show that DPO training can enable newly trained judges to *outperform* weak-judge models. The drop due to incompatibility is smaller than the advantage gained when moving from weak to strong responses, as noted in the `RefreshAdvantage` findings. This indicates that judges trained on newer responses are indeed backward compatible: they closely mimic the performance of weak-trained judges, even in out-of-distribution settings. Thus, combined with our findings in Section 5.1, we conclude that re-training with updated responses is universally beneficial: such refreshed judges are not only much better at evaluating new model responses but can also serve as drop-in replacements for their older counterparts with minimal loss in performance.

`CompatibilityShift` Findings. As shown above, judges trained on strong responses roughly match the performance of those trained on weak responses when evaluating older responses. Despite strong absolute performance, such newer judges are evaluating under a *strong-to-weak* distribution shift; Figure 3b plots the drop in performance due to this shift. Here, we observe that across all judges and recipes, judges still experience degradation due to the out-of-distribution nature of evaluation, with the lone exception being SFT-trained Llama3.1-8B. Surprisingly, here, the largest model, finetuned from Mistral-24B, experiences the largest absolute drops across all training recipes. These findings highlight that, while stronger trained judges can serve as appropriate drop-in replacements for weaker judges, distribution shift causes them to underperform relative to their potential. However, compared to the degradation from the weak-to-strong response-distribution shift (as measured by `FutureProof` in Section 5.1), these degradations are relatively smaller. This suggests that the weak-to-strong evaluation response-distribution shift is a harder setting than strong-to-weak, again highlighting the importance of retraining judges on new model responses.

5.3 CAN CONTINUAL TRAINING IMPROVE FUTURE-PROOFING AND BACKWARD-COMPATIBILITY OF JUDGE MODELS?

Experimental setup. As discussed in Sections 5.1 and 5.2, training a judge *from scratch* on responses from newer generators is advantageous in evaluations. An alternative is to *continually update* a judge originally trained on older responses by incrementally fine-tuning it on newer, stronger responses. We simulate this continual learning paradigm by iteratively training J_{weak} on responses from stronger generators, denoting the resulting model as $J_{weak \to strong}$ (see Appendix E for details). All experiments in this section use the DPO recipe due to compute constraints.

To assess the effect of continual training, we evaluate $J_{weak \to strong}$ on both future-proofing and backward-compatibility metrics, comparing its performance against that of the original weakly trained judge and the strongly trained judge, respectively. Specifically, we compare `FutureProof` and `RefreshAdvantage` when replacing J_{weak} with $J_{weak \to strong}$ in Equations (5)–(6), as shown in Figure 4. We also compare `CompatibilityShift` and `BackCompat` when replacing J_{strong} with $J_{weak \to strong}$ in Equations (7)–(8), as shown in Figure 5. Together, these comparisons reveal how continual training helps weak judges adapt to future distribution shifts while retaining compatibility with weaker responses, relative to training from scratch.

Changes in Future-Proofing. Figure 4 shows that continual training consistently improves future-proofing. `FutureProof` scores increase across all three models, while `RefreshAdvantage` decreases, approaching zero for Ministral-8B and Mistral-24B. The reduction in `RefreshAdvantage` indicates that the benefit of retraining a strong model from scratch, relative to continual training, largely disappears when evaluating stronger responses. At the same time, the higher `FutureProof` scores of $J_{weak \to strong}$ demonstrate that continual training enables better adaptation to the weak-to-strong distribution shift than simply retaining the weak model.

Changes in Backward-Compatibility. Figure 5 shows mixed but informative results on backward-compatibility. `BackCompatibility` scores increase for Mistral-24B and Llama-3.1-8B but decrease for Ministral-8B. Higher `BackCompatibility` indicates that a continually trained judge remains closer to the weakly trained judge when evaluating weak responses, compared to a model

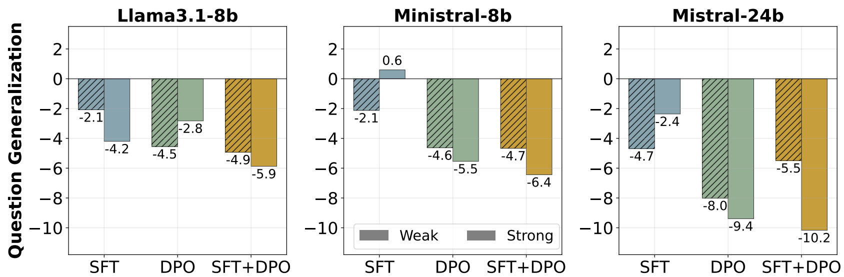


Figure 6: Generalization of judges trained on weak vs. strong responses to seen and unseen questions. Judges typically fail to generalize to unseen questions, showing large performance drops relative to evaluating unseen responses on seen questions.

trained solely on strong responses. We also observe a notable increase in `CompatibilityShift`, highlighting that continual training improves adaptation to older, weaker responses relative to purely strong-trained models. Together, these results suggest that continual training can better preserve backward-compatibility in several settings while also enhancing adaptability to distribution shifts.

5.4 How do judges generalize to unseen questions and responses?

Experimental setup. As LLMs advance, both responses and questions evolve (e.g., AIME24 vs. AIME25). We therefore examine how judges perform on previously unseen questions by sampling from $\mathcal{Q}$ in our dual-distribution framework. To quantify the benefits of question exposure during judge training, we define two evaluation splits. In the first, we select a subset of training questions and sample new responses for them, which we call the *seen-questions, unseen-responses* split. In the second, we draw questions from $\mathcal{Q}^{\text{train}}$ that were excluded from training and pair them with new responses, defining the *unseen-questions, unseen-responses* split. Comparing judge performance across these splits reveals the performance gap due to question generalization.

$$\texttt{QuestionGen}_{weak} = \text{Acc}_{weak,unseen}(J_{weak}) - \text{Acc}_{weak,seen}(J_{weak}) \tag{8}$$
$$\texttt{QuestionGen}_{strong} = \text{Acc}_{strong,unseen}(J_{strong}) - \text{Acc}_{strong,seen}(J_{strong}). \tag{9}$$

These metrics capture how well judges generalize across *questions*: responses are drawn from the same generator, with only the question split (seen vs. unseen during training) varied. A positive value of `QuestionGen` indicates better performance on unseen questions, while a negative value indicates failure to generalize to unseen questions.

Findings. As shown in Figure 6, current judge models do not generalize well to unseen questions, with nearly all judges exhibiting performance drops compared to evaluating on seen questions with unseen responses. Surprisingly, we find that SFT enables the best generalization, with SFT-trained judges showing the smallest absolute drops in most cases. Mistral-24B, however, exhibits the largest drops within each training recipe, indicating poorer generalization compared to smaller models. Overall, our experiments reveal that exposing judges to the questions they are likely to evaluate can lead to significant performance gains.

6 Conclusion

We present a dual-distribution framework for automatic evaluation and analyze four key questions surrounding finetuned LLM-as-judge models, a crucial component of the LLM development cycle. First, we study future-proofing and show that judges trained on older responses struggle to evaluate outputs from newer, stronger LLMs, but re-training on newer responses yields substantial gains. Second, we examine backward compatibility and find that judges trained on newer responses incur only minor drops, or even improvements, when evaluating older responses. Third, we demonstrate that continual learning provides a more balanced adaptation to both older and newer response distributions compared to training solely on stronger or weaker responses. Finally, we investigate question generalization and find that judges experience large drops in performance on questions unseen during training. Overall, our work highlights critical challenges and actionable strategies for developing robust, future-proof, and backward-compatible judge models.

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A LLM Usage

Other than being used as part of the experiments conducted in this work, LLMs were used solely as a writing assistance tool in preparing this paper submission. Their role was limited to polishing

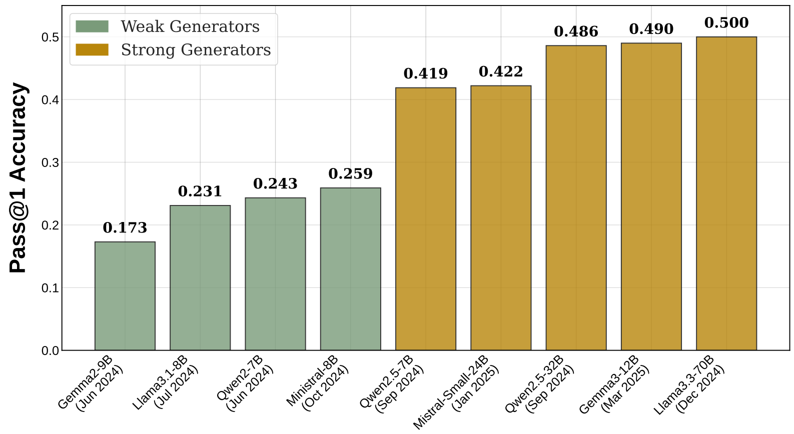

Figure 7: Generator strength on the DeepScaleR dataset, measured using pass@1 with 20 independently sampled responses. A clear clustering emerges, with stronger, newer models (yellow) outperforming weaker, older (green) ones.

language, improving clarity, and reducing redundancy. The prompt used for this purpose was similar to "Please revise the writing of this, making sure to remove any grammatical mistakes." All research ideas, experimental designs, analyses, and claims presented in the paper are entirely the original work of the authors. No part of the conceptual, methodological, or empirical contributions relies on or originates from LLM outputs.

B GENERATORS AND GENERATOR STRENGTHS

Shorthand	Full Model Identifier
Llama3.3-70B	`meta-llama/Llama-3.3-70B-Instruct`
Llama3.1-8B	`meta-llama/Llama-3.1-8B-Instruct`
Qwen2-7B	`Qwen/Qwen2-7B-Instruct`
Qwen2.5-7B	`Qwen/Qwen2.5-7B-Instruct`
Qwen2.5-14B	`Qwen/Qwen2.5-14B-Instruct`
Qwen2.5-32B	`Qwen/Qwen2.5-32B-Instruct`
Gemma2-9B	`google/gemma-2-9b-it`
Gemma3-12B	`google/gemma-3-12b-it`
Ministral-8B	`mistralai/Ministral-8B-Instruct-2410`
Mistral-24B	`mistralai/Mistral-Small-24B-Instruct-2501`

Table 1: Mapping of shorthand model names to their corresponding Hugging Face identifiers.

To ground our study, we choose the mathematics domain, as it provides objective verifiability and has also been the focus of much contemporary LLM reasoning research. We begin with a set of candidate generators and a collection of math questions Q, along with verifiable ground-truth answers $A^{\star}$. For each question, we sample 20 responses from each generator using temperature sampling and compute a *pass@1* score. This score represents the probability of obtaining at least one correct solution when randomly selecting one solution from the 20 attempts, where correctness is determined by matching the generator's responses against $A^{\star}$.

Concretely, we use the DeepScaleR Luo et al. (2025) dataset, which contains 40K challenging Olympiad-level math problems spanning multiple years with corresponding ground-truth answers. We then select popular open-source instruction-tuned models, as listed in Table 1: Gemma-2-9B (Riviere et al., 2024), Gemma-3-12B (Kamath et al., 2025), Llama-3.1-8B, Llama-3.3-70B Dubey et al. (2024), Ministral-8B (Team, a), and Mistral-Small-24B (Team, b), Qwen2-7B (Yang et al., 2024a), Qwen2.5-7B (Yang et al., 2024b), and Qwen2.5-32B (Yang et al., 2024b).

Figure 7, which plots the pass@1 scores of these models, reveals two distinct tiers within the group: Newer, larger models (e.g., Qwen2.5-32B, Mistral-Small-24B) tend to have pass rates around 0.45, whereas smaller, older models lag, with average pass-rate around 0.25. We therefore divide our set of models into two groups: Weak generators (Gemma-2-9B, Llama3.1-8B, Qwen2-7B, Ministral-8B) and strong generators (Gemma-3-12B, Qwen2.5-7B, Mistral-Small-24B, Llama3.3-70B, and Qwen2.5-32B).

C Consistent Accuracy and Judge's Performance Across splits

Consistent Accuracy. Since judge models are prone to positional biases (Wang et al., 2023; Li et al., 2024b; Xu et al., 2025b)—where their preference shifts depending on whether R_1 or R_2 appears first in the prompt—it is standard practice to evaluate judges using both response orderings (Tan et al., 2025; Xu et al., 2025a;b). Concretely, for input $x = (Q, R_1, R_2)$, let $\bar{x}$ denote the same sample, but with response order flipped in the input prompt, i.e., $\bar{x} = (Q, R_2, R_1)$. Then, evaluation with *consistent accuracy* considers the judge correct only if it correctly identifies the better response under both orderings:

$$\text{Acc} = \frac{1}{|P|} \sum_{x \in P} \mathbb{1}[\hat{V}(x) = V^{*}(x) \wedge \hat{V}(\bar{x}) = V^{*}(\bar{x})], \tag{10}$$

where $\mathbb{1}[\cdot]$ is the indicator function, P is the evaluation set consisting of pairs $(x, V^{\star}(x))$, and the judge's verdicts $\hat{V}(x)$ are compared against the ground-truth verdicts $V^{\star}(x)$.

Furthermore, we report all consistent accuracy scores for our trained judges across different evaluation splits in Table 2.

D Research Questions in the Dual-Distribution Formulation

As described in Section 3, the dual-distribution formulation separates the *question distribution* $\mathcal{Q}$ from the *response distribution* $\mathcal{R}$, reflecting two real-world sources of shift: (1) more capable generators (an evolving $\mathcal{R}$) and (2) new questions (an evolving $\mathcal{Q}$). This decomposition allows us to isolate and quantify the impact of each factor on judge performance. Building on this, we investigate several practical questions about the *shelf life* of trained judges, focusing on four distinct settings:

How future-proof are judge models? For a judge to be future-proof, it must be able to evaluate responses from newer, stronger models. To study this, we examine how a judge trained on responses from the current generation of weak models performs when evaluating responses from strong models. Specifically, we train a judge on $\mathcal{R}^{train}_{weak}$ and evaluate it on both $\mathcal{R}^{test}_{weak}$ and $\mathcal{R}^{test}_{strong}$. This setup characterizes how robust judges are to a *distribution shift from weak to strong* responses. Additionally, we quantify the gains from retraining a judge on strong responses by replacing training data from $\mathcal{R}^{train}_{weak}$ with responses from $\mathcal{R}^{train}_{strong}$.

How backward-compatible are judge models? Newly trained judges are fine-tuned to evaluate newer, stronger response-generating models. However, does this focus on state-of-the-art generators come at the expense of performance on older, more established generators? To complement our future-proofing experiments, we examine backward compatibility. Specifically, given a judge trained on responses from $\mathcal{R}^{train}_{strong}$, we ask: how well does it match a judge trained on weaker responses from $\mathcal{R}^{train}_{weak}$ when both are evaluating $\mathcal{R}^{test}_{weak}$ responses? Beyond this comparison, evaluating weaker responses with a judge trained on strong responses also introduces a *distribution shift from strong to weak* responses. We quantify any performance losses that result from this shift.

Can continual learning improve future-proofing and backward-compatibility of judge models? Rather than training a new judge from scratch on $\mathcal{R}_{strong}$, we start with a judge trained on $\mathcal{R}^{train}_{weak}$ and continually fine-tune it on $\mathcal{R}^{train}_{strong}$ to obtain a continually trained judge. In parallel to the settings above, we ask whether the continually trained judge narrows the gap on $\mathcal{R}^{test}_{strong}$ relative to one trained only on $\mathcal{R}^{train}_{weak}$, and whether it retains performance on $\mathcal{R}^{test}_{weak}$ relative to a judge trained from scratch on $\mathcal{R}^{train}_{strong}$. This setup tests whether continual training helps a weak judge adapt to the weak to strong response shift while preserving compatibility with older responses.

How do judges generalize across unseen questions? As new questions are introduced for evaluating LLMs, judge models must accurately assess responses to these questions. Here, we quantify the benefit of a judge model having seen a question during training. To study this form of *generalization*, we construct two evaluation splits. The first is a *seen-questions, unseen-responses* split, created by selecting questions that appeared in the training set and sampling a new set of responses for these questions from $\mathcal{R}^{train}$. The second is an *unseen-questions, unseen-responses* split, generated by sampling questions from $\mathcal{Q}^{train}$ that were not included in the training data, along with their corresponding responses from $\mathcal{R}^{train}$. Comparing performance across these splits enables us to assess how well judges generalize to previously seen questions versus entirely new ones.

E Training Setup Details

Dataset Construction. To create the training and evaluation splits, we first construct pairwise input samples for the judge, following prior work (Tan et al., 2025; Wang et al., 2024b). For each question, we sample multiple responses from each generator, and each response is then labeled as "correct" or "incorrect" according to the ground-truth answer $A^{\star}$. We then form response pairs, where each pair consists of one correct response and one incorrect response, resulting in a pairwise sample with an objectively correct answer. Importantly, responses in a pair are drawn from a single generator only. This choice ensures that the judge learns to distinguish correctness based on reasoning quality rather than relying on stylistic differences between models, which could occur if responses from different generators were mixed in a single pair. For each generator and question, we only keep samples where there is at least one correct and one incorrect response and if this condition is not met, the question is discarded for that generator. Based on the generator strengths defined above, we construct datasets of aggregated pairwise samples consisting exclusively of either weak or strong responses, which we refer to as our *weak dataset* and *strong dataset*, respectively.

Judge Data Distillation & Training Objectives. We train judges using three commonly adopted recipes: supervised fine-tuning (SFT) (Li et al., 2024b; Kim et al., 2024a; Vu et al., 2024), direct preference optimization (DPO) (Hu et al., 2024; Wang et al., 2024b), and a combined SFT and DPO objective (Wang et al., 2024a; Ye et al., 2024; Saad-Falcon et al., 2024). As these recipes require supervision, specifically, the CoT explanation C (Sec. 2), we adopt the common *teacher model* convention (Li et al., 2024b; Wang et al., 2024a). We prompt GPT-4o with (Q, R_1, R_2) inputs, sampling multiple responses $(C, \hat{V})$ per input. Based on the ground-truth verdict V^{*}, we categorize responses as correct (positive) samples y^{+} or incorrect (negative) samples y^{-}. We only keep inputs for which at least one y^{+} and y^{-} exists. This ensures that the inputs are exactly comparable for SFT and DPO. Positive samples are then used for SFT, whereas positive-negative pairs are used for DPO-based recipes.

Train and Evaluation Splits. To analyze the four practical questions described in Section 1 using the dual-distribution framework from Section 3, we split the weak and strong datasets into training and test sets. For testing, we construct two distinct splits: an *unseen-questions* split and a *seen-questions* split. The unseen-questions split contains questions not present during training, while seen-questions split reuses training questions but samples *new* responses, with pairs constructed following the same process as described above. Unless otherwise specified, we use unseen-questions split for evaluation. Each training set contains roughly 70K samples, and each evaluation split contains about 2.5K response-order-unflipped samples (5K after including response-order flips).

Base Judge and Generator Details. We choose three base models to finetune: Llama-3.1-8B, Ministral-8B, and Mistral-24B, covering a range of model sizes and intrinsic strengths. Prior work (Tan et al., 2025) has shown that models often struggle to judge the correctness of pairs of their own sampled responses. Another line of work (Chen et al., 2025b; Panickssery et al., 2024) has shown that models can recognize their own responses and exhibit self-bias. Thus, to disentangle any effects of training a judge on self-generated responses, we exclude the base judge model from serving as a generator. Specifically, we create two training sets (each with weak and strong splits), ensuring that the base judge model is not included in the list of generators. We summarize these training sets and the associated base models in Table 3.

Hyperparameters. All experiments with SFT, DPO, SFT+DPO are implemented using the Axolotl framework Axolotl maintainers and contributors (2023). For SFT, we sweep learning rates in $\{1 \times 10^{-6}, 2.5 \times 10^{-6}, 5 \times 10^{-6}, 1 \times 10^{-5}\}$ with a cosine decay scheduler. Across all evaluation splits, a learning rate of 2.5×10^{-6} consistently yields the best performance. For DPO, we adopt standard hyperparameter choices from prior work (Ivison et al., 2023), using a learning rate of 5×10^{-7} and a preference strength parameter $\beta = 0.1$. For SFT+DPO, we optimize a joint loss with equal weighting between the SFT and DPO objectives, using the same DPO hyperparameters (learning rate 5×10^{-7}, $\beta = 0.1$). All weak and strong judges are trained for 3 epochs, corresponding to 2,800 gradient steps. For continual training experiments (section 5.3), we start from a weak-response DPO-trained judge (trained for 3 epochs) and further train it on strong responses for 1 additional epoch, amounting to roughly 1,000 additional gradient steps. We sweep $\beta \in \{0.1, 1.0\}$ and report results in the main text using $\beta = 1.0$; additional results are included in Table 2 and in Appendix C.

Metric	Base	SFT	DPO	SFT+DPO
Llama3.1-8B (Judge trained on Weak)				
Weak, Seen	32.44	48.14	43.95	63.40
Strong, Seen	28.41	44.66	36.62	60.48
Weak, Unseen	30.79	46.06	39.41	58.47
Strong, Unseen	27.76	41.91	33.94	55.29
Llama3.1-8B (Judge trained on Strong)				
Weak, Seen	32.44	45.33	42.53	61.72
Strong, Seen	28.41	46.41	43.74	65.15
Weak, Unseen	30.79	44.12	39.61	57.60
Strong, Unseen	27.76	42.21	40.91	59.27
Llama3.1-8B (Continual, $\beta = 0.1$)				
Weak, Seen	32.44	–	44.69	–
Strong, Seen	28.41	–	41.19	–
Weak, Unseen	30.79	–	40.09	–
Strong, Unseen	27.76	–	38.41	–
Llama3.1-8B (Continual, $\beta = 1.0$)				
Weak, Seen	32.44	–	45.43	–
Strong, Seen	28.41	–	39.13	–
Weak, Unseen	30.79	–	40.07	–
Strong, Unseen	27.76	–	37.22	–
Ministral-8B (Judge trained on Weak)				
Weak, Seen	33.87	48.06	61.04	61.39
Strong, Seen	28.72	41.94	55.55	56.41
Weak, Unseen	33.81	45.93	56.41	56.72
Strong, Unseen	29.14	41.91	54.86	53.26
Ministral-8B (Judge trained on Strong)				
Weak, Seen	33.87	45.05	60.60	62.25
Strong, Seen	28.72	43.31	64.69	67.30
Weak, Unseen	33.81	42.62	57.15	58.82
Strong, Unseen	29.14	43.90	59.15	60.86
Ministral-8B (Continual, $\beta = 0.1$)				
Weak, Seen	33.87	–	62.11	–
Strong, Seen	28.72	–	60.43	–
Weak, Unseen	33.81	–	54.67	–
Strong, Unseen	29.14	–	53.13	–
Ministral-8B (Continual, $\beta = 1.0$)				
Weak, Seen	33.87	–	59.24	–
Strong, Seen	28.72	–	58.51	–
Weak, Unseen	33.81	–	55.28	–
Strong, Unseen	29.14	–	54.84	–
Mistral-24B (Judge trained on Weak)				
Weak, Seen	41.00	52.18	76.57	76.90
Strong, Seen	37.69	45.34	72.16	71.94
Weak, Unseen	40.75	47.49	68.56	71.41
Strong, Unseen	38.03	46.57	65.36	65.21
Mistral-24B (Judge trained on Strong)				
Weak, Seen	41.00	47.55	73.75	75.69
Strong, Seen	37.69	50.07	79.12	81.31
Weak, Unseen	40.75	45.85	66.30	68.52
Strong, Unseen	38.03	47.70	69.73	71.14
Mistral-24B (Continual, $\beta = 0.1$)				
Weak, Seen	41.00	–	73.70	–
Strong, Seen	37.69	–	73.80	–
Weak, Unseen	40.75	–	64.45	–
Strong, Unseen	38.03	–	62.37	–
Mistral-24B (Continual, $\beta = 1.0$)				
Weak, Seen	41.00	–	78.22	–
Strong, Seen	37.69	–	75.45	–
Weak, Unseen	40.75	–	66.83	–
Strong, Unseen	38.03	–	66.08	–

Table 2: *Judge's Consistent Accuracy.* Columns represent different judge training configurations: Base (zero-shot), SFT, DPO, and SFT+DPO. Each model is presented in multiple blocks: one for judges trained on Weak data (J_{Weak}), one for judges trained on Strong data (J_{Strong}), and additional blocks for continual training runs ($J^{\beta}_{\text{weak}\rightarrow\text{strong}}$) with $\beta \in \{0.1, 1.0\}$. Rows correspond to evaluation splits, defined by the source of responses (Weak or Strong) and the novelty of questions (Seen or Unseen). Base (zero-shot) values are repeated across all blocks to facilitate direct comparison between different judge training strategies.

Base Judge LLM	Weak Response Dataset	Strong Response Dataset
Ministral-8B, Mistral-Small-24B	Gemma2-9B, Qwen2-7B, Llama3.1-8B	Qwen2.5-7B, Gemma3-12B, Llama3.3-70B
Llama3.1-8B	Gemma2-9B, Qwen2-7B, Ministral-8B	Qwen2.5-7B, Gemma3-12B, Mistral-Small-24B

Table 3: Overview of training data composition on a per-base LLM basis. To mitigate bias from the difficulty of evaluating self-generated responses, we avoid training judge models on their own responses. This produces per-judge training datasets composed of different generator responses.