Verifier-Free RL for LLMs via Intrinsic Gradient-Norm Reward

Xuexiang Wen[1] **Hang Yu**[2] **Linchao Zhu**[1†] **Gaoang Wang**[1†]
[1]Zhejiang University [2]Ant Group
{xuexiangwen, zhulinchao}@zju.edu.cn gaoangwang@intl.zju.edu.cn

Abstract

While Reinforcement Learning with Verifiable Rewards (RLVR) has recently emerged as a promising post-training paradigm for Large Language Models (LLMs), its dependency on the gold label or domain-specific verifiers limits its scalability to new tasks and domains. In this work, we propose **Verifier-free Intrinsic Gradient-Norm Reward** (**VIGOR**), a simple reward that uses only the policy model itself. Given a prompt, VIGOR samples a group of completions and assigns higher within-group rewards to outputs that induce smaller ℓ_2 norms of the teacher-forced negative log-likelihood gradients under the current parameters. Intuitively, lower gradient norms suggest the completion aligns better with the current policy, serving as an intrinsic preference signal for policy optimization. To make this intrinsic signal practical for RL, we correct the systematic length bias of averaged token-level gradients with a $\sqrt{T}$ scaling, and apply group-wise rank shaping to stabilize reward scales across prompts. Across mathematical reasoning benchmarks, VIGOR outperforms the state-of-the-art Reinforcement Learning from Internal Feedback (RLIF) baseline, and it also exhibits cross-domain transfer to code benchmarks when trained only on math data. For instance, on Qwen2.5-7B-Base post-trained on MATH, VIGOR improves the average math accuracy by +3.31% and the average code accuracy by +1.91% over this baseline, while exhibiting more stable training dynamics.

1 Introduction

Large Language Models (LLMs) have shown remarkable capabilities in reasoning tasks and general tasks. To further enhance LLMs' performance, Reinforcement Learning with Verifiable Rewards (RLVR) has emerged as a promising post-training paradigm.

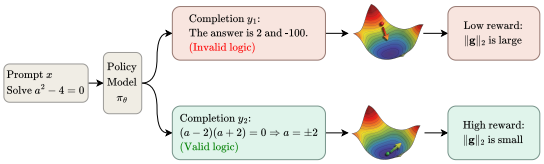


Figure 1: **Intuition behind VIGOR.** For a prompt x, we sample two completions $y_1, y_2 \sim \pi_\theta$, each inducing its own teacher-forced loss surface $\ell(y_i \mid x; \theta)$ over the parameters θ. The arrow at each point indicates the direction of the local gradient $\nabla_\theta \ell$, and the steepness of the surface at that point reflects its magnitude $\|\mathbf{g}\|_2$. Under the current policy, the invalid y_1 (red) incurs a higher NLL and sits on a steep slope (large $\|\mathbf{g}\|_2$), while the valid y_2 (green) lies in a flatter basin (small $\|\mathbf{g}\|_2$). VIGOR turns this parameter-space signal into a verifier-free reward by assigning higher reward to completions with smaller $\|\mathbf{g}\|_2$.

However, RLVR depends on the task-specific, programmatic verifiers (e.g., exact-match answer checkers for math or unit tests for code) (DeepSeek-AI et al., 2025; Wang et al., 2025). When such verifiers are unavailable, the reward signal becomes hard to specify, limiting RLVR's applicability and scalability.

To mitigate this, previous work explores verifier-free alternatives. One crucial line adopts majority voting to derive pseudo-labels from multiple samples, treating the consensus answer as the reward signal (Zuo et al., 2025; Zhang et al., 2025c; Shafayat et al., 2025). Despite the promising results, voting-based methods typically assume that final answers can be reliably extracted and normalized for aggregation across samples, which may limit their applicability to open-ended generation settings (Wang et al., 2023). Another prominent line constructs intrinsic rewards from the model's internal signals like entropy, enabling verifier-free post-training (Prabhudesai et al., 2025; Zhao et al., 2025). Despite the encouraging gains, such in-

†Corresponding authors.

Findings of the Association for Computational Linguistics: ACL 2026, pages 32089–32102

trinsic rewards may become brittle as training progresses, leading to instability and even performance degradation (Zhang et al., 2025b). Overall, we still lack verifier-free rewards that are both broadly applicable to free-form outputs and stable during RL, without relying on auxiliary components beyond the policy model itself.

In this work, we propose **VIGOR**, a verifier-free intrinsic *gradient-norm* reward computed solely from the policy model. Concretely, for each sampled completion, we compute the gradient norm of the teacher-forced negative log-likelihood under the current parameters (without updating the model), and assign higher rewards to completions that induce smaller gradient norms. This design is motivated by a first-order optimization view: smaller gradients typically correspond to milder updates and thus smoother training dynamics. To make the signal robust in practice, we (i) correct the systematic length bias with a $\sqrt{T}$ normalization, and (ii) apply rank-based reward shaping to stabilize reward scales across prompts. We conduct two separate post-training runs on Qwen2.5 base models, one using a mathematics dataset and the other using a code dataset, and evaluate both resulting models on a broad suite of benchmarks spanning mathematical reasoning, code generation, general multi-task capability, and instruction following.

Our contributions are summarized as follows:

- We propose VIGOR, a verifier-free intrinsic gradient-norm reward for RL of LLMs; on Qwen2.5-7B post-trained on MATH, it surpasses a state-of-the-art verifier-free RLIF baseline by +3.31 avg. math and +1.91 avg. code accuracy, with more stable training dynamics.
- We introduce $\sqrt{T}$ length correction for gradient norms and rank-based normalization to mitigate length bias and stabilize reward scales across prompts.
- We conduct extensive experiments with separate math and code post-training, evaluating both in-domain and cross-domain performance on a broad suite of reasoning, instruction-following, and multi-task benchmarks, with ablations validating both components.

2 Related Work

Reinforcement Learning with Verifiable rewards. Reinforcement learning from externally verifiable rewards has recently demonstrated strong improvements in LLMs' reasoning ability on mathematics and programming tasks (Shao et al., 2024; DeepSeek-AI et al., 2025; Yu et al., 2025; Team et al., 2025; Xiaomi et al., 2025). These works often instantiate an outcome reward model (ORM) that scores only final answers, using PPO-style optimizers such as GRPO (Shao et al., 2024) and DAPO (Yu et al., 2025). Besides, some researchers focus on the process reward models (PRMs) that provide denser reward signals by assigning step-level feedback along the reasoning trajectory, alleviating the sparsity of final-answer rewards (Lightman et al., 2023; Lin et al., 2025).

Reinforcement Learning with Intrinsic and Self-Supervised Rewards. Beyond domains with reliable verifiers, obtaining high-quality rewards remains challenging. Recent works therefore explore intrinsic and self-supervised rewards. These rewards are generated by the model itself instead of external verifiers. Methods like TTRL (Zuo et al., 2025) and Co-rewarding (Zhang et al., 2025c) leverage majority voting (Shafayat et al., 2025) to get pseudo labels for optimization. In contrast, some methods exploit the intrinsic model signals, such as self-certainty and entropy-based objectives, to construct the reward without labels (Zhao et al., 2025; Prabhudesai et al., 2025; Agarwal et al., 2025). EMPO (Zhang et al., 2025a) also minimizes semantic entropy to improve performance, but its semantic entropy computation relies on auxiliary semantic equivalence model beyond the policy model itself.

3 Method

In this section, we propose an intrinsic gradient-norm reward for RL of LLMs. Unlike RL with verifiable rewards (RLVR), our method does not require an external verifier, and instead leverages internal training dynamics of the language model as supervision.

3.1 Preliminaries

We formulate LLM reasoning as a sequence generation problem. Given a prompt x, the policy π_θ generates a complete sequence $y = [r; a]$, where r denotes the chain-of-thought reasoning and a denotes the final answer.

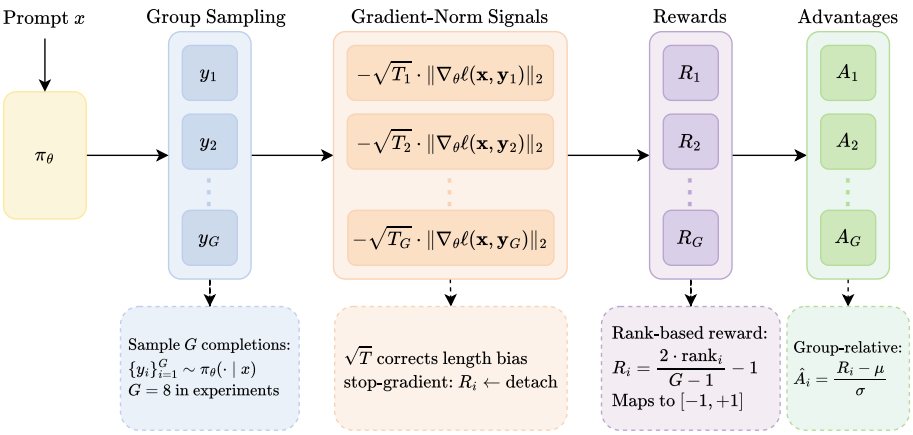


Figure 2: Overview of the proposed method. For each prompt $\mathbf{x}$, we sample a group of G completions from the current policy π_θ, compute the ℓ_2 gradient norm of the token-averaged NLL for each completion, and apply a $\sqrt{T}$ length correction to obtain the raw signal $S_{\text{GN}}(\mathbf{x}, \mathbf{y})$. The corrected signals are then mapped to rank-based rewards $R_i \in [-1, +1]$ and normalized into group-relative advantages $\hat{A}_i$ for policy updates.

Under this formulation, reinforcement learning (RL) provides a natural framework for optimizing the policy π_θ. We review Group Relative Policy Optimization (GRPO) (Shao et al., 2024), an RL algorithm tailored for LLMs. For each prompt x, it samples a group of G completions $\{y_i\}_{i=1}^G$ from π_θ. The advantage $\hat{A}_i$ for each completion is computed by standardizing the rewards within the group:

$$\hat{A}_i = \frac{R(x, y_i) - \frac{1}{G}\sum_{j=1}^G R(x, y_j)}{\sqrt{\frac{1}{G}\sum_{j=1}^G \left(R(x, y_j) - \bar{R}\right)^2}}, \qquad (1)$$

where $R(x, y_i)$ is the reward for completion y_i. The policy model π_θ is then updated by maximizing a PPO-style objective that encourages completions with high group-relative advantages $\hat{A}_i$, regularized by the KL divergence between the current and reference policy.

3.2 Intrinsic Gradient-Norm Reward

Motivation. While GRPO provides an efficient optimization framework, RLVR approaches typically rely on externally verifiable reward signals (e.g., ground-truth matching for math or execution-based signals for code tasks). This reliance constrains their capability to generalize to open-ended or weakly verifiable tasks where such external supervision is unavailable.

From an optimization perspective, first-order stationarity is characterized by near-zero gradients. The gradient norm therefore provides a natural proxy for how close the current parameters are to a stationary region: a smaller norm typically implies weaker first-order variation of the loss locally and consequently more stable parameter updates. Motivated by this observation, we propose an *intrinsic gradient-norm reward* that prefers completions inducing smaller ℓ_2 gradient norms under the same prompt. Intuitively, such outputs reflect more stable response patterns under the current policy and assigning them higher relative reward suppresses abrupt gradient fluctuations and encourages smoother, more controllable training dynamics without requiring any external verifier.

The Length Bias Problem. For each prompt x, we sample a group of completions $\{y_i\}_{i=1}^G$ from the current policy $\pi_\theta(x)$ and derive an intrinsic signal from their training-dependent gradients. For a completion $y = (y_1, \ldots, y_T)$ with non-padding length $T = |y|$, we define its average token-level negative log-likelihood as

$$\ell_{\text{mean}}(x, y) = \frac{1}{T}\sum_{t=1}^{T} \ell_t(x, y), \qquad (2)$$

where $\ell_t(x, y) = -\log \pi_\theta(y_t \mid x, y_{<t})$ denotes the per-token NLL.

We then compute the corresponding gradient $\mathbf{g}(x, y) = \nabla_\theta \ell_{\text{mean}}(x, y)$ and treat $\|\mathbf{g}(x, y)\|_2$ as an intrinsic signal: smaller norms generally correspond to milder updates and smoother optimization behavior. We detach this gradient norm from the computation graph and use it solely as a scalar reward signal.

However, this choice introduces a systematic length bias: since ℓ_{mean} averages ℓ_t over T to-

Length bin	Avg. tokens	$\|\mathbf{g}\|_2$	$\sqrt{T}\|\mathbf{g}\|_2$
1	~ 250	~ 180	$\sim 2.85 \times 10^3$
2	~ 500	~ 130	$\sim 2.91 \times 10^3$
3	~ 750	~ 105	$\sim 2.88 \times 10^3$
4	~ 1000	~ 90	$\sim 2.84 \times 10^3$

Table 1: Empirical verification of $\sqrt{T}$ scaling. We bin training completions by sequence length and report the average raw gradient norm $\|\mathbf{g}\|_2$ and the rescaled $\sqrt{T}\|\mathbf{g}\|_2$. The rescaled values remain nearly constant across bins, confirming that $\sqrt{T}$ neutralizes the length-driven variation rather than imposing a length penalty.

kens, the gradient $\mathbf{g}(x, y)$ is likewise a $1/T$-scaled sum of per-token gradient contributions $\nabla_\theta \ell_t(x, y)$. These per-token contributions vary in sign and magnitude as the context $y_{<t}$ changes, so as T grows they partially cancel across token positions, causing $\|\mathbf{g}(x, y)\|_2$ to shrink even when completion quality does not improve. As a loose analogy, under an independence assumption on per-token contributions, one would expect $\|\mathbf{g}\|_2 = O(1/\sqrt{T})$. Although this assumption does not strictly hold for autoregressive LLMs, sufficient cancellation still occurs in practice.

To verify this $O(1/\sqrt{T})$ scaling, we group all training completions into bins by sequence length and compute the average gradient norm per bin. As shown in Table 1, the raw $\|\mathbf{g}\|_2$ varies by roughly $2\times$ across length bins, while the rescaled $\sqrt{T}\|\mathbf{g}\|_2$ remains nearly constant, confirming the $O(1/\sqrt{T})$ scaling behavior. As a result, $\|\mathbf{g}(x, y)\|_2$ tends to shrink as T grows even when completion quality does not improve, making a gradient-norm-based reward vulnerable to length hacking. To counteract this bias, we multiply by $\sqrt{T}$ to produce an approximately length-invariant raw signal:

$$S_{\text{GN}}(x, y) = -\sqrt{T}\,\|\mathbf{g}(x, y)\|_2. \tag{3}$$

The negative sign converts gradient-norm minimization into a reward maximization objective. This correction is empirically necessary: removing $\sqrt{T}$ induces severe length inflation and accuracy collapse (Figure 5), whereas with $\sqrt{T}$ the reward becomes approximately length-neutral (Table 1).

Rank-based Normalization of the Intrinsic Signal. The raw gradient-norm-based signal $S_{\text{GN}}(x, y)$ is only meaningful in a relative sense within a group of completions, and it may exhibit large scale variations across different prompts. Therefore, we transform the raw signal into a normalized rank-based intrinsic reward. We denote the resulting rank-normalized reward by $R_{\text{GN}}(x, y)$.

Algorithm 1 Intrinsic Gradient-Norm Reward for GRPO

Require: Batch B, policy π_θ, reference π_{ref}, group size G
$\mathcal{T} \leftarrow \emptyset$ ▷ Initialize trajectory buffer
for each prompt $x \in B$ **do**
  // 1. Sample completions
  Sample group $\{y_i\}_{i=1}^{G} \sim \pi_\theta(\cdot \mid x)$
  // 2. Compute gradient norms
  for $i = 1$ to G **do**
    $g_i \leftarrow \|\nabla_\theta \ell_{\text{mean}}(x, y_i)\|_2$ ▷ detach gradient
    $S_i \leftarrow -\sqrt{|y_i|}\, g_i$ ▷ length correction
  end for
  // 3. Estimate advantage
  $\{R_i\} \leftarrow \textsc{RankNorm}(\{S_i\})$ ▷ map ranks to $[-1, 1]$
  $\{\hat{A}_i\} \leftarrow \textsc{Normalize}(\{R_i\})$ ▷ $(R_i - \mu)/\sigma$
  $\mathcal{T} \leftarrow \mathcal{T} \cup \{(x, y_i, \hat{A}_i)\}_{i=1}^{G}$
end for
Update θ maximizing GRPO objective on $\mathcal{T}$ with $\text{KL}(\pi_\theta, \pi_{\text{ref}})$

For each prompt x and corresponding completions $\{y_i\}_{i=1}^{G}$, we compute the raw signals $\{S_{\text{GN}}(x, y_i)\}_{i=1}^{G}$ and sort them from worst to best (smaller $S_{\text{GN}}(x, y_i)$ indicates larger gradient norms and thus worse completions). We assign each completion y_i an integer rank $\text{rank}_i \in \{0, \ldots, G-1\}$, with smaller ranks indicating worse completions and larger ranks indicating better ones. The resulting normalized intrinsic reward is given by

$$R_{\text{GN}}(x, y_i) = 2\,\frac{\text{rank}_i}{G-1} - 1. \tag{4}$$

This mapping assigns the worst completion ($\text{rank}_i = 0$) reward -1 and the best completion ($\text{rank}_i = G-1$) reward $+1$, with evenly spaced values in between, making the intrinsic signal comparable across prompts while preserving within-group ordering.

3.3 Policy Optimization with Intrinsic Gradient-Norm Reward

Let $\mathcal{D}_X$ denote an unlabeled prompt dataset. In this section, we integrate our proposed intrinsic gradient-norm reward into the GRPO framework. This approach optimizes the policy π_θ to favor completions with lower gradient norms given prompts $x \sim \mathcal{D}_X$.

Objective Formulation. Given a prompt x, we sample a group of G completions $\{y_i\}_{i=1}^{G} \sim \pi_\theta(\cdot \mid x)$. We assign the rank-normalized intrinsic reward R_{GN} to each generation y_i. Then our method approximately minimizes the gradient

norm $\|\mathbf{g}(x, y)\|_2$ by maximizing the following objective:

$$\mathcal{J}(\pi_\theta) = \mathbb{E}\Bigg[\frac{1}{G}\sum_{i=1}^{G}\frac{1}{|y_i|}\sum_{t=1}^{|y_i|}\min\big(r_{i,t}(\theta)\hat{A}_i,\ \bar{r}_{i,t}\hat{A}_i\big) - \beta\,\mathrm{KL}(\pi_\theta, \pi_{\text{ref}})\Bigg], \tag{5}$$

where $r_{i,t}(\theta) = \frac{\pi_\theta(y_{i,t}|x, y_{i,<t})}{\pi_{\theta_{\text{old}}}(y_{i,t}|x, y_{i,<t})}$ is the token-level probability ratio, $\bar{r}_{i,t} = \mathrm{clip}(r_{i,t}(\theta),\ 1{-}\epsilon,\ 1{+}\epsilon)$ is its clipped version, and $\hat{A}_i$ is the group-relative advantage computed by mean-std normalizing $\{R_{\text{GN}}(x, y_j)\}_{j=1}^{G}$ within each prompt group.

Stop-gradient Operation. Although $R_{\text{GN}}(x, y)$ is computed from the current model parameters via the gradient norm, we treat R_{GN} as a constant scalar and detach it from the computation graph during the policy optimization. This avoids backpropagating via the gradient-norm computation and keeps the update first-order.

4 Experiment

4.1 Experiment Setup

Models. We conduct experiments on the Qwen2.5 Base models (Qwen et al., 2025) for fair comparison with prior work and thorough evaluation across model sizes. Specifically, we use Qwen2.5-3B-Base and Qwen2.5-7B-Base.

Training Data. We adopt CodeContests (Li et al., 2022) as the code training dataset and MATH (Hendrycks et al., 2021) as the mathematics training dataset. For simplicity and training efficiency, we use the first 3,200 problems in CodeContests as the training set. For the MATH dataset, we use the full set of 7,500 training problems.

Evaluation. We evaluate our post-trained models on a diverse suite of benchmarks spanning mathematical reasoning, code generation, instruction following, and general multi-task knowledge. For mathematical reasoning, we report results on MATH-500 (Lightman et al., 2024), GSM8K (Cobbe et al., 2021), and AMC (LI et al., 2024). For code reasoning, we use LiveCodeBench (v6) (Jain et al., 2025) and CRUX (Gu et al., 2024). To assess instruction-following capability, we include IFEval (Zhou et al., 2023). Finally, we measure broad multi-task proficiency on MMLU-Pro (Wang et al., 2024). Unless otherwise specified, we follow the standard evaluation protocol for each benchmark and report pass@1 (or accuracy for multiple-choice benchmarks); for AMC, we additionally use a higher-temperature sampling setting and average results across multiple runs to reduce variance. More evaluation details are provided in Appendix B.

Baselines. We compare our method with a pretrained reference and two post-training baselines, including RL with verifiable rewards (RLVR) and verifier-free RL from internal feedback (RLIF).

- **Before RL.** The pretrained Qwen2.5-Base model without any parameter updates.
- **GT-Reward.** We reproduce the GRPO baseline implemented in the Open-R1 framework, where the reward is computed by an exact-match verifier against the ground-truth final answer: we extract the final answer from each completion, compare it with the reference, and use the resulting correctness signal as the reward.
- **INTUITOR.** A verifier-free intrinsic-reward baseline under the Reinforcement Learning from Internal Feedback (RLIF) paradigm. INTUITOR replaces the verifiable reward in GRPO with the policy model's own confidence signal computed from its likelihood, enabling fully unsupervised RL without gold labels or domain-specific verifiers (Zhao et al., 2025).

Implementation Details. Both our method and GRPO baseline are trained with Open-R1 framework (Hugging Face, 2025). For VIGOR training, we discard all reference solutions/labels and use only the problem statements as prompts. Ground-truth answers are used only for evaluation and for the GT-Reward baseline.

In mathematical reasoning task, we train Qwen2.5-3B and Qwen2.5-7B Base models on MATH dataset (Hendrycks et al., 2021). Given a prompt x, we sample a group of $G{=}8$ completions. For the GRPO baseline, we extract the final answer from each completion, compare it with the reference answer and assign a binary reward (1 for correct and 0 for incorrect after standard normalization). For our method, we also sample a group of $G = 8$ completions, but we calculate the reward R_{GN} for each completion without any external verifier. We then compute the group-relative advan-

	Mathematics				Code			Multi-task	Instruction
Methods	**GSM8K**	**MATH500**	**AMC**	**Avg.**	**LiveCodeBench**	**CRUX**	**Avg.**	**MMLU-Pro**	**IFEval**
Qwen2.5-3B-Base									
Before RL (Base)	67.93	54.80	22.28	48.34	9.57	24.38	16.98	36.92	28.30
- GT-Reward	81.96	65.60	29.97	59.17	12.32	33.00	22.66	38.17	29.91
- INTUITOR	79.68	62.40	29.21	57.10	14.88	38.70	26.79	24.48	29.11
- Ours (VIGOR)	**81.80**	**64.60**	**31.02**	**59.14**	**15.90**	**40.00**	**27.95**	**32.65**	**31.72**
Qwen2.5-7B-Base									
Before RL (Base)	43.06	63.00	21.68	42.58	1.99	17.38	9.69	47.21	35.90
- GT-Reward	84.80	74.60	42.01	67.14	7.39	55.50	31.45	43.17	34.63
- INTUITOR	87.19	**76.20**	35.99	66.46	19.81	**57.20**	38.51	43.04	34.91
- Ours (VIGOR)	**88.70**	**76.20**	**44.42**	**69.77**	**24.45**	56.38	**40.42**	**43.09**	**37.03**

Table 2: Main results of models trained on the **MATH** dataset. **Bold** marks the better score between **Ours** and **INTUITOR**. Avg. is the arithmetic mean over tasks in each block.

	Mathematics				Code			Multi-task	Instruction
Methods	**GSM8K**	**MATH500**	**AMC**	**Avg.**	**LiveCodeBench**	**CRUX**	**Avg.**	**MMLU-Pro**	**IFEval**
Qwen2.5-3B-Base									
Before RL (Base)	67.93	54.80	22.28	48.34	9.57	24.38	16.98	36.92	28.30
- INTUITOR	75.13	58.60	22.59	52.11	11.47	**39.38**	**25.43**	29.07	29.98
- Ours (VIGOR)	**77.10**	**62.80**	**29.82**	**56.57**	**11.65**	35.62	23.64	**35.01**	**32.39**

Table 3: Main results of models trained on the **CodeContests** dataset. **Bold** marks the better score between **Ours** and **INTUITOR**. Avg. is the arithmetic mean over tasks in each block.

tages from these rewards and update the policy following the same GRPO optimization procedure.

In code generation task, following the setting of INTUITOR (Zhao et al., 2025), we only train Qwen2.5-3B Base models on the first 3,200 problems of the CodeContests dataset (Li et al., 2022) and adopt the same optimization strategy in the mathematical reasoning task.

More detailed implementation details are provided in Appendix A.

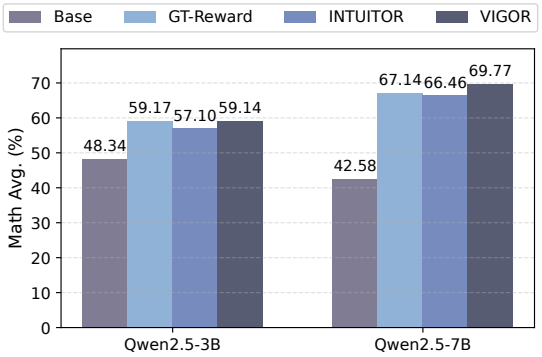
4.2 Main Results

Figure 3 provides an overview of average math and code performance across model scales, comparing Base, GRPO, INTUITOR, and VIGOR. The detailed experimental results are reported in Table 2 and Table 3. These results show that our method consistently improves the reasoning ability of Qwen2.5 Base model on both mathematics and code tasks.

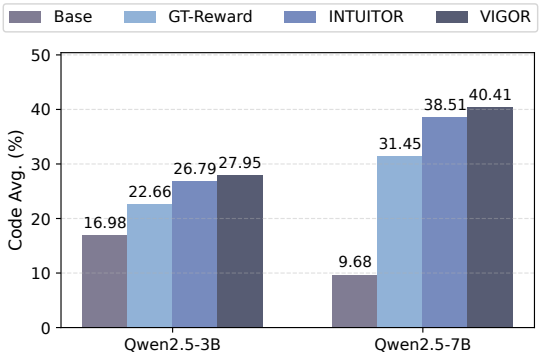
Training on MATH. Table 2 reports the experimental results trained on MATH. VIGOR substantially improves Qwen2.5 Base models on mathematic benchmarks, with the average performance increasing from 48.34% to 59.14% on Qwen2.5-3B and from 42.58% to 69.77% on Qwen2.5-7B. Specifically, VIGOR improves GSM8K and AMC by +45.64% and +22.74% on Qwen2.5-7B, indicating that the intrinsic gradient-norm signal can effectively drive reasoning improvement without answer supervision. Beyond in-domain tasks, VIGOR also transfers to code when trained on MATH, improving the average accuracy of code benchmarks by +10.97% on Qwen2.5-3B and +30.73% on Qwen2.5-7B. In terms of general multi-task and instruction-following ability, VIGOR largely preserves performance on MMLU-Pro and IFEval while outperforming the verifier-free baseline.

Training on CodeContests. Table 3 reports results of training on CodeContests with Qwen2.5-3B-Base. Since our main focus is mathematical reasoning (where we run full experiments on both 3B and 7B), we include CodeContests as a lightweight sanity check to verify that our verifier-free reward remains effective for prompt-only code generation training. For simplicity and controlled training cost, we only use a small subset of 3,200 problems from the CodeContests training split, aiming to validate generality rather than to maximize code performance.

Despite this lightweight setup, VIGOR still yields gains on code reasoning, improving the av-

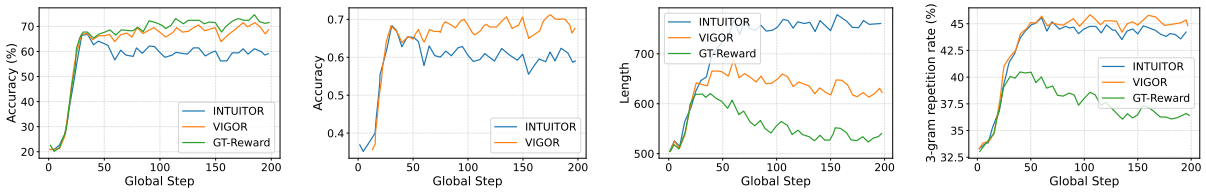


(a) Average performance on math benchmarks.



(b) Average performance on code benchmarks.

Figure 3: Average benchmark performance across model scales.



(a) Accuracy (b) Top-25% accuracy (c) Length (d) 3-gram repetition

Figure 4: Training dynamics under verifier-free intrinsic rewards. We track 198 training steps of Qwen2.5-7B-Base optimized with INTUITOR (self-certainty reward) and VIGOR (gradient-norm reward), and compare against a verifiable GT-Reward baseline. We report (a) overall accuracy, (b) accuracy of the top-25% completions ranked by within-group reward, (c) average completion length, and (d) mean 3-gram repetition rate. We omit GT-Reward in (b) since its exact-match reward directly encodes correctness.

erage code performance from 16.98% to 23.64%. In addition, CodeContests training also transfers to mathematical reasoning: VIGOR improves the math average from 48.34% to 56.57%, with consistent gains on GSM8K, MATH500, and AMC. We note, however, that on Code Avg., VIGOR (23.64%) underperforms INTUITOR (25.43%), particularly on CRUX. We attribute this to the nature of competitive programming, where correctness often hinges on discrete algorithmic choices that may not be well-reflected in gradient-norm smoothness.

4.3 Analysis

To better understand the training dynamics of our proposed gradient-norm reward, we visualize various metrics during training in Figure 4. We compare VIGOR against two baselines: (i) INTUITOR, a verifier-free method that uses self-certainty as the reward, and (ii) GT-Reward, which applies GRPO with an exact-match reward against the reference answer. In Figure 4, accuracy denotes training-time evaluation accuracy computed by exact match against references, used only for monitoring.

Training Dynamics and Stability. As shown in Figure 4(a), all methods improve rapidly during the initial stage of training. However, the trajectories diverge in later updates: INTUITOR exhibits a clear late-stage regression, where accuracy drifts downward rather than plateauing. This pattern is consistent with reward-proxy degeneration—as the policy adapts, confidence-based internal feedback becomes increasingly exploitable and can be optimized without improving correctness, causing the update direction to gradually decouple from the true objective. In contrast, VIGOR maintains a stable improvement and consistently achieves higher accuracy, indicating that the gradient-norm reward remains a more reliable training signal throughout optimization.

We hypothesize that this stability stems from defining the reward in the model's parameter space: the gradient norm aggregates signals over a high-dimensional set of parameters and is therefore less sensitive to token-level distribution shifts. In contrast, entropy- or confidence-based rewards are computed from the next-token distribution over the vocabulary and can be more easily exploited by

	Mathematics				Code			Multi-task	Instruction
Methods	**GSM8K**	**MATH500**	**AMC**	**Avg.**	**LiveCodeBench**	**CRUX**	**Avg.**	**MMLU-Pro**	**IFEval**
Qwen2.5-3B-Base									
- Full (VIGOR)	**81.80**	**64.60**	**31.02**	**59.14**	**15.90**	**40.00**	**27.95**	32.65	**31.72**
w/o $\sqrt{T}$	0.08	60.40	1.66	20.71	0.00	0.00	0.00	**36.39**	29.30
w/o rank	81.35	63.00	29.66	58.00	15.26	38.88	27.07	33.44	30.19
Qwen2.5-7B-Base									
- Full (VIGOR)	**88.70**	**76.20**	**44.42**	**69.77**	24.45	56.38	40.42	**43.09**	37.03
w/o $\sqrt{T}$	87.87	75.00	42.01	68.29	24.92	**57.75**	**41.34**	41.34	37.23
w/o rank	88.32	75.20	44.12	69.21	**25.50**	54.62	40.06	34.19	**38.32**

Table 4: Ablation study of the two key components in VIGOR: $\sqrt{T}$ length correction for gradient norms and rank-based normalization for intrinsic signal. Avg. is the arithmetic mean over tasks in each block. **Bold** indicates the best result within each model block.

Rank	Step 10 Acc. (%)	Step 20 Acc. (%)
1 (best)	70.50	72.30
2	68.20	71.10
3	67.90	69.80
4	67.00	67.40
5	66.10	67.10
6	60.70	66.00
7	55.30	65.00
8 (worst)	52.70	63.40

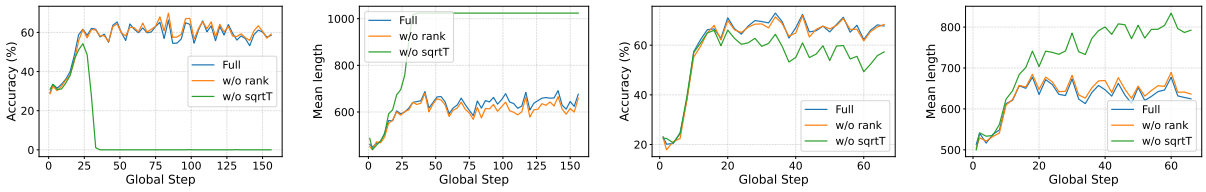
Table 5: Rank–accuracy monotonicity on Qwen2.5-3B-Base trained on MATH. For each prompt we sample $G{=}8$ completions, rank them by the length-corrected gradient-norm score $\sqrt{T}\|\mathbf{g}\|_2$ (rank 1 = lowest norm, best), and report the average accuracy per rank position across all prompts.

superficial behaviors.

Reward Reliability. Figure 4(b) reports the accuracy of the top-25% completions ranked by the within-group intrinsic reward (with group size $G{=}8$, this corresponds to the top-2 completions per prompt), assessing whether high intrinsic rewards consistently align with correctness. VIGOR maintains higher and relatively stable top-25% accuracy across training, exhibiting the reliability of its intrinsic reward signal. In contrast, INTUITOR exhibits a clear degradation in top-25% accuracy in later stages, implying that high-reward samples become progressively less aligned with correctness. This behavior is consistent with reward degeneration: as training proceeds, confidence-based internal signals can be increasingly exploited. We omit GT-Reward in Figure 4(b) since exact-match rewards are tightly coupled with correctness, making this analysis less informative.

Rank–Accuracy Monotonicity. To quantify how well the gradient-norm ranking aligns with task correctness, we conduct a fine-grained monotonicity analysis on Qwen2.5-3B-Base trained on MATH. At training steps 10 and 20, for each prompt we sample $G{=}8$ completions, compute the length-corrected score $\sqrt{T}\|\mathbf{g}\|_2$, rank the 8 completions within each prompt (ascending; lower score corresponds to better rank), and report the average accuracy per rank position across all prompts. As shown in Table 5, a clear monotonic trend holds at both checkpoints: completions ranked better by gradient norm consistently achieve higher accuracy. At step 10, accuracy ranges from 70.50% (rank 1) to 52.70% (rank 8), yielding a gap of 17.8 percentage points. At step 20, the gap narrows to 8.9 points as overall accuracy improves and the model approaches convergence, which is expected behavior. Importantly, the monotonic ordering is preserved throughout, confirming that the gradient-norm ranking remains a directionally consistent signal rather than degrading over training. This fine-grained analysis complements the top-25% accuracy trend in Figure 4(b) and provides statistical validation that the intrinsic gradient-norm reward is meaningfully correlated with correctness.

Output Length and Repetition. Figure 4(c) shows clear differences in generation length. INTUITOR exhibits a pronounced increase in completion length over training, while VIGOR produces substantially shorter outputs; GT-Reward yields the shortest generations overall. We further examine surface-level degeneration using the mean 3-gram repetition rate, which measures the fraction of repeated 3-grams within a completion. As shown in Figure 4(d), VIGOR exhibits repetition rates comparable to INTUITOR, and both are higher than GT-Reward. This observation decouples length growth



(a) 3B Accuracy (b) 3B Mean length (c) 7B Accuracy (d) 7B Mean length

Figure 5: Training dynamics of the Qwen2.5-3B and 7B model under ablations. *Full* uses the complete VIGOR reward; *w/o rank* removes within-group rank-based normalization; *w/o* $\sqrt{T}$ removes length correction. Removing $\sqrt{T}$ induces severe length-bias exploitation: generations become increasingly long and the accuracy collapses (especially on 3B).

from repetition: it suggests that VIGOR's gains are driven by curbing the tendency for unconstrained length inflation, ensuring optimization focuses on reasoning quality rather than length hacking.

4.4 Ablation Study

To investigate the contribution of each component in VIGOR, we conduct ablation studies on Qwen2.5-3B and 7B Base model trained on MATH dataset. The results are reported in Table 4.

Impact of $\sqrt{T}$ length correction. The $\sqrt{T}$ length correction proves crucial for stabilizing the training. As shown in Figure 5(a) and 5(b), removing this length correction causes catastrophic degradation in Qwen2.5-3B-Base, resulting in near-zero accuracy on GSM8K/AMC, a complete loss of transferability to code tasks and the emergence of length hacking. This highlights that the raw gradient norm of token-averaged NLL exhibits a strong length-dependent bias, making the intrinsic signal exploitable and unstable without correction. In contrast, the drop on 7B is smaller, suggesting larger models may provide a less noisy intrinsic signal, but $\sqrt{T}$ correction remains beneficial overall.

Impact of within-group rank shaping. We observe that the necessity of rank shaping correlates with model scale. For the Qwen2.5-3B Base model, the removal of rank shaping results in a trade-off on general tasks, with slight gains on MMLU-Pro offset by losses on IFEval. In contrast, the Qwen2.5-7B Base model relies on it to maintain general capabilities. Notably, removing the rank shaping causes a severe accuracy regression of 8.90% on MMLU-Pro. We attribute this to the instability of the raw gradient-norm reward across prompts—its scale varies substantially and occasionally contains extreme outliers. Rank shaping mitigates this issue by using only within-prompt relative order, preventing a small subset of high-magnitude cases from dominating the updates and steering training away from general knowledge.

Additional ablation results, including normalization variants and a scalable LM-head-only variant, are provided in Appendix C.

5 Conclusions

In this work, we introduce VIGOR, a verifier-free intrinsic reward for RL post-training of LLMs when gold answers or domain verifiers are unavailable. VIGOR uses per-completion gradient norms as reward, favoring low-norm completions for stable optimization, with length correction and within-group rank shaping for robustness. Across math and code reasoning benchmarks, VIGOR improves performance and stabilizes training. We hope VIGOR motivates further work on scalable verifier-free RL post-training.

Limitations

Our study primarily targets verifiable reasoning tasks; it remains unclear how well VIGOR transfers to more open-ended generation settings (e.g., long-form writing or dialogue), where additional constraints may be needed to avoid pathological optimization. In addition, VIGOR requires per-sample gradient-norm computation, which introduces non-trivial automatic-differentiation overhead compared to forward-only signals such as likelihood or entropy, and although our LM-head-only variant largely mitigates this cost, scaling to substantially larger models may still require further approximations. More broadly, gradient norm is only a proxy objective and may not consistently track downstream utility, leaving room for potential misalignment or reward exploitation.

Acknowledgments

This work was supported in part by the "Pioneer" and "Leading Goose" R&D Program of Zhejiang (No. 2025C02032). This work was also supported by Ant Group through the CAAI-Ant Research Fund and the Earth System Big Data Platform of the School of Earth Sciences, Zhejiang University.

References

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

Karl Cobbe, Vineet Kosaraju, Mohammad Bavarian, Mark Chen, Heewoo Jun, Lukasz Kaiser, Matthias Plappert, Jerry Tworek, Jacob Hilton, Reiichiro Nakano, Christopher Hesse, and John Schulman. 2021. Training verifiers to solve math word problems. *Preprint*, arXiv:2110.14168.

DeepSeek-AI, Daya Guo, Dejian Yang, Haowei Zhang, Junxiao Song, Ruoyu Zhang, Runxin Xu, Qihao Zhu, Shirong Ma, Peiyi Wang, Xiao Bi, Xiaokang Zhang, Xingkai Yu, Yu Wu, Z. F. Wu, Zhibin Gou, Zhihong Shao, Zhuoshu Li, Ziyi Gao, and 181 others. 2025. Deepseek-r1: Incentivizing reasoning capability in llms via reinforcement learning. *Preprint*, arXiv:2501.12948.

Alex Gu, Baptiste Roziere, Hugh James Leather, Armando Solar-Lezama, Gabriel Synnaeve, and Sida Wang. 2024. CRUXEval: A benchmark for code reasoning, understanding and execution. In *Forty-first International Conference on Machine Learning*.

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

Hugging Face. 2025. Open r1: A fully open reproduction of deepseek-r1.

Naman Jain, King Han, Alex Gu, Wen-Ding Li, Fanjia Yan, Tianjun Zhang, Sida Wang, Armando Solar-Lezama, Koushik Sen, and Ion Stoica. 2025. Livecodebench: Holistic and contamination free evaluation of large language models for code. In *The Thirteenth International Conference on Learning Representations*.

Jia LI, Edward Beeching, Lewis Tunstall, Ben Lipkin, Roman Soletskyi, Shengyi Costa Huang, Kashif Rasul, Longhui Yu, Albert Jiang, Ziju Shen, Zihan Qin, Bin Dong, Li Zhou, Yann Fleureau, Guillaume Lample, and Stanislas Polu. 2024. Numinamath. [https://huggingface.co/AI-MO/NuminaMath-CoT](https://github.com/project-numina/aimo-progress-prize/blob/main/report/numina_dataset.pdf).

Yujia Li, David Choi, Junyoung Chung, Nate Kushman, Julian Schrittwieser, Rémi Leblond, Tom Eccles, James Keeling, Felix Gimeno, Agustin Dal Lago, Thomas Hubert, Peter Choy, Cyprien de Masson d'Autume, Igor Babuschkin, Xinyun Chen, Po-Sen Huang, Johannes Welbl, Sven Gowal, Alexey Cherepanov, and 7 others. 2022. Competition-level code generation with alphacode. *Science*, 378(6624):1092–1097.

Hunter Lightman, Vineet Kosaraju, Yura Burda, Harri Edwards, Bowen Baker, Teddy Lee, Jan Leike, John Schulman, Ilya Sutskever, and Karl Cobbe. 2023. Let's verify step by step. *Preprint*, arXiv:2305.20050.

Hunter Lightman, Vineet Kosaraju, Yuri Burda, Harrison Edwards, Bowen Baker, Teddy Lee, Jan Leike, John Schulman, Ilya Sutskever, and Karl Cobbe. 2024. Let's verify step by step. In *The Twelfth International Conference on Learning Representations*.

Yen-Ting Lin, Di Jin, Tengyu Xu, Tianhao Wu, Sainbayar Sukhbaatar, Chen Zhu, Yun He, Yun-Nung Chen, Jason Weston, Yuandong Tian, Arash Rahnama, Sinong Wang, Hao Ma, and Han Fang. 2025. Step-kto: Optimizing mathematical reasoning through stepwise binary feedback. *Preprint*, arXiv:2501.10799.

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

Qwen, :, An Yang, Baosong Yang, Beichen Zhang, Binyuan Hui, Bo Zheng, Bowen Yu, Chengyuan Li, Dayiheng Liu, Fei Huang, Haoran Wei, Huan Lin, Jian Yang, Jianhong Tu, Jianwei Zhang, Jianxin Yang, Jiaxi Yang, Jingren Zhou, and 25 others. 2025. Qwen2.5 technical report. *Preprint*, arXiv:2412.15115.

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

Zhihong Shao, Peiyi Wang, Qihao Zhu, Runxin Xu, Junxiao Song, Xiao Bi, Haowei Zhang, Mingchuan Zhang, Y. K. Li, Y. Wu, and Daya Guo. 2024. Deepseekmath: Pushing the limits of mathematical reasoning in open language models. *Preprint*, arXiv:2402.03300.

Kimi Team, Yifan Bai, Yiping Bao, Guanduo Chen, Jiahao Chen, Ningxin Chen, Ruijue Chen, Yanru Chen, Yuankun Chen, Yutian Chen, Zhuofu Chen, Jialei Cui, Hao Ding, Mengnan Dong, Angang Du, Chenzhuang Du, Dikang Du, Yulun Du, Yu Fan, and 150 others. 2025. Kimi k2: Open agentic intelligence. *Preprint*, arXiv:2507.20534.

Xuezhi Wang, Jason Wei, Dale Schuurmans, Quoc Le, Ed Chi, Sharan Narang, Aakanksha Chowdhery, and Denny Zhou. 2023. Self-consistency improves chain

of thought reasoning in language models. *Preprint*, arXiv:2203.11171.

Yinjie Wang, Ling Yang, Ye Tian, Ke Shen, and Mengdi Wang. 2025. Co-evolving llm coder and unit tester via reinforcement learning. *Preprint*, arXiv:2506.03136.

Yubo Wang, Xueguang Ma, Ge Zhang, Yuansheng Ni, Abhranil Chandra, Shiguang Guo, Weiming Ren, Aaran Arulraj, Xuan He, Ziyan Jiang, Tianle Li, Max Ku, Kai Wang, Alex Zhuang, Rongqi Fan, Xiang Yue, and Wenhu Chen. 2024. Mmlu-pro: A more robust and challenging multi-task language understanding benchmark. *Preprint*, arXiv:2406.01574.

LLM-Core Xiaomi, :, Bingquan Xia, Bowen Shen, Cici, Dawei Zhu, Di Zhang, Gang Wang, Hailin Zhang, Huaqiu Liu, Jiebao Xiao, Jinhao Dong, Liang Zhao, Peidian Li, Peng Wang, Shihua Yu, Shimao Chen, Weikun Wang, Wenhan Ma, and 46 others. 2025. Mimo: Unlocking the reasoning potential of language model – from pretraining to posttraining. *Preprint*, arXiv:2505.07608.

Qiying Yu, Zheng Zhang, Ruofei Zhu, Yufeng Yuan, Xiaochen Zuo, Yu Yue, Weinan Dai, Tiantian Fan, Gaohong Liu, Lingjun Liu, Xin Liu, Haibin Lin, Zhiqi Lin, Bole Ma, Guangming Sheng, Yuxuan Tong, Chi Zhang, Mofan Zhang, Wang Zhang, and 16 others. 2025. Dapo: An open-source llm reinforcement learning system at scale. *Preprint*, arXiv:2503.14476.

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

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

Zizhuo Zhang, Jianing Zhu, Xinmu Ge, Zihua Zhao, Zhanke Zhou, Xuan Li, Xiao Feng, Jiangchao Yao, and Bo Han. 2025c. Co-rewarding: Stable self-supervised rl for eliciting reasoning in large language models. *Preprint*, arXiv:2508.00410.

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

Jeffrey Zhou, Tianjian Lu, Swaroop Mishra, Siddhartha Brahma, Sujoy Basu, Yi Luan, Denny Zhou, and Le Hou. 2023. Instruction-following evaluation for large language models. *Preprint*, arXiv:2311.07911.

Yuxin Zuo, Kaiyan Zhang, Li Sheng, Shang Qu, Ganqu Cui, Xuekai Zhu, Haozhan Li, Yuchen Zhang, Xinwei Long, Ermo Hua, Biqing Qi, Youbang Sun, Zhiyuan Ma, Lifan Yuan, Ning Ding, and Bowen Zhou. 2025. Ttrl: Test-time reinforcement learning. *Preprint*, arXiv:2504.16084.

A Additional Training Details

We provide additional details of the training setup and hyperparameters.

A.1 Training Setting

We conduct GRPO training using the Open-R1 framework and its vLLM-based rollout backend for efficient generation (Hugging Face, 2025).

In our setup, we reserve one GPU for vLLM rollout generation. Therefore, for post-training on **MATH**, Qwen2.5-3B-Base trained on 4×A6000 uses 3 training GPUs (3 training processes) and runs 156 optimization steps, while Qwen2.5-7B-Base trained on 8×H800 uses 7 training GPUs (7 training processes) and runs 66 optimization steps. The step counts are chosen according to the available compute budget under each hardware setting. For **CodeContests**, we also train on 4×A6000 (with one GPU reserved for rollout), but use only a 3,200-problem subset and run 66 optimization steps.

Post-training on MATH (main setting). Our main experiments focus on mathematical reasoning. We run full post-training on MATH with both Qwen2.5-3B-Base and Qwen2.5-7B-Base. Table 6 summarizes the training configuration for this setting. For a fair comparison, we keep all hyperparameters identical across methods within this setting, and the only difference lies in reward computation (Table 9).

Post-training on CodeContests (lightweight sanity check). We additionally include CodeContests as a lightweight sanity check to verify that our verifier-free reward remains effective for prompt-only code generation training. To keep training cost controlled, we only post-train Qwen2.5-3B-Base and use a small subset of 3,200 problems from the CodeContests training split. In this setting, we compare *only* two verifier-free methods, INTUITOR and VIGOR, and do not include execution-based RLVR baselines that require task-specific verifiers. Table 8 summarizes the training configuration for this setting. For a fair comparison, we keep all hyperparameters identical across methods within this setting, and the only difference lies in reward computation (Table 9).

Gradient-norm computation. We compute the gradient norm sequentially across the G completions within each prompt. For each completion y_i, we construct the scalar loss $\ell_{\text{mean}}(x, y_i)$ and call

Hyperparameter	Qwen2.5-3B-Base	Qwen2.5-7B-Base
Batch size per GPU	4	4
Gradient Accumulation	32	32
Max prompt length	512	512
Max response length	1024	1024
Train steps	156	66
Learning rate	2e-6	1e-6
Rollouts G	8	8
Clip ratio	0.2	0.2
Warmup schedule	Cosine	Cosine
Warmup ratio	0.1	0.1
KL coefficient β	0.01	0.01
Optimizer	AdamW (β_1=0.9, β_2=0.999, ϵ=10^{-8})	
Training temperature	0.9	0.9

Table 6: Training configuration on **MATH**. All methods (GT-Reward, INTUITOR, and VIGOR) share the same configuration; they differ only in reward computation (Table 9).

Method	Steps	Wall-clock	Min/step	GPU-hours	Avg. mem (GB)
GT-Reward	66	2h34m08s	2.34	20.55	66.23
INTUITOR	66	3h57m35s	3.60	31.68	66.32
VIGOR	66	3h35m00s	3.26	28.67	74.00
VIGOR (LM-head-only)	66	2h47m16s	2.53	22.31	66.25

Table 7: End-to-end training cost for Qwen2.5-7B on 8×H800 (7 training GPUs + 1 vLLM rollout GPU) for 66 optimization steps. Wall-clock includes both vLLM rollouts and GRPO optimization. GPU-hours are computed as 8× wall-clock hours. Avg. mem reports the average GPU memory usage measured by `nvidia-smi`.

`torch.autograd.grad` to obtain $\nabla_\theta \ell_{\text{mean}}$, from which we compute $\|\mathbf{g}\|_2$. This requires G=8 sequential backward passes per prompt. We do not retain computational graphs across completions to control memory usage.

A.2 Training Budget and Resource Usage

We report end-to-end wall-clock time including both vLLM rollouts and GRPO optimization, as well as average GPU memory usage measured by `nvidia-smi`. Table 7 provides a representative example for Qwen2.5-7B trained on 8×H800 for 66 optimization steps.

Compared to GT-Reward, INTUITOR and VIGOR incur additional training cost, taking 3h57m and 3h35m versus 2h34m, respectively. In terms of GPU memory, VIGOR requires higher maximum reserved memory across GPUs (74.0 GB), compared to 66.2–66.3 GB for GT-Reward and INTUITOR. We attribute the higher memory consumption of VIGOR to computing gradient-norm based intrinsic rewards, which requires additional gradient statistics during reward computation. Overall, VIGOR trains slightly faster than INTUITOR, trading off higher GPU memory usage. The LM-head-only variant further reduces wall-clock time to 2h47m and memory usage to 66.25 GB, approaching the cost of GT-Reward.

A.3 Prompt Format

MATH prompts format. We use the Qwen chat template to construct prompts, following a `system–user–assistant` format. We use different system prompts for Qwen2.5-3B-Base and Qwen2.5-7B-Base on MATH. For the 3B model, we apply a more constrained instruction to better regularize the smaller model: "`Let's think step by step and output the final answer within \boxed{}.`" For the 7B model, we use a more general assistant-style instruction: "`You are a helpful AI Assistant, designed to provide well-reasoned and detailed responses. Please provide a step-by-step solution to the following problem.`"

The following shows a concrete example used during 7B training:

```
<|im_start|>system
You are a helpful AI Assistant, designed to
provide well-reasoned and detailed responses.
Please provide a step-by-step solution to the
following problem.<|im_end|>
<|im_start|>user
```

Hyperparameter	Qwen2.5-3B-Base
Batch size per GPU	4
Gradient Accumulation	32
Max prompt length	1024
Max response length	2048
Train steps	66
Learning rate	5e-6
Rollouts G	8
Clip ratio	0.2
Warmup schedule	Cosine
Warmup ratio	0.1
KL coefficient β	0.01
Optimizer	AdamW (β_1=0.9, β_2=0.999, ϵ=10^{-8})
Training temperature	0.9

Table 8: Training configuration on **CodeContests** with Qwen2.5-3B-Base. All methods (GT-Reward, INTUITOR, and VIGOR) share the same configuration; they differ only in reward computation (Table 9).

Method	Reward signal	External requirement
GT-Reward	Outcome reward with an exact-match verifier.	Gold answers / task-specific verifier.
INTUITOR	Self-certainty reward computed from the policy model's likelihood signal.	None.
VIGOR (Ours)	Gradient-norm intrinsic reward with length correction and rank shaping.	None.

Table 9: Reward signals used for GRPO training. Within each setting, all hyperparameters are identical across methods; the only difference lies in reward computation.

Dataset	Problems	Usage
MATH500	500	Math eval
GSM8K	1,319	Math eval
AMC	83	Math eval
LiveCodeBench	1,055	Code eval
CRUX	800	Code eval
IFEval	541	Instruction eval
MMLU-Pro	12,032	Multi-task eval

Table 10: Datasets used for evaluation.

Let $f(x) = 3x^2 - 7$ and $g(f(4)) = 9$. What is $g(f(-4))$?<|im_end|>
<|im_start|>assistant

CodeContests prompt format. For CodeContests, we only post-train Qwen2.5-3B-Base. We construct prompts using the same Qwen chat template in a system–user–assistant format and start generation from the final <|im_start|>assistant marker. The system prompt is designed to elicit code-only outputs: "You are an AI designed to help solve competitive programming problems by generating Python code." The following shows a concrete example used during 3B training:

<|im_start|>system
You are a competitive programming assistant. Write a correct and efficient Python 3 program that reads from stdin and writes to stdout. Output only the code.<|im_end|>
<|im_start|>user
Write a Python function or program that fulfills the task described below. Provide only the code as output.
Task Description: Given three integers A_1, A_2, A_3, print bust if $A_1+A_2+A_3 \geq 22$, otherwise print win.
Input: A_1 A_2 A_3 Output: bust or win.

<|im_start|>assistant

B Additional Evaluation Details

Evaluation Setting. We evaluate the model on the datasets listed in Table 10 using the corresponding evaluation scripts. For all benchmarks except AMC, we use greedy decoding with temperature = 0 and generate one completion per prompt (pass@1). For AMC, since it contains only 83 problems (Table 10), the evaluation variance can be higher; therefore we use temperature = 0.6 and report avg@8 (average over 8 independent samples/runs) for a more stable estimate.

Method	GSM8K	MATH500	AMC	Avg.
INTUITOR	87.19	76.20	35.99	66.46
INTUITOR w/ rank	88.93	74.60	40.36	67.96
VIGOR	88.70	76.20	44.42	69.77

Table 11: Disentangling regularization from the gradient-norm signal on Qwen2.5-7B trained on MATH.

Method	GSM8K	MATH500	AMC	Avg.
VIGOR (Rank)	88.70	76.20	44.42	69.77
VIGOR (Min-Max)	88.70	74.40	43.97	69.02

Table 12: Rank vs. Min-Max normalization on Qwen2.5-7B trained on MATH.

Metrics. We report pass@1 for math and code generation benchmarks (MATH500, GSM8K, LiveCodeBench-v6, CRUX) and accuracy for multiple-choice benchmarks (MMLU-Pro). Instruction following is evaluated with the IFEval metric as defined in its benchmark.

C Additional Ablation Results

We report additional ablation experiments on Qwen2.5-7B-Base trained on MATH to complement the main ablation study in Section 4.4. All variants use the same hyperparameters and differ only in the specified component.

Disentangling regularization from the gradient-norm signal. To verify that VIGOR's improvements are not solely attributable to rank normalization, we apply the same rank-based shaping to INTUITOR's confidence reward. As shown in Table 11, INTUITOR with rank normalization improves from 66.46% to 67.96% Math Avg., but remains below VIGOR (69.77%), confirming that the gradient-norm signal itself is the primary driver of improvement.

Rank vs. Min-Max normalization. We compare rank normalization against Min-Max normalization, which maps rewards linearly to $[-1, +1]$ while preserving within-group magnitude differences. Table 12 shows that rank normalization slightly outperforms Min-Max (69.77% vs. 69.02%), suggesting that discarding magnitude information in favor of bounded, outlier-robust updates is preferable for the high-variance gradient-norm reward.

LM-head-only gradient norm. To reduce computational overhead, we restrict gradient-norm computation to the LM head parameters only. As shown in Table 13, this variant achieves comparable performance (69.53% vs. 69.77% Math Avg.) while substantially reducing memory and wall-clock cost (Table 7), making VIGOR practical for larger models.

Variant	GSM8K	MATH500	AMC	Avg.
Full model	88.70	76.20	44.42	69.77
LM-head only	88.63	75.40	44.57	69.53

Table 13: LM-head-only gradient-norm variant on Qwen2.5-7B trained on MATH.