RESTRAIN: From Spurious Votes to Signals — Self-Driven RL with Self-Penalization

Zhaoning Yu[†,1,*], **Will Su**[†,4], **Leitian Tao**[3], **Haozhu Wang**[2], **Aashu Singh**[4], **Hanchao Yu**[4], **Jianyu Wang**[4], **Hongyang Gao**[1], **Weizhe Yuan**[2,5], **Jason Weston**[2,5], **Ping Yu**[‡,2], **Jing Xu**[‡,2]

[1]Iowa State University, [2]FAIR at Meta SuperIntelligence Lab, [3]UW–Madison, [4]Meta, [5]NYU
[*]Work done during an internship at Meta, [†]Joint first author, [‡]Joint last author

Reinforcement learning with human-annotated data has boosted chain-of-thought reasoning in large reasoning models, but these gains come at high costs in labeled data while faltering on harder tasks. A natural next step is experience-driven learning, where models improve without curated labels by adapting to unlabeled data. We introduce REinforcement learning with Self-resTRAINt training (**RESTRAIN**), a self-penalizing RL framework that converts the absence of gold labels into a useful learning signal. Instead of overcommitting to spurious majority votes, RESTRAIN exploits signals from the model's entire answer distribution: penalizing overconfident rollouts and low-consistency examples while preserving promising reasoning chains. This self-penalization mechanism integrates seamlessly into policy optimization methods such as GRPO, enabling continual self-improvement without supervision. On challenging reasoning benchmarks, RESTRAIN delivers large gains using only unlabeled data. With Qwen3-4B-Base and OctoThinker Hybrid-8B-Base, it boosts Pass@1 by up to +**140.7% on AIME25**, +**36.2% on MMLU_STEM**, and +**19.6% on GPQA-Diamond**, nearly matching gold-label training while using no gold labels. These results demonstrate that RESTRAIN establishes a scalable path toward stronger reasoning without gold labels.
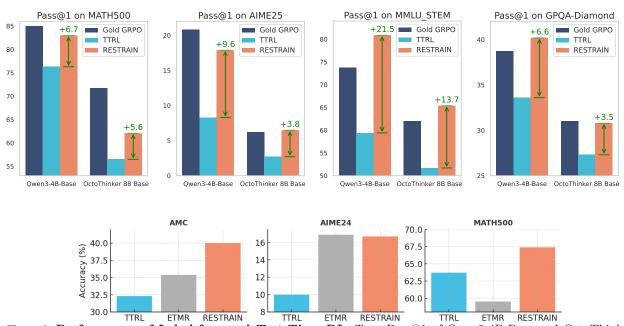
∞ Meta

Figure 1 Performance of Label-free and Test-Time RL. Top: Pass@1 of Qwen3-4B-Base and OctoThinker Hybrid-8B-Base trained on DAPO-14k-MATH without gold label. RESTRAIN outperforms TTRL and nearly matches the Gold-label GRPO upper bound, even surpassing it on MMLU-STEM and GPQA-Diamond. Bottom: Test-time training Llama3.1-8B-Instruct using unlabeled test data from AIME24, AMC23, and MATH500, reporting Pass@1 accuracy. RESTRAIN significantly outperforms TTRL and ETMR, especially on AMC and MATH500.

1   Introduction

Recent advances in LLMs (Guo et al., 2025; Jaech et al., 2024; Yang et al., 2025) show that Reinforcement Learning (RL) with human-annotated data and verifiable rewards (RLVR) greatly enhances long chain-of-thought reasoning (Wei et al., 2022), achieving strong performance on challenging benchmarks. Yet RLVR remains limited: it depends on ever-growing quantities of high-quality labeled data. Achieving superhuman performance, models will eventually need to operate in environments where even humans lack definitive answers and cannot offer reliable feedback on outputs. In these situations, models must develop the ability to self-improve without direct supervision. This motivates exploring RL on unlabeled data, where progress arises from self-improvement rather than curated labels, with large external corpora serving as a training signal (Zuo et al., 2025). In this work, we study RL in an unsupervised setting to advance reasoning generalization.

A central challenge in enabling self-improvement without labeled data is how a model can generate its own learning signals. One natural direction is self-rewarding methods, where the model generates its own reward signals—for instance, ranking or scoring its rollouts based on its own judgments (Yuan et al., 2024). While these methods remove the dependence on gold labels, evidence remains limited that such methods consistently improve performance on complex reasoning tasks. A second line of work leverages the model's internal agreement, such as using majority voting across multiple rollouts (Zuo et al., 2025; Shafayat et al., 2025; Liu et al., 2025a; Prasad et al., 2024). Yet this approach suffers from reliability and robustness issues that can cause model training collapse: models frequently generate responses with low self-consistency or low confidence across multiple attempts, and for challenging reasoning tasks, the majority-voted answer itself can be systematically flawed. In such cases, minority rollouts can contain the correct solution (Stahlberg and Byrne, 2019; Stahlberg et al., 2022), but these are ignored when overconfident spurious majorities dominate. Training on such distorted reward signals limits scalability as task diversity and complexity increase. The key challenge, therefore, is not merely generating self-derived rewards, but ensuring that they provide robust signals that drive genuine reasoning improvement.

To address this gap, we introduce RESTRAIN, a framework for self-driven RL with self-penalization. Instead of relying on gold labels or external supervision, RESTRAIN leverages the model's own predictions by (1) considering all predicted answers rather than only majority votes, (2) penalizing low-confidence rollouts with negative advantages, and (3) down-weighting low-agreement prompts with fragile majority votes. By integrating self-penalization directly into the RL objective, RESTRAIN turns the absence of labels into rollout-level and prompt-level learning signals. We evaluate RESTRAIN on two base models and two tasks across six benchmarks. Notably, RESTRAIN raises Pass@1 by 140.7% on AIME25, 36.2% on MMLU_STEM, and 19.6% on GPQA-Diamond. Even more striking, its performance nearly matches gold-label supervision—lagging by only 0.4 points. These results establish RESTRAIN as a scalable approach to self-driven RL, pushing reasoning models beyond supervised limits.

2   RESTRAIN

We introduce the main ideas of RESTRAIN below and in Figure 3.

Preliminaries   We adopt Grouped Relative Policy Optimization (GRPO) (Shao et al., 2024) as our main RL algorithm. GRPO optimizes a policy π_θ by sampling n rollouts per prompt x with gold label y, and updating with a PPO-style objective against a fixed reference policy π_{ref}, using a group-mean baseline for variance reduction. For each rollout y_i, we denote by reward $r_i = R(y_i, y|x)$ with advantage A_i. The GRPO objective for each prompt x with gold label y is:

$$\mathcal{L}_{\text{GRPO}}(x, y; \theta) = \frac{1}{n}\sum_{i=1}^{n} \min\Big(\rho_i(\theta)\, A_i,\ \text{clip}\big(\rho_i(\theta), 1 - \epsilon, 1 + \epsilon\big) A_i\Big)\ -\ \beta \mathbb{D}_{KL}\left[\pi_\theta \| \pi_{\text{ref}}\right] \tag{1}$$

2.1   Pseudo-label weighting

In unsupervised settings without gold labels, a model can give multiple predictions for a given prompt x, regardless of their correctness. Figure 2 reports accuracies on model predicted answers for the Qwen3-4B-Base

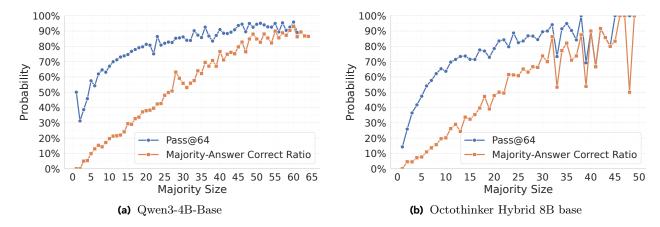**(a)** Qwen3-4B-Base **(b)** Octothinker Hybrid 8B base

Figure 2 Majority-Vote Reliability. Pass@64 and the majority-voted accuracy over 64 samples are compared on the DAPO-MATH dataset for Qwen3-4B-Base (left) and OctoThinker Hybrid-8B-Base (right). The large gap between Pass@64 and majority-vote shows that correct answers often diverge from majority votes. Accuracy also drops sharply when the majority size is small, revealing that majority votes can carry spurious signals. These observations motivate our self-penalizing framework, which seeks robust promising reasoning paths beyond unreliable majority votes.

model (a) and the OctoThinker Hybrid-8B-Base model (b) on the DAPO-MATH dataset. Although the Majority Correct Ratio rises with the majority vote size (number of solutions that agree), there remains a large gap between Pass@64 and the majority correct ratio, revealing that majority votes can be spurious and often fail to capture the true answer. To bridge this gap, we introduce a pseudo-label weighting scheme. Rather than collapsing all probability mass onto the most frequent answer (majority voting) or distributing it uniformly across candidates, our method assigns weights proportional to the observed vote counts. This produces a consensus distribution that down-weights spurious low-frequency answers while avoiding the brittleness of requiring consensus, providing the foundation for our self-penalization framework.

Construction Given a prompt x, we draw n rollouts $\{y_i\}_{i=1}^n \sim \pi_\theta(\cdot \mid x)$ and collect the set of unique answers $\{a_j\}_{j=1}^m$ with counts c_j. We treat each a_j as a pseudo label and compute the weighted loss as follows:

$$\mathcal{L}_{\text{GRPO}}(x;\theta) = \sum_{j=1}^m w_j \cdot \mathcal{L}_{\text{GRPO}}(x, a_j; \theta) \tag{2}$$

where w_j is a pseudo-label weight obtained by applying a monotonic function g to frequency $f_j = \frac{c_j}{n}$:

$$w_j = \frac{g(f_j)}{\sum_{\ell=1}^m g(f_\ell)}. \tag{3}$$

We use a Gaussian function centered at the $k \in [0, 1]$ with bias $\sigma > 0$ as our shaping function g.

Interpretation Equation 3 prevents collapse to a single majority answer while penalizing spurious low-frequency predictions through a form of *soft selection* over answer frequencies: predictions with higher frequencies receive proportionally larger weights. The skewness of this weighting is controlled by the monotonic shaping function $g(\cdot)$: a steeper g concentrates probability mass on high-frequency answers, whereas a smoother g distributes weight more broadly across answers.

2.2 Negative rollout penalization

Existing methods (Zuo et al., 2025; Shafayat et al., 2025) often rely on the majority-voted answer being correct, making low self-consistency regions prone to spurious training signals. Our proposed pseudo-label weighting subsection 2.1 instead leverages control of Pass@n: if any rollout is correct, it provides a valid positive signal, yielding more robust learning under weak consensus. However, when the majority size is very
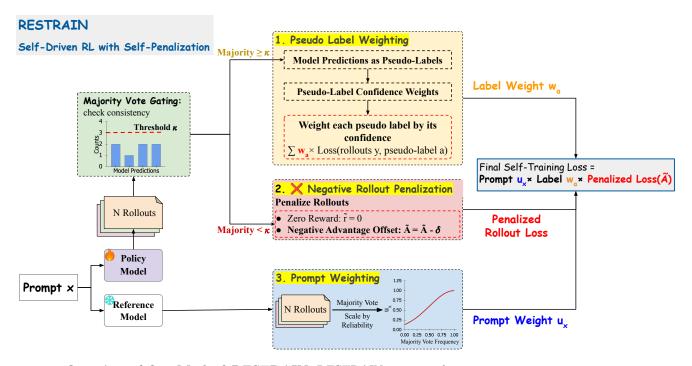
Figure 3 Overview of Our Method RESTRAIN: RESTRAIN consists of 3 core components: **1. Pseudo Label Weighting** which takes into account all possible model-predicted answers as candidate pseudo-labels when calculating final losses. **2. Negative Rollout Penalization** which penalizes rollouts with very low confidence by setting zero reward and applying negative advantage offsets to the losses. **3. Prompt Weighting** which downweights entire examples where the reference model predicts with low self-consistency.

low, Pass@n often degrades because the model may generate no correct rollouts at all. As shown in Figure 2, prompts with very low majority size correspond to unreliable supervision where no answer can be confidently trusted. To handle such cases, we introduce negative rollout penalization, which assumes all responses are incorrect and applies a uniform negative offset. This reduces explicitly penalizing all rollouts and encourages the model to explore alternative reasoning paths.

Construction   Consider the GRPO loss term $\mathcal{L}_{\mathrm{GRPO}}(x, a_j; \theta)$ associated with pseudo-label a_j. For each rollout y_i, denote by $r_{i,j} = R(y_i, a_j)$ the reward and $A_{i,j}$ the corresponding advantage. Let $M(x) = \max_j c_j$ denote the majority count of prompt x, where c_j is the vote count for label a_j.

When the self-consistency is low ($M(x) < \kappa$), we treat all candidate answers as unreliable, zero out their rewards, and apply a uniform penalty $\delta \geq 0$ to the advantages of all rollouts.

$$\tilde{r}_{i,j} = \begin{cases} r_{i,j} & \text{if } M(x) \geq \kappa \\ 0 & \text{if } M(x) < \kappa \end{cases}, \qquad \tilde{A}_{i,j} = \begin{cases} A_{i,j} & \text{if } M(x) \geq \kappa \\ A_{i,j} - \delta & \text{if } M(x) < \kappa \end{cases} \tag{4}$$

In PPO/GRPO objectives, this means that all model predictions with $M(x) < \kappa$ contribute only negative updates, penalizing all rollouts with low self-consistency. This discourages reinforcement of spurious majority votes and steers the model away from unreliable reasoning paths.

2.3   Prompt-level weighting

Previous penalizing schemes operate at a rollout level. In addition, we introduce a prompt-level penalty. For some prompts, the model exhibits high uncertainty, while for others it produces highly consistent responses. To account for this variation, we scale the update for each prompt by a *fixed* weight that reflects the model's confidence: low-confidence prompts receive smaller updates, and high-confidence prompts receive larger updates. To prevent spurious feedback loops (e.g., inflated confidence during training), these weights are computed once using a frozen base model and kept constant thereafter.

Construction For each prompt x, we sample n rollouts from the reference policy π_{ref} and compute the majority count c_{ref}. We define the prompt weight again using the monotonic function $g(\cdot)$:

$$u_x = g(\frac{c_{\mathrm{ref}}}{n}) \tag{5}$$

We apply u_x to each prompt for all training updates. Unlike pseudo-label weights, prompt-level weights are precomputed offline and remain fixed during the RL training. In Appendix E, we will show offline-computed prompt-level weights outperform online variants that are dynamically updated during training.

Final RESTRAIN loss Jointly applying pseudo-label weights w_j from Equation 3 and negative rollout penalization $\tilde{A}_{ij}$ from Equation 4, and the prompt-level weight u_x from Equation 5, we derive our final RESTRAIN loss:

$$\mathcal{L}_{RESTRAIN}(x;\theta) = u_x \sum_{j=1}^{m} w_j \underset{\Downarrow \text{ expand}}{\tilde{L}_{GRPO}(x, a_j; \theta)} \tag{6}$$

$$\tilde{L}_{GRPO}(x, a_j; \theta) = -\frac{1}{n} \sum_{i=1}^{n} \min\left(\rho_i(\theta)\,\tilde{A}_{i,j},\ \mathrm{clip}(\rho_i(\theta), 1-\epsilon, 1+\epsilon)\,\tilde{A}_{i,j}\right)$$
$$- \beta\,\mathbb{D}_{\mathrm{KL}}[\pi_\theta \,\|\, \pi_{\mathrm{ref}}] \tag{7}$$

3 Experimental Setup

Datasets We evaluate the effectiveness of RESTRAIN on two mathematical and reasoning tasks:

- **DAPO-14k-MATH**: We adopt the processed DAPO derived from DAPO-Math-17k (Yu et al., 2025b) which deduplicates prompts and standardizes the formatting of both prompts and reference answers. From this release, we further exclude 3k Chinese language prompts and use 14k English language prompts as our training split, with no further modifications.

- **Synthetic S1k**: A 5k synthetic reasoning dataset from CoT-Self-Instruct (Yu et al., 2025a). Starting from the curated S1k seed set (Muennighoff et al., 2025), Yu et al. (2025a) prompt LLMs to reason step by step and then synthesize new instructions of similar difficulty. Each synthetic example contains both a novel question and a verifiable target answer produced generated by LLM. This dataset complements existing curated math datasets by providing a fully synthetic yet diverse set of reasoning problems, and allows us to systematically test our method under a purely synthetic data generation setting.

Base Models To evaluate the generalizability of our method across different backbone models, we conduct experiments using the following models of various model families and sizes: we use Qwen3-4B-Base and Octothinker Hybrid 8B base (Wang et al., 2025b), which is midtrained from Llama3.1-8B (Dubey et al., 2024), as well as the Llama3.1-8B-Instruct model.

Benchmarks Our benchmark suite comprises six publicly available benchmarks spanning mathematics (four) and science (two). (1) MATH-500 (Hendrycks et al., 2021), (2) AIME25 (Li et al., 2024), (3) OlympiadBench (math subset) (Yang et al., 2024), we use the mathematics portion only. (4) Minerva_math (Yang et al., 2024): the mathematics split from the Minerva quantitative-reasoning suite. (5) MMLU_STEM (Yang et al., 2024), (6) GPQA-Diamond (Yang et al., 2024).

Metrics We evaluate with averaged Pass@1 (Chen et al., 2021) across six benchmarks, sampling 16 predictions per question using a temperature of 0.6 and a top-p value of 0.95 and averaging their 16 Pass@1 accuracies. We use the official evaluation codebase of Qwen2.5-math (Yang et al., 2024).

Baselines We compare RESTRAIN against three recent label-free RLVR methods:

- *TTRL* (Zuo et al., 2025): treats the majority-voted answer as the single pseudo-label, reinforcing it during RL updates. This makes training heavily dependent on the majority being correct, and thus vulnerable to spurious votes.

Table 1 On DAPO-14k-MATH: RESTRAIN outperforms all unsupervised baselines. All Pass@1 results(%) are averaged over 16 seeds. The best results are highlighted in **bold**. RESTRAIN outperforms existing baselines without access to gold labels for both Qwen3-4-Base and Octothinker Hybrid-8B Base. In particular, Qwen4-B-Base trained without access to gold labels using RESTRAIN nearly matches the performance of GRPO with gold labels.

Model	MATH500	AIME25	Olym.	Minerva.	MMLU.	GPQA-d.	Avg. ↑
Qwen3-4B-Base	68.0	7.9	35.4	26.0	58.3	32.2	38.0
w/ access to gold labe							
GRPO	85.0	20.8	50.1	40.1	73.7	38.7	51.4
w/o access to gold label							
TTRL	76.3	8.3	39.6	35.9	59.4	33.6	42.2
SRT (easy prompt)	77.8	7.9	39.7	36.3	60.5	34.9	42.8
SRT (offline majority label)	76.9	12.0	39.8	34.2	59.4	34.5	43.1
RESTRAIN (Ours)	**83.0**	**17.9**	**47.0**	**36.5**	**80.9**	**40.2**	**51.0**
Δ(RESTRAIN - TTRL)	+6.7	+9.6	+7.4	+0.6	+21.5	+6.6	+8.8
	↑8.8%	↑115.7%	↑18.7%	↑1.7%	↑36.2%	↑19.6%	↑20.9%
OctoThinker Hybrid-8B-Base	29.8	0.8	12.1	9.3	8.6	24.6	19.2
w/ access to gold label							
GRPO	71.7	6.2	35.2	31.3	62.0	31.0	39.6
w/o access to gold label							
TTRL	56.5	2.7	23.2	22.1	51.7	27.3	30.6
SRT (offline majority label)	58.5	1.7	23.6	27.6	56.4	29.3	32.8
RESTRAIN	**62.1**	**6.5**	**24.0**	26.1	**65.4**	**30.8**	**35.8**
Δ(RESTRAIN - TTRL)	+5.1	+3.8	+0.8	+4.0	+13.7	+3.5	+5.2
	↑9.0%	↑140.7%	↑3.4%	↑18.1%	↑26.5%	↑12.8%	↑17.0%

- *Self-Rewarded Training (SRT)* (Shafayat et al., 2025) proposes two heuristics to mitigate majority-vote collapse:
  - Offline majority label: computes majority votes offline, reducing—but not eliminating—the risk of rewarding self-consistency instead of correctness.
  - Easy prompts: filters training to "easy" prompts with high vote ratios, discarding low-consensus prompts that often contain valuable but underrepresented reasoning paths.
- *Entropy-based Test-Time Reinforcement Learning (ETTRL)* (Liu et al., 2025a) is an entropy-based strategy that improves test-time reinforcement learning for LLM reasoning. We include ETTRL as a baseline only in our Test-Time RL experiments. As the original paper reports results only for test-time training (TTT) and no public implementation is available, we do not extend ETTRL to large-scale label-free RL training (e.g., DAPO-MATH or synthetic S1k).

4 Main Results

RESTRAIN outperforms unsupervised baselines. On DAPO-MATH-14k (Table 1), RESTRAIN - training without gold labels - substantially outperforms existing unsupervised baselines TTRL and SRT. It achieves 51.0%, compared to TTRL (42.2%, +8.8 pp), Offline Majority Label (43.1%, +7.9 pp), and Easy Prompts (42.8%, +8.2 pp). A consistent trend appears on the 5k synthetic corpus (Table 2), where RESTRAIN remains the strongest label-free approach, exceeding the next-best baseline by at least 7.7 pp on average. Notably, when excluding the two science-heavy benchmarks (MMLU_STEM and GPQA-Diamond), RESTRAIN nearly closes the gap with distilling the supervised "reference target" by Qwen3-4B instruct : 45.9% vs. 47.7%, a margin of only 1.8 pp. On OctoThinker Hybrid-8B, we observe the same effect: RESTRAIN consistently

Table 2 **Synthetic S1k dataset: Our RESTRAIN outperforms all unsupervised baselines.** All Pass@1 results(%) are averaged over 16 seeds. The best results are highlighted in **bold**. When training from Qwen3-4B-Base model on synthetic reasoning tasks without gold label, our method RESTRAIN also outperforms existing unsupervised baselines by 18%.

Model	MATH500	AIME25	Olym.	Minerva.	MMLU.	GPQA-d.	Avg. ↑
Qwen3-4B-Base	68.0	7.9	35.4	26.0	58.3	32.2	38.0
w/ access to Qwen3-4B label							
GRPO	83.7	18.9	48.4	39.7	83.6	43.5	53.0
w/o access to Qwen3-4B label							
TTRL	76.0	9.2	39.3	35.9	57.6	32.8	41.8
SRT (easy prompt)	76.4	8.1	39.6	34.8	57.5	33.0	41.6
SRT (offline majority label)	75.8	10.4	39.2	33.1	57.1	33.1	41.4
RESTRAIN (Ours)	**81.7**	**20.0**	**45.5**	**36.5**	**73.4**	**40.0**	**49.5**
Δ(RESTRAIN - TTRL)	+5.7	+10.8	+6.2	+0.6	+15.8	+7.2	+7.7
	↑7.5%	↑117.4%	↑15.8%	↑1.7%	↑27.4%	↑22.0%	↑18.4%

surpasses unsupervised baselines TTRL and SRT by large margins. These results underscore the power of self-driven RL with self-penalization, showing that label- and prompt-level penalties transform noisy unlabeled training into signals strong enough to rival gold-label supervision.

RESTRAIN almost reaches the gold-label upper bound on Qwen3-4B-Base In Table 1, we treat the *Gold-label* setting as an empirical upper bound for label-free RLVR, achieving an average accuracy of 51.4%. Remarkably, RESTRAIN reaches 51.0%, trailing by only 0.4 pp—essentially matching supervised GRPO without using labels. Even more striking, RESTRAIN surpasses the gold-label GRPO on MMLU_STEM, scoring **80.9%** vs. 73.7% and on GPQA-Diamond, **40.2%** vs. 38.7%. This suggests strong cross-domain generalization without gold-labels despite being trained solely on the math-focused DAPO-14k dataset. We hypothesize that gold-label supervision encourages overfitting to domain-specific patterns, limiting transfer to science tasks, while RESTRAIN—through self-penalization—relies on distributional signals rather than gold answers, reducing overfitting and preserving generalization across domains.

RESTRAIN outperforms other Test Time RL Training methods Test Time RL training focuses on the adaptation to test-time data. We compare our method with recent test-time RL methods like TTRL (Zuo et al., 2025) and Entropy-fork Tree Majority Rollout (ETMR) (Liu et al., 2025a) on LLama3.1-8B-Instruct model, following the same setup as in Liu et al. (2025a), with all methods trained on test prompts without access to gold labels. In Table 3, our approach achieves consistent improvements across challenging math reasoning benchmarks.

Table 3 **Comparing RESTRAIN v.s. Two Test Time RL Training Methods: TTRL and ETMR on Llama3.1-8B-Instruct.** All results(%) are by greedy decoding following Liu et al. (2025a). RESTRAIN also outperforms the existing test-time scaling method by 11%.

Test-Time Method	AIME24	AMC23	MATH500	Avg. ↑
TTRL	10.0	32.3	63.7	35.3
ETMR (Liu et al., 2025a)	**16.9**	35.4	59.5	37.3
RESTRAIN (Ours)	16.7	**40.0**	**67.4**	**41.4**
Δ(RESTRAIN - ETMR)	−0.2	+4.6	+7.9	+4.1

It surpasses TTRL and ETMR on AMC23 and MATH-500 by margins of +13.0% and +13.3%, respectively, yielding an overall +11.0% gain in average accuracy. These results demonstrate that our method can also scale very effectively at test time.

RESTRAIN can effectively prevent model collapse Figure 4 shows the averaged Pass@1 on MATH500 across multiple unsupervised methods. The base model is Qwen3-4B-Base, and all methods are trained on the 14k DAPO dataset. We observe that TTRL improves at first but quickly collapses after 50 steps. In contrast, our method RESTRAIN prevents this sudden collapse and keeps training stable throughout. We attribute this stability to RESTRAIN, which does not exclusively reward the majority-vote answer; instead, it assigns soft weights to all distinct answers in proportion to their empirical f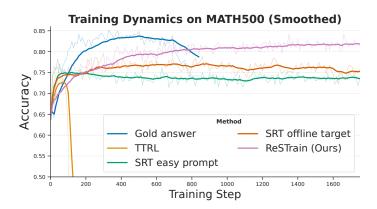requencies. This frequency-aware weighting smooths the learning signal, curbs overconfident updates, and mitigates sudden collapse.

Figure 4 RESTRAIN has more stable training dynamics. In contrast to TTRL, our method RESTRAIN steadily improves model performances.
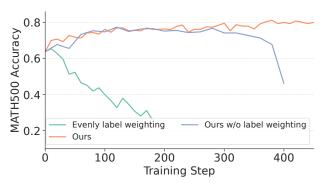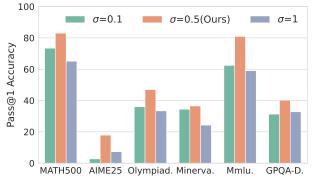
5 Ablation Study

Effectiveness of each component in our RESTRAIN Table 4 presents the impact of each component of our proposed RESTRAIN. The removal of pseudo-label weighting results in the most substantial performance degradation because training collapses quickly. Omitting negative rollout penalization also hurts performance, reducing the average score from 51.0 to 42.1. Finally, removing prompt-level weighting leads to a more modest performance decrease, yet still validates its positive contribution to the model. Taken together, these results show that all components are necessary for stable and effective unsupervised training.

Table 4 Each component in RESTRAIN is important. Each row represents the model's performance with one component removed. The best results are highlighted in bold.

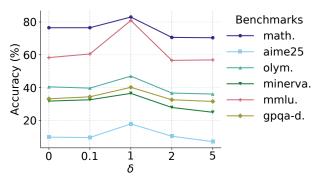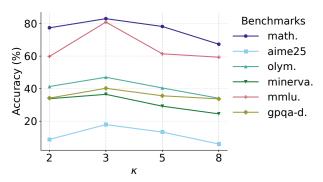| Model	MATH500	AIME25	Olym.	Minerva.	MMLU.	GPQA-d.	Avg. ↑
RESTRAIN	**83.0**	17.9	**47.0**	36.5	**80.9**	**40.2**	**51.0**
(-) Pseudo-label weighting	67.3	6.0	34.1	24.5	59.3	33.7	37.5
(-) Negative Rollout Penalization	77.3	9.6	39.9	36.2	56.4	33.0	42.1
(-) Prompt-level weighting	82.7	**18.1**	46.7	**37.8**	63.8	37.0	47.7

(a) Accuracy curve on MATH500 benchmark.

(b) Performance for different σ values.

Figure 5 Effect of Pseudo-Label Weighting. Pseudo-label Weighting prevents training collapse, and the hyperparameter σ can control the "skewness" of the pseudo-label weight distribution.

(a) Performance of our RESTRAIN with different negative advantage offset δ.

(b) Performance of our RESTRAIN with different majority count threshold κ, the number of rollout in our experiment is 16.

Figure 6 Effect of Pseudo-Label Weighting. Model performance is sensitive to hyperparameters in Negative Rollout Penalization.

Pseudo-label weighting is crucial to avoid training collapse   To assess the impact of our pseudo-label weighting module on training and performance, we run two ablation experiments. In the first experiment, we apply prompt-level weighting, negative rollout penalization, and use the majority vote answer as a pseudo label. In the second experiment, we replace the frequency-based soft weights with uniform weights over all targets for each prompt. Figure 5a reports the outcome: without pseudo-label weighting, training becomes unstable and eventually fails. Uniform weighting performs even worse, accelerating degradation and leading to an earlier collapse. This shows that merely considering all targets is insufficient—low-frequency pseudo-labels are typically erroneous/noisy, and assigning them the same weight as high-frequency (likely correct) pseudo-labels can steer the model in the wrong direction. In contrast, frequency-based soft weighting suppresses rare noise and stabilizes training.

Hyperparameter σ in Pseudo-label Weighting σ controls the "skewness" or concentration of the prompt-level weight distribution. When σ is very small, the weighting approaches a step-like function that sharply distinguishes majority from minority answers, effectively behaving like hard majority voting and largely ignoring less frequent responses. In contrast, a large σ produces a broad, flat distribution, leading to softer, more evenly spread weights across answers. From Figure 5b, a smaller σ ($\sigma = 0.1$) underperforms because it gives too much influence to noisy, infrequent answers. Conversely, a larger σ ($\sigma = 1$) is also suboptimal as it fails to leverage valuable signals from correct minority responses. Thus, $\sigma = 0.5$ provides the best balance, effectively filtering noise while retaining the full distributional signal from the model's outputs.

Hyperparameters in Negative Rollout Penalization   Figure 6a ablates the negative advantage offset δ, which dictates the magnitude of the penalty applied to low-consensus rollouts. The results demonstrate that the model's performance is sensitive to δ. With the penalty disabled ($\delta = 0$), the model simply ignores those low-confidence prompts. Performance is similar to the ablation without Negative Rollout Penalization (Table 4), indicating that simply discarding low-confidence prompts does not hinder training. The best accuracy occurs at $\delta = 1$, suggesting that a moderate penalty effectively discourages the model from generating noisy, low-confidence outputs, thereby stabilizing the training signal and enhancing reasoning capabilities. When the penalty magnitude is increased further to $\delta=2$ and $\delta=5$, a consistent and sharp decline in accuracy is observed across all benchmarks. This indicates that an excessively large penalty is detrimental, likely because it over-penalizes the model and may suppress potentially correct, albeit low-frequency, reasoning paths.

Figure 6b varies the majority size threshold κ for triggering the negative penalty; the penalty is applied if the count of the most frequent answer is less than κ. The data reveals a similar trend where performance is suboptimal at both low and high values of κ, peaking at a value of $\kappa = 3$. A threshold that is too lenient ($\kappa = 2$) fails to penalize many noisy, low-confidence training examples, thus limiting performance improvement. Conversely, a threshold that is too strict ($\kappa = 5$ or 8) suppresses potentially valid reasoning paths in outputs with moderate consensus and causes a significant drop in accuracy. Therefore, the threshold value strikes

a crucial balance, effectively filtering unreliable training signals without excessively restricting the model's learning process.

6   Related Work

RL with Verifiable Rewards   RL has shown great promise in improving LLMs, as demonstrated by the success of RL from human feedback (RLHF) and from AI feedback (RLAIF), which aligns model responses with human preferences (Lee et al., 2023; Ouyang et al., 2022; Liu et al., 2025b; Yue et al., 2025). More recently, reinforcement learning with verifiable rewards (Gao et al., 2024; Shao et al., 2024; Guo et al., 2025; Yang et al., 2025; Wen et al., 2025; Song et al., 2025; Team et al., 2025; Fatemi et al., 2025; Wang et al., 2025a; Li et al., 2025b) has been developed to further enhance reasoning capabilities in domains such as mathematics and code. Despite its promise, RLVR is largely limited to settings where a verifiable gold label or exhaustive validators exist, and its outcome-based rewards may limit generalization to tasks that are out of distribution.

Unsupervised Reward Estimation   Accurately capturing reward signals without relying on human labels has been the focus of several recent studies. Early work like STaR (Zelikman et al., 2022) relies on repeated outcome evaluation. Self-Rewarding LMs (Yuan et al., 2024) explores using LLM-as-a-Judge to provide its own rewards to do self-training. SCPO (Prasad et al., 2024) introduced self-consistency as an alternative to human-annotated rewards, demonstrating its effectiveness in improving reasoning tasks through (iterative) DPO training. Building on these ideas, TTRL (Zuo et al., 2025) further explored self-consistency signals in an online setting, which treats the majority-voted answer as a pseudo label and leverages the GRPO algorithm (Shao et al., 2024) to update the model. However, TTRL was found to suffer from overconfidence issues, resulting in mode collapse. To address this, SRT (Shafayat et al., 2025) proposed using offline-generated labels and curriculum learning; ETTRL (Liu et al., 2025a) proposed an entropy-based mechanism that enhances the balance between exploration and exploitation, thus mitigating overconfidence and improving overall performance; EVOL-RL (Zhou et al., 2025) introduced novelty reward to increase exploration. Other unsupervised methods derive intrinsic rewards from a model's internal feedback—Reinforcement Learning from Internal Feedback (RLIF). For example, some approaches measure the model's output certainty, using metrics like token- and trajectory-level entropy (Prabhudesai et al., 2025; Agarwal et al., 2025) or self-confidence (Li et al., 2025a). Along these lines, Intuitor (Zhao et al., 2025) utilizes a model's internal confidence termed "self-certainty" as its sole intrinsic reward. Another method, EMPO (Zhang et al., 2025a), uses clustering to extract semantic entropy across multiple rollouts and compute corresponding advantages. Zhang et al. (2025b) theoretically analyzes internal equivalence among RLIF methods and claims that the prior of the base model causes training collapse.

Unlikelihood Penalization   Unlikelihood training is a widely adopted technique in neural text generation to penalize undesirable outputs. (Welleck et al., 2019) reduces the probability of specific "negative candidate" tokens. (Li et al., 2019) later employed this approach to improve logical consistency, demonstrating its effectiveness as a general framework for mitigating known biases in dialogue by penalizing a carefully selected set of negative tokens at each generation step. More recently, NSR (Zhu et al., 2025) extended this principle from the neural text generation model to LLMs post-training with their Negative Sampling Rejection (NSR) method. In the context of RLVR, they show that penalizing entire negative trajectories consistently improves performance, preserves generation diversity, and promotes generalization over the base model.

7   Conclusion

In this paper, we propose RESTRAIN, a self-penalizing reinforcement learning framework that transforms the absence of gold labels into a learning signal, enabling models to self-improve without gold labels. By (i) weighting all predicted targets rather than only the majority, (ii) penalizing low-confidence rollouts within the policy objective, and (iii) discounting prompts with low self-consistency, RESTRAIN enables robust self-improvement and mitigates the training collapse of majority-vote heuristics. Empirically, it delivers more stable optimization and stronger generalization on challenging reasoning tasks like math and science.

References

Shivam Agarwal, Zimin Zhang, Lifan Yuan, Jiawei Han, and Hao Peng. The unreasonable effectiveness of entropy minimization in llm reasoning. *arXiv preprint arXiv:2505.15134*, 2025.

Mark Chen, Jerry Tworek, Heewoo Jun, Qiming Yuan, Henrique Ponde De Oliveira Pinto, Jared Kaplan, Harri Edwards, Yuri Burda, Nicholas Joseph, Greg Brockman, et al. Evaluating large language models trained on code. *arXiv preprint arXiv:2107.03374*, 2021.

Abhimanyu Dubey, Abhinav Jauhri, Abhinav Pandey, Abhishek Kadian, Ahmad Al-Dahle, Aiesha Letman, Akhil Mathur, Alan Schelten, Amy Yang, Angela Fan, et al. The llama 3 herd of models. *arXiv e-prints*, pages arXiv–2407, 2024.

Mehdi Fatemi, Banafsheh Rafiee, Mingjie Tang, and Kartik Talamadupula. Concise reasoning via reinforcement learning. *arXiv preprint arXiv:2504.05185*, 2025.

Jiaxuan Gao, Shusheng Xu, Wenjie Ye, Weilin Liu, Chuyi He, Wei Fu, Zhiyu Mei, Guangju Wang, and Yi Wu. On designing effective rl reward at training time for llm reasoning. *arXiv preprint arXiv:2410.15115*, 2024.

Daya Guo, Dejian Yang, Haowei Zhang, Junxiao Song, Ruoyu Zhang, Runxin Xu, Qihao Zhu, Shirong Ma, Peiyi Wang, Xiao Bi, et al. Deepseek-r1: Incentivizing reasoning capability in llms via reinforcement learning. *arXiv preprint arXiv:2501.12948*, 2025.

Dan Hendrycks, Collin Burns, Saurav Kadavath, Akul Arora, Steven Basart, Eric Tang, Dawn Song, and Jacob Steinhardt. Measuring mathematical problem solving with the math dataset. *arXiv preprint arXiv:2103.03874*, 2021.

Aaron Jaech, Adam Kalai, Adam Lerer, Adam Richardson, Ahmed El-Kishky, Aiden Low, Alec Helyar, Aleksander Madry, Alex Beutel, Alex Carney, et al. Openai o1 system card. *arXiv preprint arXiv:2412.16720*, 2024.

Harrison Lee, Samrat Phatale, Hassan Mansoor, Thomas Mesnard, Johan Ferret, Kellie Lu, Colton Bishop, Ethan Hall, Victor Carbune, Abhinav Rastogi, et al. Rlaif vs. rlhf: Scaling reinforcement learning from human feedback with ai feedback. *arXiv preprint arXiv:2309.00267*, 2023.

Jia Li, Edward Beeching, Lewis Tunstall, Ben Lipkin, Roman Soletskyi, Shengyi Huang, Kashif Rasul, Longhui Yu, Albert Q Jiang, Ziju Shen, et al. Numinamath: The largest public dataset in ai4maths with 860k pairs of competition math problems and solutions. *Hugging Face repository*, 13(9):9, 2024.

Margaret Li, Stephen Roller, Ilia Kulikov, Sean Welleck, Y-Lan Boureau, Kyunghyun Cho, and Jason Weston. Don't say that! making inconsistent dialogue unlikely with unlikelihood training. *arXiv preprint arXiv:1911.03860*, 2019.

Pengyi Li, Matvey Skripkin, Alexander Zubrey, Andrey Kuznetsov, and Ivan Oseledets. Confidence is all you need: Few-shot rl fine-tuning of language models. *arXiv preprint arXiv:2506.06395*, 2025a.

Tianjian Li, Yiming Zhang, Ping Yu, Swarnadeep Saha, Daniel Khashabi, Jason Weston, Jack Lanchantin, and Tianlu Wang. Jointly reinforcing diversity and quality in language model generations. *arXiv preprint arXiv:2509.02534*, 2025b.

Jia Liu, ChangYi He, YingQiao Lin, MingMin Yang, FeiYang Shen, ShaoGuo Liu, and TingTing Gao. Ettrl: Balancing exploration and exploitation in llm test-time reinforcement learning via entropy mechanism. *arXiv preprint arXiv:2508.11356*, 2025a.

Zichen Liu, Changyu Chen, Wenjun Li, Penghui Qi, Tianyu Pang, Chao Du, Wee Sun Lee, and Min Lin. Understanding r1-zero-like training: A critical perspective. *arXiv preprint arXiv:2503.20783*, 2025b.

Niklas Muennighoff, Zitong Yang, Weijia Shi, Xiang Lisa Li, Li Fei-Fei, Hannaneh Hajishirzi, Luke Zettlemoyer, Percy Liang, Emmanuel Candès, and Tatsunori Hashimoto. s1: Simple test-time scaling, 2025. https://arxiv.org/abs/2501.19393.

Long Ouyang, Jeffrey Wu, Xu Jiang, Diogo Almeida, Carroll Wainwright, Pamela Mishkin, Chong Zhang, Sandhini Agarwal, Katarina Slama, Alex Ray, et al. Training language models to follow instructions with human feedback. *Advances in neural information processing systems*, 35:27730–27744, 2022.

Mihir Prabhudesai, Lili Chen, Alex Ippoliti, Katerina Fragkiadaki, Hao Liu, and Deepak Pathak. Maximizing confidence alone improves reasoning. *arXiv preprint arXiv:2505.22660*, 2025.

Archiki Prasad, Weizhe Yuan, Richard Yuanzhe Pang, Jing Xu, Maryam Fazel-Zarandi, Mohit Bansal, Sainbayar Sukhbaatar, Jason Weston, and Jane Yu. Self-consistency preference optimization. *arXiv preprint arXiv:2411.04109*, 2024.

Sheikh Shafayat, Fahim Tajwar, Ruslan Salakhutdinov, Jeff Schneider, and Andrea Zanette. Can large reasoning models self-train? *arXiv preprint arXiv:2505.21444v1*, 2025.

Zhihong Shao, Peiyi Wang, Qihao Zhu, Runxin Xu, Junxiao Song, Xiao Bi, Haowei Zhang, Mingchuan Zhang, YK Li, Yang Wu, et al. Deepseekmath: Pushing the limits of mathematical reasoning in open language models. *arXiv preprint arXiv:2402.03300*, 2024.

Mingyang Song, Mao Zheng, Zheng Li, Wenjie Yang, Xuan Luo, Yue Pan, and Feng Zhang. Fastcurl: Curriculum reinforcement learning with progressive context extension for efficient training r1-like reasoning models. *arXiv e-prints*, pages arXiv–2503, 2025.

Felix Stahlberg and Bill Byrne. On nmt search errors and model errors: Cat got your tongue? *arXiv preprint arXiv:1908.10090*, 2019.

Felix Stahlberg, Ilia Kulikov, and Shankar Kumar. Uncertainty determines the adequacy of the mode and the tractability of decoding in sequence-to-sequence models. *arXiv preprint arXiv:2204.00471*, 2022.

Kimi Team, Angang Du, Bofei Gao, Bowei Xing, Changjiu Jiang, Cheng Chen, Cheng Li, Chenjun Xiao, Chenzhuang Du, Chonghua Liao, et al. Kimi k1. 5: Scaling reinforcement learning with llms. *arXiv preprint arXiv:2501.12599*, 2025.

Yiping Wang, Qing Yang, Zhiyuan Zeng, Liliang Ren, Liyuan Liu, Baolin Peng, Hao Cheng, Xuehai He, Kuan Wang, Jianfeng Gao, et al. Reinforcement learning for reasoning in large language models with one training example. *arXiv preprint arXiv:2504.20571*, 2025a.

Zengzhi Wang, Fan Zhou, Xuefeng Li, and Pengfei Liu. Octothinker: Mid-training incentivizes reinforcement learning scaling. *arXiv preprint arXiv:2506.20512*, 2025b.

Jason Wei, Xuezhi Wang, Dale Schuurmans, Maarten Bosma, Fei Xia, Ed Chi, Quoc V Le, Denny Zhou, et al. Chain-of-thought prompting elicits reasoning in large language models. *Advances in neural information processing systems*, 35:24824–24837, 2022.

Sean Welleck, Ilia Kulikov, Stephen Roller, Emily Dinan, Kyunghyun Cho, and Jason Weston. Neural text generation with unlikelihood training. *arXiv preprint arXiv:1908.04319*, 2019.

Liang Wen, Yunke Cai, Fenrui Xiao, Xin He, Qi An, Zhenyu Duan, Yimin Du, Junchen Liu, Lifu Tang, Xiaowei Lv, et al. Light-r1: Curriculum sft, dpo and rl for long cot from scratch and beyond. *arXiv preprint arXiv:2503.10460*, 2025.

An Yang, Baosong Yang, Binyuan Hui, Bo Zheng, Bowen Yu, Chang Zhou, Chengpeng Li, Chengyuan Li, Dayiheng Liu, Fei Huang, et al. Qwen2 technical report. *arXiv preprint arXiv:2407.10671*, 2024.

An Yang, Anfeng Li, Baosong Yang, Beichen Zhang, Binyuan Hui, Bo Zheng, Bowen Yu, Chang Gao, Chengen Huang, Chenxu Lv, et al. Qwen3 technical report. *arXiv preprint arXiv:2505.09388*, 2025.

Ping Yu, Jack Lanchantin, Tianlu Wang, Weizhe Yuan, Olga Golovneva, Ilia Kulikov, Sainbayar Sukhbaatar, Jason Weston, and Jing Xu. Cot-self-instruct: Building high-quality synthetic prompts for reasoning and non-reasoning tasks. *arXiv preprint arXiv:2507.23751*, 2025a.

Qiying Yu, Zheng Zhang, Ruofei Zhu, Yufeng Yuan, Xiaochen Zuo, Yu Yue, Weinan Dai, Tiantian Fan, Gaohong Liu, Lingjun Liu, et al. Dapo: An open-source llm reinforcement learning system at scale. *arXiv preprint arXiv:2503.14476*, 2025b.

Weizhe Yuan, Richard Yuanzhe Pang, Kyunghyun Cho, Sainbayar Sukhbaatar, Jing Xu, and Jason Weston. Self-rewarding language models. *arXiv preprint arXiv:2401.10020*, 3, 2024.

Yang Yue, Zhiqi Chen, Rui Lu, Andrew Zhao, Zhaokai Wang, Shiji Song, and Gao Huang. Does reinforcement learning really incentivize reasoning capacity in llms beyond the base model? *arXiv preprint arXiv:2504.13837*, 2025.

Eric Zelikman, Yuhuai Wu, Jesse Mu, and Noah Goodman. Star: Bootstrapping reasoning with reasoning. *Advances in Neural Information Processing Systems*, 35:15476–15488, 2022.

Qingyang Zhang, Haitao Wu, Changqing Zhang, Peilin Zhao, and Yatao Bian. Right question is already half the answer: Fully unsupervised llm reasoning incentivization. *arXiv preprint arXiv:2504.05812*, 2025a.

Yanzhi Zhang, Zhaoxi Zhang, Haoxiang Guan, Yilin Cheng, Yitong Duan, Chen Wang, Yue Wang, Shuxin Zheng, and Jiyan He. No free lunch: Rethinking internal feedback for llm reasoning. *arXiv preprint arXiv:2506.17219*, 2025b.

Xuandong Zhao, Zhewei Kang, Aosong Feng, Sergey Levine, and Dawn Song. Learning to reason without external rewards. *arXiv preprint arXiv:2505.19590*, 2025.

Yujun Zhou, Zhenwen Liang, Haolin Liu, Wenhao Yu, Kishan Panaganti, Linfeng Song, Dian Yu, Xiangliang Zhang, Haitao Mi, and Dong Yu. Evolving language models without labels: Majority drives selection, novelty promotes variation. *arXiv preprint arXiv:2509.15194*, 2025.

Xinyu Zhu, Mengzhou Xia, Zhepei Wei, Wei-Lin Chen, Danqi Chen, and Yu Meng. The surprising effectiveness of negative reinforcement in llm reasoning. *arXiv preprint arXiv:2506.01347*, 2025.

Yuxin Zuo, Kaiyan Zhang, Li Sheng, Shang Qu, Ganqu Cui, Xuekai Zhu, Haozhan Li, Yuchen Zhang, Xinwei Long, Ermo Hua, et al. Ttrl: Test-time reinforcement learning. *arXiv preprint arXiv:2504.16084*, 2025.

Appendix

A   A Pseudo Code of RESTRAIN Loss Function

This section shows a pseudo code of our RESTRAIN loss function calculation for one single prompt.

Listing 1 The pseudo-code of the RESTRAIN loss function for one prompt

```python
def restrain_loss(outputs, prompt_weight, threshold, neg_offset):
    # --- Extract answers ---
    answers = [extract_answer(output) for output in outputs]

    # --- Majority size M(x) ---
    counts = Counter(answers)
    Mx = counts.most_common(1)[0][1]

    # --------------------------------
    # Branch 1: Negative penalization
    # --------------------------------
    if Mx < threshold:
        rewards = [0.0] * len(outputs)
        adv = calculate_advantages(rewards)
        adv = [a - neg_offset for a in adv]
        loss = calculate_loss(adv)
        return prompt_weight * loss

    # --------------------------------
    # Branch 2: Pseudo-label weighting
    # --------------------------------
    # Calculate label weights
    freqs = counts.values() / len(outputs)
    label_weights = calculate_label_weight(freqs)

    # Calculate each label loss, then weighted sum to a final loss
    final_loss = 0.0
    for i, label in enumerate(counts.keys()):
        rewards = [reward_fn(ans, label) for ans in answers]
        adv = calculate_advantages(rewards)
        loss = calculate_loss(adv)
        final_loss += label_weights[i] * loss

    return prompt_weight * final_loss
```

B An Algorithm of the Per-prompt RESTRAIN Loss Function

Algorithm 1: Per-prompt RESTRAIN Loss

Input	: Responses $\mathcal{O} = \{o_1, \ldots, o_n\}$; prompt weight $u_x > 0$; majority threshold κ; negative offset $\delta \geq 0$.
Output	: Loss L.

1 $\mathcal{A} = \{a_1, \ldots, a_m\} \leftarrow \mathtt{Set}([\mathtt{ExtractAnswer}(o_i)]_{i=1}^n);\quad M(x) \leftarrow \max(\mathtt{Count}(a))$

2 **if** $M(x) < \kappa$ **then**

3 $\quad\quad$ $r_i \leftarrow 0\ \forall i;\quad \mathbf{adv} \leftarrow \mathtt{CalculateAdvantages}(\{r_i\}_{i=1}^n);\quad adv_i \leftarrow adv_i - \delta\ \forall i;$

4 $\quad\quad$ **return** $L \leftarrow u_x \cdot \mathtt{CalculateLoss}(\mathbf{adv})$

5 **else**

6 $\quad\quad$ **for** $j = 1$ **to** m **do**

7 $\quad\quad\quad\quad$ $f_j \leftarrow c(t_j)/n;\quad \tilde{w}_j \leftarrow \mathtt{CalculateWeight}(f_j)$

8 $\quad\quad$ $Z \leftarrow \sum_{j=1}^m \tilde{w}_j;\quad w_j \leftarrow \tilde{w}_j/Z\ \forall j;$

9 $\quad\quad$ $L_{\mathrm{final}} \leftarrow 0;$

10 $\quad\quad$ **for** $j = 1$ **to** m **do**

11 $\quad\quad\quad\quad$ $r_i \leftarrow \mathtt{RewardFn}(\mathcal{A}[i], a_j)\ \forall i;\quad \mathbf{adv} \leftarrow \mathtt{CalculateAdvantages}(\{r_i\}_{i=1}^n);\quad \ell_j \leftarrow \mathtt{CalculateLoss}(\mathbf{adv});$

12 $\quad\quad\quad\quad$ $L_{\mathrm{final}} \leftarrow L_{\mathrm{final}} + w_j \cdot \ell_j$

13 $\quad\quad$ **return** $L \leftarrow u_x \cdot L_{\mathrm{final}}$
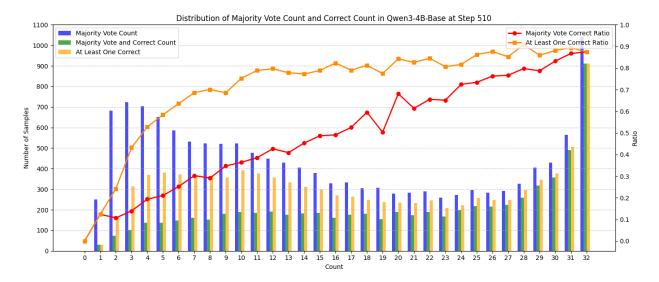

C Discussion of Motivation

Figure 7 Statistics of Majority Vote Count and Pass@32. The model is trained with Pseudo-label Weighting and Prompt-level weighting. We select a checkpoint when the training converges and use the checkpoint to do inference on the training set to analyze the majority vote count and pass@32.

Figure 7 summarizes majority-vote statistics at step 510 for Qwen3-4B-Base trained with our pseudo-label and prompt-level weighting on the DAPO dataset. The x-axis represents the majority vote count. The chart highlights two key trends: (a) The red line shows the **Majority Vote Correct Ratio**. As the majority vote count decreases (moving left on the graph), the probability that the most frequent answer is actually correct drops almost linearly. (b) The orange line shows the **At Least One Correct Ratio** (i.e. Pass@k). This is the probability that at least one of the generated responses was correct, even if it wasn't the majority answer. This distinction is important for understanding different training methods. A method like TTRL is highly dependent on the majority vote being correct (the red line). When the consensus is low (a low majority vote count), TTRL receives an unreliable and often incorrect training signal. Our proposed method, however, relies on the principle of at least one correct answer being present (the orange line). As long as one of the generated responses is correct, our model receives a valid positive signal for training. This makes it more robust, especially in cases where there isn't a strong consensus on the correct answer. However, the chart also reveals a critical weakness. For very low majority vote counts, the orange line shows a dramatic drop. This indicates that when the model's consensus is extremely low, it's highly probable that none of the generated responses are correct. In this scenario, our method is exposed to significant training noise because there is no positive signal to learn from. To address this specific problem, we introduce our negative rollout penalization to discourage the model from generating sets of answers where none are correct.

16

D Detailed Results

D.1 Benchmarks

Our benchmark suite comprises six publicly available datasets spanning mathematics (four) and science (two). (1) MATH-500(Hendrycks et al., 2021): a 500-problem subset of the MATH corpus, emphasizing competition-style problems across algebra, geometry, number theory, and combinatorics. (2) AIME25 (Li et al., 2024): the official 2025 American Invitational Mathematics Examination questions. (3) OlympiadBench (math subset) (Yang et al., 2024): olympiad-level problems sourced from national/international contests; we use the mathematics portion only. (4) Minerva_math (Yang et al., 2024): the mathematics split from the Minerva quantitative-reasoning suite. (5) MMLU_STEM (Yang et al., 2024): the STEM categories of MMLU (e.g., physics, chemistry, biology, mathematics-adjacent subjects). (6) GPQA-Diamond (Yang et al., 2024): the highest-difficulty split of GPQA with expert-written, graduate-level science questions spanning physics, chemistry, and biology.

In addition to the 6 benchmarks reported in the main paper, we evaluate on three additional benchmarks. They are (1) **AMC23**(Li et al., 2024): prompts drawn from the 2023 American Mathematics Competitions, covering core high-school problem-solving domains. (2) **AIME24** (Li et al., 2024): the official 2024 American Invitational Mathematics Examination questions. (3) **s1k (verifiable subset)** (Muennighoff et al., 2025): a subset of 893 s1k examples with verifiable answers from Yu et al. (2025a).

D.2 Implementation Details

We implement TTRL, SRT, and RESTRAIN using the VERL codebase. To validate correctness, we reproduce a representative experiment from the original papers with our implementations and verify that the resulting accuracies match. Since ETMR has not released code, we report its results as stated in the original paper. For hyperparameters, we use a learning rate of 1×10^{-6}, and adopt the AdamW optimizer for the policy model. We set kl loss coefficient to 0.001, and the entropy coefficient to 0. For rollout, we sample 16 responses using a temperature of 1.0 for training. The maximum generation length is set to 4096 for Qwen3-4B-Base and Llama3.1-8B-Instruct, and 8192 for Octothinker Hybrid 8B base model. We set the number of epochs to 20. All experiments were conducted on 32 * NVIDIA A100 80GB GPUs.

Table 5 The table shows the evaluation results of training Qwen3-4B-Base on **14k DAPO dataset**, all results(%) are averaged over 16 seeds. The best results are highlighted in **bold**.

Model	math.	amc.	aime24	aime25	olym.	miner.	mmlu.	gpqa.	s1k	avg
Qwen3-4B-Base	68.0	45.6	10.4	7.9	35.4	26.0	58.3	32.2	5.1	32.1
w/ access to gold label										
GRPO	85.0	69.3	21.2	20.8	50.1	40.1	73.7	38.7	12.2	45.7
w/o access to gold label										
TTRL	76.3	52.6	12.0	8.3	39.6	35.9	59.4	33.6	4.6	35.8
SRT (easy prompt)	77.8	52.3	13.5	7.9	39.7	36.3	60.5	34.9	5.6	36.5
SRT (offline majority label)	76.9	51.8	10.4	12.0	39.8	34.2	59.4	34.5	4.7	36.0
RESTRAIN	**83.0**	**60.2**	**20.3**	**17.9**	**47.0**	**36.5**	**80.9**	**40.2**	**10.3**	**44.0**
Oct.Hybrid-8B-Base	29.8	16.1	1.9	0.8	12.1	9.3	8.6	24.6	2.1	15.0
w/ access to gold label										
GRPO	71.7	49.4	10.8	6.2	35.2	31.3	62.0	31.0	7.2	33.9
w/o access to gold label										
TTRL	56.5	32.2	3.9	2.7	23.2	22.1	51.7	27.3	3.5	24.8
RESTRAIN	**61.6**	**33.6**	**6.0**	**8.5**	**24.6**	**25.0**	**64.6**	**29.9**	**4.4**	**28.7**

D.3 Addition Results

Table 5 and 6 show full results of our RESTRAIN on nine benchmarks. Results show that our method can outperform all unsupervised methods on both Qwen3-4B-Base and Octothinker Hybrid 8B base models with two different training datasets.
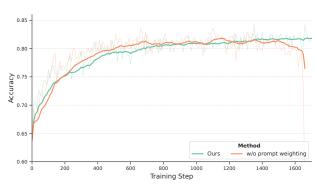
Table 6 The table shows the evaluation results of training Qwen3-4B-Base on **5k Synthetic S1k dataset**, all results(%) are averaged over 16 seeds. The best results are highlighted in **bold**.

Model	math.	amc.	aime24	aime25	olym.	miner.	mmlu.	gpqa.	s1k	Avg. ↑
Qwen3-4B-Base	68.0	45.6	10.4	7.9	35.4	26.0	58.3	32.2	5.1	32.1
w/ access to Qwen3-4B label										
GRPO	83.7	64.5	23.7	18.9	48.4	39.7	83.5	43.5	11.5	46.4
w/o access to Qwen3-4B label										
TTRL	76.0	50.2	10.8	9.2	39.3	35.9	57.6	32.8	4.8	35.2
SRT (easy prompt)	76.4	52.3	12.1	8.1	39.6	34.8	57.5	33.0	4.9	35.4
SRT (offline majority label)	75.8	53.3	11.9	10.4	39.2	33.1	57.1	33.1	4.6	35.4
RESTRAIN	**81.7**	**58.4**	**17.9**	**20.0**	**45.5**	**36.5**	**73.4**	**40.0**	**8.8**	**42.5**

E   Ablation Study of Prompt-level Weighting

In this section, we evaluate the effect of prompt-level weighting on training. We ablate it by two experiments, one is comparing our offline prompt weighting with online prompt weighting, another is setting all prompt weights to 1 ("w/o prompt weighting"). As shown in Figure 8, in the first experiment, online prompt weighting quickly collapses while offline prompt weighting can continue to improve. For the second experiment, both methods' accuracies improve quickly at the start. Initially, the model without prompt weighting learns slightly faster. However, our method soon overtakes it and consistently maintains a higher accuracy. Notably, the performance of the model without prompt weighting becomes unstable and drops sharply after 1,500 training steps. In contrast, our method's accuracy remains stable and continues to improve. This suggests that offline prompt-level weighting is key to achieving both higher final accuracy and greater training stability.

(a) Online Prompt Weighting collapses very quickly at around 100 steps.

(b) Without Prompt Weighting, the model will ultimately collapse.

Figure 8 **Offline Prompt-weighting can help model train stable.**

F Study of Hyperparameter Weight Bias σ in Pseudo-label Weighting

In this section, we examine the bias parameter σ used in pseudo-label weighting. Table 7 reports the tuning results. When σ is small, the scheme effectively reduces to selecting the majority-vote answer as the pseudo label; when σ is large, it approaches uniform weighting. We observe that σ values near zero or above 1 lead to training collapse and substantially worse performance, whereas a moderate setting (e.g., $\sigma = 0.5$) yields the best stability and accuracy.

Table 7 Ablation of Pseudo-label Weighting. The table shows the evaluation results of training Qwen3-4B-Base on **14k DAPO-Math dataset** by varying the hyperparameter weight bias, all results(%) are averaged over 16 seeds. The best results are highlighted in **bold**.

Target Level Weighting Bias σ Small σ = skewed on majority label Large σ = evenly dist. on all labels	MATH500	AIME25	Olym.	Minerva.	MMLU.	GPQA-d.	Avg. ↑
$\sigma = 0$ (0 weights on non-majority labels)	67.8	7.7	34.7	24.1	58.6	32.1	37.5
$\sigma = 0.1$	73.4	2.7	36.0	34.4	62.4	31.3	40.0
$\sigma = 0.25$	76.5	9.6	39.7	32.6	60.52	34.4	42.2
$\sigma = 0.5$	**83.0**	**17.9**	**47.0**	**36.5**	**80.9**	**40.2**	**51.0**
$\sigma = 1$	65.1	7.3	33.4	24.3	59.0	32.8	37.0
$\sigma = 2$	66.2	6.2	33.1	23.8	58.9	31.4	36.6
$\sigma = 5$	61.1	5.8	32.6	23.7	58.4	33.3	35.8
$\sigma = \infty$ (evenly distributed)	66.8	6.9	34.6	24.6	59.8	32.9	37.6

G Study of Hyperparameter Negative Advantage Offset δ and Majority Count Threshold κ

In this section, we examine how the negative-advantage offset δ and the majority-count threshold κ influence performance. The offset δ scales the penalty applied to low-consensus rollouts; if set too high, it over-penalizes the policy and induces a sharp accuracy decline. The threshold κ decides which prompts are treated as low-consensus: a strict threshold discards many informative examples and hurts accuracy, while an overly loose threshold admits noisy cases and weakens the intended penalization. Appropriate, balanced choices of δ and κ suppress noise without sacrificing useful signal.

Table 8 Results on Different Negative rollout Penalty. The table shows the evaluation results of training Qwen3-4B-Base on **14k DAPO-Math dataset** by varying the negative advantage offset, all results(%) are averaged over 16 seeds. The best results are highlighted in **bold**.

Negative Advantage Offset	MATH500	AIME25	Olym.	Minerva.	MMLU.	GPQA-d.	Avg. ↑
$\delta = 0$	76.5	9.8	40.5	31.8	58.3	33.2	41.7
$\delta = 0.1$	78.7	13.3	40.8	36.5	59.3	35.5	44.0
$\delta = 1$	**83.0**	**17.9**	**47.0**	**36.5**	**80.9**	**40.2**	**51.0**
$\delta = 2$	70.6	10.4	36.7	27.9	56.6	32.6	39.1
$\delta = 5$	70.4	7.1	36.1	25.0	56.9	31.6	37.9

Table 9 Results on Different Majority Count Threshold. The table shows the evaluation results of training Qwen3-4B-Base on **14k DAPO-Math dataset** by varying the weight bias, all results(%) are averaged over 16 seeds. The best results are highlighted in **bold**.

Majority Count Threshold (for negative rollouts)	MATH500	AIME25	Olym.	Minerva.	MMLU.	GPQA-d.	Avg. ↑
$\kappa = 2$	77.4	8.8	41.3	33.8	59.8	34.2	42.5
$\kappa = 3$	**83.0**	**17.9**	**47.0**	**36.5**	**80.9**	**40.2**	**51.0**
$\kappa = 5$	78.2	13.3	40.4	29.2	61.4	35.6	43.0
$\kappa = 8$	67.3	6.0	34.1	24.5	59.3	33.7	37.5

H   The Use of Large Language Models

In this work, we use LLM for writing polishing and do not use it for any other purpose.