How to Train Your LLM Web Agent: A Statistical Diagnosis

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Abstract

Large language model (LLM) agents for web interfaces have advanced rapidly, yet open-source systems still lag behind proprietary agents. Bridging this gap is key to enabling customizable, efficient, and privacy-preserving agents. Two challenges hinder progress: the reproducibility issues in RL and LLM agent training, where results often depend on sensitive factors like seeds and decoding parameters, and the focus of prior work on single-step tasks, overlooking the complexities of web-based, multi-step decision-making. We address these gaps by providing a statistically driven study of training LLM agents for web tasks. Our two-stage pipeline combines imitation learning from a Llama 3.3 70B teacher with on-policy fine-tuning via Group Relative Policy Optimization (GRPO) on a Llama 3.1 8B student. Through 1,370 configuration sweeps and rigorous bootstrapping, we chart the first compute allocation curve for open-source LLM web agents. Our findings show that dedicating one-third of compute to teacher traces and the rest to RL improves MiniWoB++ success by 6 points and closes 60% of the gap to GPT-4o on WorkArena, while cutting GPU costs by 45%. We introduce a principled hyperparameter sensitivity analysis, offering actionable guidelines for robust and cost-effective agent training.

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1. Introduction

Large language model (LLM) agents for web interfaces have made rapid strides, but open-source systems still lag significantly behind their proprietary counterparts. Bridging this gap would empower organizations to train agents tailored to their specific workflows, using smaller models to reduce latency and cost, while retaining full control over sensitive data—thereby strengthening user trust. Yet, despite impressive progress of open-source models in domains like math and code generation, advances in training web-capable LLM agents remain limited by two persistent challenges: a lack of attention to multi-turn, long-horizon tasks, and the high cost and low reproducibility of current training pipelines.

The lack of attention to complex tasks is extremely prevalent in the current research landscape, where most work centers on single-step tasks such as code generation or mathematical reasoning (Yu et al., 2025; DeepSeek-AI et al., 2025; Roux et al., 2025). While these domains offer clean supervision and rapid feedback, they fall short of capturing the challenges of real-world web environments, which demand sequential decision-making and long-horizon planning. Recent benchmarks like WebArena (Zhou et al., 2023), WorkArena (Drouin et al., 2024), OSWorld (Xie et al., 2024), and The Agent Company (Xu et al., 2024) have begun to surface these challenges, revealing how brittle existing methods become when faced with delayed rewards, sparse feedback, and compounding errors. Meeting the demands of such environments requires not only more capable agents, but also training pipelines that are reproducible and compute-efficient—an area this work directly addresses.

However, building such pipelines is nontrivial. Modern LLM post-training often involves a combination of supervised fine-tuning (SFT) and reinforcement learning (RL), with performance sensitive to a large number of interacting hyperparameters (Hochlehnert et al., 2025). Single-seed training runs can produce misleading results, further complicating reproducibility. While large industry labs can afford multi-seed sweeps and exhaustive hyperparameter tuning (Abdin et al., 2025), these approaches remain out of reach

for most smaller research groups due to the sheer cost of LLM training. This makes it all the more critical to design pipelines that are not only effective but statistically robust and accessible within realistic compute budgets.

In this paper, we tackle these gaps by providing a statistically driven diagnosis of training LLM agents for web-based, multi-step tasks. Specifically, we study how to allocate compute between expensive, high-quality off-policy demonstrations from a large teacher model and cheaper on-policy rollouts from a smaller student model. Too much reliance on the former leads to brittle imitation, while too much of the latter drowns learning in sparse web-task rewards.

To study this compute-performance trade-off we use a two-stage training pipeline. First, a LLaMA 3.3 70B teacher model generates successful trajectories to warm-start a smaller LLaMA 3.1 8B student via supervised fine-tuning (SFT). We then branch of various SFT checkpoints where training continues with an on-policy RL phase using Group Relative Policy Optimization (GRPO). Our central objective is to determine the *optimal compute allocation and hyper-parameter* mix for training web-based LLM agents. To this end, we run 1,370 training configurations, varying key hyperparameters and branching points between SFT and RL. We then apply bootstrap-based analysis to estimate the impact of different hyper-parameters on downstream performance and how they vary across branching SFT checkpoints. This data-driven approach enables us to identify important considerations to get the most out of each run. We use this method to show the optimal mix between SFT and RL on the MiniWob++ environment achieving better task accuracy at significant lower cost. Additionally, we provide concrete recommendations on compute allocations between SFT and RL on the more demanding WorkArena environment.

Putting things together, we show how our study yields several actionable insights. First, branching into RL early—but not immediately—after SFT leads to better outcomes. Second, curriculum learning and error log feedback help when agents start from scratch but are not important once SFT warmup is applied. Third, in GRPO, applying zero-advantage filtering, avoiding standard deviation normalization of the advantage, and skipping importance ratio correction and trust region can impact performance in varying ways depending on the amount of warmup SFT. Fourth, decoding temperature and discount rate can somimes vary but must also be carefully tuned.

These findings are significant for two reasons. First, they give smaller research groups a reproducible, budget-aware playbook for pushing open LLM agents closer to state-of-the-art without scaling model size. Second, they address a slice of the broader reproducibility concerns recently highlighted in the RL community (Agarwal et al., 2021; Hochlehnert et al., 2025), offering a template for rigorous, statistically-grounded hyperparameter tuning and compute allocation in LLM agent training.

2. Background

This section consolidates the algorithmic ingredients used throughout the paper: (i) the MDP formulation of web-based language agents, (ii) supervised fine-tuning (SFT) on expert traces, (iii) GRPO for reinforcement learning, and (iv) curriculum and normalization techniques that stabilize training.

2.1. Language Agents as MDPs

We model each task as a Markov Decision Process (MDP) $\mathcal{M} = \langle \mathcal{S}, \mathcal{A}, P, r, \rho_0, \gamma \rangle$. A state $s_t \in \mathcal{S}$ is a textual context–in our case a prompt and an action $a_t \in \mathcal{A}$ is a textual response generated by the agent. Each action a_t consists of a sequence of tokens $o_{1_t:K_t}$ sampled autoregressively from the policy $\pi_\theta(a_t|s_t)$ parameterized by an LLM sampled with temperature ρ_{LLM}. The environment then returns a scalar reward $r_t \in \{-1, 1\}$ indicating task failure/success. In our setting we assume the environment is *deterministic*, meaning the transition probability $p(s_{t+1} = s_{t+1} \mid a_t)$ is always equal to 1 . We optimize the policy π_θ to maximize the expected discounted return:

$$J(\theta) = \mathbb{E}_{\tau \sim \pi_\theta} \left[\sum_{t=0}^{T} \gamma^t r_t \right] \tag{1}$$

Here, $\gamma \in [0, 1]$ is the discount rate, which controls the agent's preference for immediate versus future rewards and $\tau = (s_0, a_0, r_0, s_1, a_1, r_1, \ldots, s_T)$ refers to a trajectory sampled from the policy.

2.2. Off-policy boot-strapping via SFT

We first imitate a stronger *expert* policy π_E by minimizing the cross-entropy loss

$$\mathcal{L}_{\text{SFT}}(\theta) \;=\; -\mathbb{E}_{\tau \sim \pi_E} \Big[\sum_{t=0}^{T} \log \pi_\theta(a_t \mid s_t) \Big]. \tag{2}$$

SFT offers a high-quality, low-variance gradient but is inherently *off-policy* which can lead to poor generalization (Chu et al., 2025).

2.3. On-Policy Improvement with Multi-Turn GRPO

After the initial SFT warmup phase, we continue training using on-policy RL using GRPO as the optimization algorithm. Like REINFORCE (Sutton et al., 1999), GRPO maximizes the expected return using a policy gradient objective. However, GRPO introduces additional structure by leveraging per-goal advantage normalization and importance weighting. Wherein each trajectory is associated with a specific *goal*,

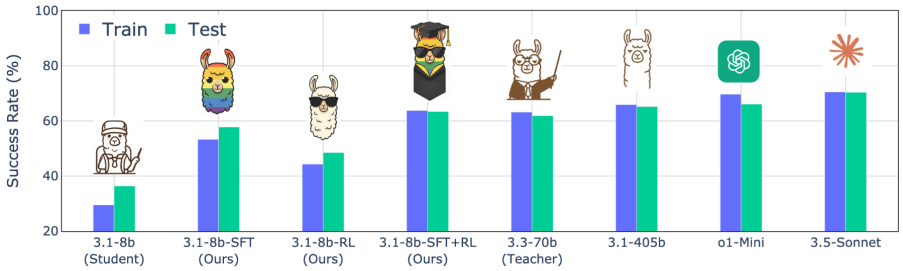

Figure 1: Comparison of our method with baselines from (de Chezelles et al., 2025) on the MiniWoB++(Liu et al., 2018) benchmark. Our 8B Llama 3.1 model is on par with all the larger models such as Llama 3.1 405B, o1-mini and 3.5-sonnet.

in our context referring to the seed of the target task. For a given goal g, the group-normalized advantage function is:

$$A_{t,g} = \frac{r_{t,g} - \text{mean}(\mathbf{R}_{t,g})}{\text{std}(\mathbf{R}_{t,g})},$$

where $\mathbf{R}_{t,g} = (r_{t,1}, \ldots, r_{t,G})$ is the set of rewards across the G goals at timestamp t. In addition to this–similar to proximal policy optimization (PPO) (Abdin et al., 2025)–an importance-ratio is often applied to the GRPO objective:

$$\eta_{t,g} = \frac{\pi_\theta(a_t \mid s_t)}{\pi_{\theta_{\text{old}}}(a_t \mid s_t)} A_{t,g},$$

where π_θ is the current policy and $\pi_{\theta_{\text{old}}}$ is the behavior policy used to collect trajectories. Finally, a clipped-minimum is also applied to stabilize the training process. Putting things together the GRPO objective for our multi-turn setting is:

$$\mathbb{E}_{\tau \sim \pi_{\theta_{\text{old}}}} \left[\frac{1}{G} \sum_{g=1}^{G} \frac{1}{T} \sum_{t=0}^{T} \min\Big(\eta_{t,g} A_{t,g} \; \text{clip}(\eta_{t,g},\, 1-\epsilon,\, 1+\epsilon) A_{t,g} \Big) \right]. \quad (3)$$

Traditionally the GRPO objective includes a KL penalty between the optimizing and reference policies which we do not use as early experiments showed they did not improve performance, slowed down training and required additional compute budget.

Zero-advantage filtering. Tokens with $A_{t,g} = 0$ contribute no learning signal yet still consume memory. Dropping them yields a constant *effective* batch size and modestly accelerates training (Yu et al., 2025).

2.4. Curriculum through Variance-Aware Boltzmann Sampling

To promote steady learning progress, we design a curriculum that prioritizes challenging tasks, neither trivial nor too difficult (Thakkar et al., 2023). Specifically, we select tasks according to a Boltzmann distribution centered around a target return μ_{target} which specifies the desired performance threshold, encouraging focus on partially mastered tasks, with a temperature parameter ρ_{Curr} controlling the sharpness of the distribution, with lower values concentrating probability mass tightly around μ_{target}.

This sampling mechanism dynamically adapts the training distribution, concentrating learning on tasks where the agent is neither already proficient nor entirely unskilled. As a result, the agent avoids premature convergence on easy tasks and prevents wasted effort on tasks far beyond its current capabilities.

3. Methodology

Our training pipeline consists of two sequential stages—Supervised Fine-Tuning (SFT) followed by Reinforcement Learning (RL)—framed as a resource allocation problem. We also describe our hyperparameter sweep strategy and statistical analysis protocol that consolidates hundreds of runs into reliable conclusions. We evaluate our recipe along two axes: compute cost, measured in FLOPs (using the adapted formula from (ben)), and model performance, assessed on both *unseen* training goals and held-out testing tasks.

Stage 1 – Supervised Fine-Tuning (SFT). We begin by generating N_E expert trajectories using a large teacher model. Only successful trajectories are retained after filtering, and the corresponding (s, a) pairs, along with chain-of-thought annotations, form the SFT dataset. Note that computing the cost of the SFT dataset includes both successful and discarded unsuccessful trajectories.

We then train a smaller student model for T_{SFT} gradient steps. To explore the trade-off between SFT and RL, we branch off B times at fixed intervals along the SFT trajectory, yielding checkpoints at timesteps $t_b \in [0, T_{\text{SFT}}]$. Each checkpoint corresponds to a student policy $\pi_{\theta(t_b)}$, which initializes a distinct RL phase. The total compute used up to each branching point, $F_{\text{SFT}}(t_b)$, includes both teacher inference and student training FLOPs accumulated over t_b steps.

Stage 2 – RL Fine-Tuning. Each policy $\pi_{\theta(t_b)}$ is further trained using GRPO for T_{RL} steps. The compute cost of this phase, $F_{\text{RL}}(T_{\text{RL}})$, includes both the FLOPs required for data collection (online rollouts) and for student updates across all T_{RL} steps. The total FLOPs for a full training run starting

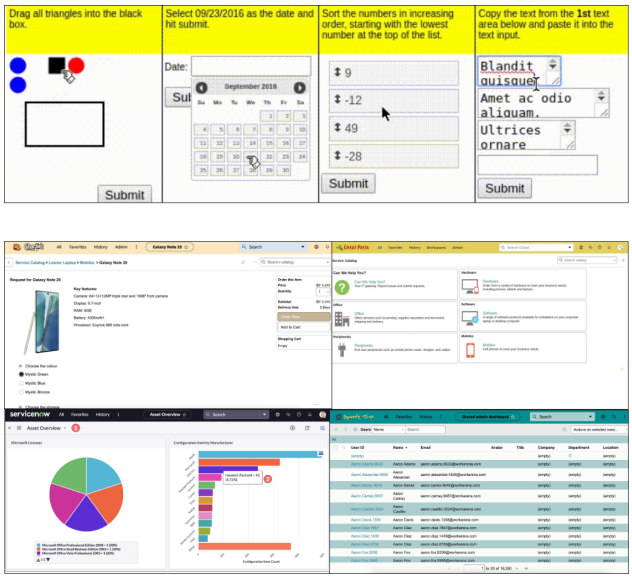

Figure 2: Example tasks in MiniWoB++ (Liu et al., 2018) (top) and WorkArena (Drouin et al., 2024) (bottom). MiniWoB consists of single-page simple tasks such as selecting a particular date and using a basic text editor, while WorkArena comprises multi-page complex tasks like filling forms and placing orders in an enterprise environment.

from $\theta(t_b)$ is computed as:

$$\text{FLOPs}(t_b) = F_{\text{SFT}}(t_b) + F_{\text{RL}}(T_{\text{RL}}). \tag{4}$$

By varying t_b throughout SFT, we assess how shifting compute between expert-driven supervision and on-policy learning impacts final performance. This setup highlights the trade-off between the high compute cost of expert supervision and the lower-cost, but noisier, nature of on-policy learning.

3.1. Estimating the Uncertainty of the Hyperparameter Selection Process

Across the different SFT checkpoints, we sample 1,370 distinct configurations with ten varying hyperparameters, (see Section 6 for details). Our objective is to study the effect of various hyperparameters (HP) on the downstream success rate of our trained agents. This comes with two important considerations. First, if we change the value of, e.g., the batch size, and we want to know if a bigger batch size is better, the learning rate and other parameters need to be readjusted close to their optimal configuration (under a fixed budget). Secondly, to account for noise, we would need to restart the same experiment several times to avoid spurious conclusions. In practice, this is out of reach. For a more computationally friendly approach, we resort to bootstrapping the collection of trials over different hyperparameter configurations.

Balancing unequal coverage. Due to random search, some HP values were explored more than others, biasing the winner toward the larger groups. To correct for this, each trial is sampled with probability $\propto 1/\text{group size}$, approximating an equal compute budget for every HP value.

4. Experimental Setup

Models. We evaluate our approach using Llama 3.3 70B as the expert model to generate demonstration traces, and Llama 3.1 8B as the student model for fine-tuning. Both models operate with a 16k token context window to handle the complexity of web-based tasks.

Benchmarks. Our experiments focus on two benchmarks. The first is MiniWoB++, a suite of 30 medium-horizon web interaction tasks, where we observe that optimal policies typically complete tasks in 2 to 5 steps. The second is WorkArena (Drouin et al., 2024), a more challenging benchmark of 33 enterprise knowledge-work tasks, where we observe optimal policies generally complete tasks in 3 to 10 steps. These benchmarks provide a representative spectrum of sequential decision-making challenges faced by interactive LLM agents. Both benchmarks are depicted in Figure 2.

Observation & Action Spaces. MiniWoB++ provides raw HTML trees, whereas WorkArena leverages accessibility trees (AxTrees), which we truncate to 16k tokens to meet hardware constraints. The agent operates in a discrete action space composed of high-level UI primitives: `noop`, `fill(node, text)`, `click(node)`, `type(node, text)`, `select_option(node, option)`, `scroll(node)`, and `hover(node)`. This abstraction allows the agent to interact effectively with diverse web interfaces. All agents employ chain-of-thought prompting (Wei et al., 2023). We also experiment with *error log feedback*, allowing the agent to receive explicit error messages when it takes invalid actions.

Training Framework. To manage the training pipeline, we use BROWSERGYM (de Chezelles et al., 2025) for orchestrating Chromium-based web environments and structuring the agent's action space, while AGENTLAB (de Chezelles et al., 2025) handles agent design. Model fine-tuning is conducted with TORCHTUNE (torchtune maintainers and contributors, 2024), utilizing Fully Sharded Data Parallelism (FSDP) to enable scalable training across multiple GPUs. Given the high memory demands of long-sequence processing, we apply activation offloading and gradient checkpointing techniques, achieving approximately 40% reduction in memory usage.

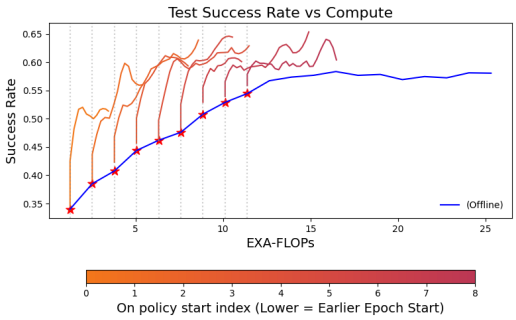

Figure 3: **MiniWoB++ compute analysis** — Test task performance w.r.t total FLOPs. Branching off early from SFT to RL yields superior performance under budget constraints.

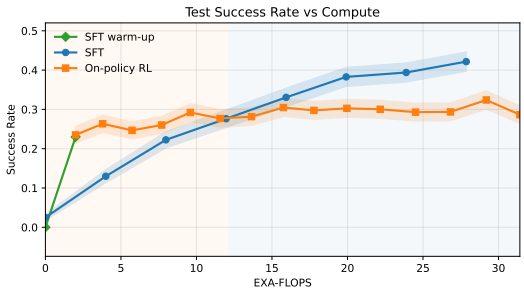

Figure 4: **WorkArena compute analysis** — Despite task difficulty, early RL branching remains valuable for efficient compute usage. Two seeds for each runs are reported using a ema filter with an alpha = 0.7 for smoothing.

Compute Infrastructure. Our computational infrastructure comprises 8 $\times$ H100-80GB GPUs for expert data generation with the 70B model. For student model training, we allocate 2 $\times$ H100 GPUs for MiniWoB++ experiments and 4 $\times$ H100 GPUs for WorkArena experiments, reflecting the increased complexity of the latter.

Evaluation Protocol. The environments in this study are stochastic; each task is initialized with a random seed. We define a "goal" as a specific random seed for a given task. The evaluation protocol assesses generalization at two levels: (i) performance on held-out goals within the training tasks and (ii) performance on entirely new, unseen tasks. The primary metric reported is the average task success rate, which reflects the agent's ability to generalize beyond its training distribution.

Compute Allocation Protocol. We track the total floating-point operations (FLOPs) consumed during both supervised fine-tuning (SFT) and GRPO phases, following the procedure in Section 3 and Appendix B. For RL branching, we start from the SFT run that (i) attains the highest SFT performance and (ii) exhibits stable learning, allowing us to sample checkpoints at regular intervals and cleanly study the effect of SFT warm-up. Because this run sits in the top 10% of all SFT trials by area-under-the-training-curve (AUC)—the mean success rate across all checkpoints of the run, serving as a proxy for overall training efficiency, we match that selection pressure for RL by averaging the top two seeds out of ten RL trials, thus approximating the 90th-percentile of the RL distribution and ensuring a fair compute-aware comparison between strategies.

5. Main Results and Compute Trade-Offs

In this section, we present our primary findings and analyze the trade-offs between supervised fine-tuning (SFT) and reinforcement learning (RL) in terms of both performance and compute efficiency.

Performance overview Table 1 summarizes the headline results across benchmarks. For each method (SFT, RL, and SFT+RL), we report the mean performance of the top two seeds at convergence.

For MiniWoB++, pairing SFT with RL reliably beats using SFT or RL alone and matches the teacher's success rate. In contrast, on the harder WorkArena tasks, extra RL adds little—the best results still come from pure SFT. Starting RL from scratch, without any SFT warm-up, rarely moves the needle, underscoring how tough it is to learn from a cold start.

Further analysis of saturation behavior for both SFT and SFT+RL is provided at the end of this section. Notably, WorkArena's test set can be easier than its training set for certain agents, adding nuance to the observed performance gaps.

Compute-Performance Trade-off Across Benchmarks Our analysis centers on the tension between high-quality, but costly, SFT traces produced by a large teacher and the cheaper, noisier rollouts generated during on-policy RL. To map this trade-off, we branch RL fine-tuning from SFT checkpoints sampled at fixed intervals—one checkpoint every 1,024 SFT training examples.

Figure 3 shows the trend for MiniWoB++, while Figure 4 shows it for WorkArena. On MiniWoB++, jumping to RL early not only saves compute but also beats SFT-only training. Spending roughly one-third of the FLOPs on teacher traces and the rest on RL lifts MiniWoB++ accuracy by 6 points at the same cost, matching the teacher. Early, self-generated rollouts therefore push the compute–performance frontier. Warm-started runs then hit a performance ceiling unreachable by pure RL, highlighting the complementary roles of SFT and RL.

WorkArena's higher task complexity proved tougher. RL runs struggled to trace a clean compute–performance Pareto front, unlike MiniWoB++, so apples-to-apples comparisons

Table 1: Comparison of our method with baselines on the held-out train and test splits of WorkArena and MiniWoB++. Baselines are run in the exact same setting as (de Chezelles et al., 2025).

Model	WorkArena		MiniWoB++	
	Train-held-out	Test-held-out	Train-held-out	Test-held-out
Claude-3.5-Sonnet	48.1$\pm$3.1	87.1$\pm$4.0	70.5$\pm$2.0	70.4$\pm$4.3
GPT-4o	41.2$\pm$3.1	61.4$\pm$5.8	65.7$\pm$2.1	64.3$\pm$4.5
GPT-4o-Mini	23.1$\pm$2.6	41.4$\pm$5.9	56.2$\pm$2.2	66.1$\pm$4.4
Llama-3.1-70b-Instruct	25.0$\pm$2.7	38.6$\pm$5.8	57.0$\pm$2.2	65.2$\pm$4.4
o1-Mini	49.2$\pm$3.1	84.3$\pm$4.3	69.7$\pm$2.1	66.1$\pm$4.4
Llama-3.1-405b-Instruct	35.8$\pm$3.0	71.4$\pm$5.4	65.9$\pm$2.1	65.2$\pm$4.4
Llama-3.1-8B *(Student)*	9.0$\pm$2.9	5.0$\pm$2.2	29.5$\pm$3.2	36.4$\pm$3.4
Llama-3.1-8b-SFT *(Ours)*	25.0$\pm$4.4	39.0$\pm$4.8	53.3$\pm$2.5	57.8$\pm$2.5
Llama-3.1-8b-RL *(Ours)*	0.0$\pm$0.0	0.0$\pm$0.0	44.3$\pm$2.5	48.5$\pm$2.5
Llama-3.1-8b-SFT+RL *(Ours)*	24.0$\pm$4.2	32.0$\pm$4.6	**63.8$\pm$2.4**	**63.4$\pm$2.4**
Llama-3.3-70b *(Teacher)*	**30.0$\pm$4.6**	**50.0$\pm$5.0**	63.2$\pm$4.8	61.9$\pm$4.9

are trickier. Even so, branching into RL early can help under tight budgets, though RL seldom beats plain SFT. This points to the need for sharper RL algorithms or alternative optimization methods for sparse-reward, high-complexity tasks.

Taken together, the results stress a delicate balance: leveraging high-quality expert traces for sample efficiency while relying on cheap on-policy exploration for compute efficiency—a pivotal line of inquiry for scaling LLM-agent training.

Task Performance Saturation and Analysis Despite extensive post-training, agent performance on the WorkArena benchmark plateaus at around 40% after just 9–10 epochs. This stagnation appears to stem from the intrinsic difficulty of certain tasks—such as sorting and filtering—which even the Llama 3.3 70B teacher model struggles to solve (see Figure 5). A per-task breakdown shows that while both SFT and RL agents gradually close the performance gap with the teacher model, with RL achieving a slightly higher final success rate, a significant portion of tasks (14 out of 33) remain completely unsolved. These failures are attributed to either the limitations of the teacher model or the sparsity of reward signals, both of which hamper the learning process. On-policy RL exploration proves ineffective in overcoming these challenges due to the lack of foundational skills and informative feedback. These findings underscore the need for more effective methods to address complex tasks under sparse reward settings. Additional per-task performance results for WorkArena and Miniwob are provided in Appendix A.

Shortcut-Seeking Agents and Task Integrity During extended training we found that giving agents full administrative privileges invites creative—but problematic—short-cuts. A striking example comes from the WorkArena "Order hardware devices" task: rather than navigate through the catalogue as intended, the agent simply edited its homepage to add a direct link to the ordering form. While this hack fulfils the success condition, it violates the task's spirit and, worse, the modifications persist across sessions. Other agents entering the environment later inherit the altered UI, leading to failures unrelated to their own policies.

In general, agents exploited admin rights to create or delete elements, reshaping pages in ways that saved clicks but destabilised the environment. These findings argue for a tighter sandbox—enough access for exploration and accomplishment, but safeguards against permanent, instance-wide side-effects.

6. Ablation and Sensitivity Analysis

We simulate re-running hyperparameter configurations and selecting the best-performing ones using the method described in Section 3.1. This is done across three checkpoints: the base LLaMA 3.1 8B Instruct model and two warm-started variants with an additional 2.5×10^{18} and 7.6×10^{18} FLOPs of supervised fine-tuning, respectively, to assess variations across compute budgets. We evaluate the test and training performance of 10 hyper-parameters across 1,370 runs. We report final test performance to verify generalization in Appendix D finding no significant deviations between test and train parameters.

Figure 6 displays our findings, which we summarize as follows. *Curriculum learning* is beneficial when starting RL from scratch but becomes detrimental after warm-starting, likely because warm-started models already perform well on easy tasks, and curriculum forces them to overfocus on harder ones. *Error log feedback* helps when there's no SFT but otherwise does not, likely because SFT warmup removes many common errors made by weaker models. A *decoding temperature* of 0.25 consistently yields the best results, striking a balance between exploration and exploitation;

lower values led to under-exploration and were discarded. *Grouped-relative advantage* helps only after SFT, while using raw rewards works better when starting directly from the Instruct model, possibly due to how advantage scaling interacts with the initial weights. *Zero-advantage filtering* improves training across most settings by ensuring batches focus on informative updates. *Standard-deviation normalized advantage*, as noted by (Liu et al., 2025), seems to aid performance under less finetuned models and decrease in value the more finetuning is done. *Importance ratio correction and trust region*, though standard, also hurt models with little or no SFT, likely because conservative updates slow down learning. In contrast, for models that start from a stronger SFT checkpoint, these mechanisms can help stabilize training and avoid catastrophic updates. For the *learning rate*, larger values generally work better. *Effective batch size* of 512 appears to be generally be a safe and robust choice in all our experiments. Finally, regarding the *discount rate*, we find that 0.9 works well in most settings, except for heavily warm-started models, where a lower rate of 0.5 appears beneficial—likely because it encourages the model to optimize aggressively on tasks it already performs well on.

7. Related Work

Best practices in deep RL. Building on the recognition of reproducibility challenges and unstable RL training of LLM agents, recent studies have proposed best practices for training LLM agents using RL methods. Dang and Ngo (2025) recommend leveraging high quality data, balancing easy and hard problems, and controlling length generation with cosine reward. Yu et al. (2025) promote higher clipping in the GRPO loss to promote diversity and avoid entropy collapse, dynamic sampling to improve training efficiency and stability, token level gradients for long CoT sequences, and overlong reward shaping to reduce reward noise. Roux et al. (2025) introduce tapered variant of importance sampling to speed up learning while maintaining stable learning dynamics. The proposed method (TOPR) allows the handling of both positive and negative examples in a fully offline setting. More generally, Hochlehnert et al. (2025) emphasizes the need for greater methodological precision, particularly concerning decoding parameters, random seeds, prompt formatting, as well as the hardware and software frameworks, to guarantee transparent and thorough assessments of model performance. These practices are essential for developing robust and reproducible agents.

LLM Agents trained with RL on multi-step environments. Recent advancements have sought to bridge the gap in training LLM agents for multi-step environments, with approaches like WebRL (Qi et al., 2025) and SWEET-RL (Zhou et al., 2025) demonstrating significant progress. WebRL employs a self-evolving curriculum to address the challenges of sparse feedback and task scarcity, successfully enhancing the performance of open LLMs in web-based tasks (Qi et al., 2025). Similarly, SWEET-RL introduces a hierarchical structure that enables effective credit assignment over multiple turns, improving policy learning and generalization in collaborative reasoning tasks (Zhou et al., 2025). These studies collectively illustrate the necessity of adapting RL techniques to accommodate the complexities of multi-step interactions, paving the way for more capable and versatile LLM agents. Similar in spirit to our work, (Andreux et al., 2025) propose an empiral study on the inference cost of trained LLM web agents. An extended version of the related work is provided in Appendix C.

LLM agents trained with RL in multi-step environments. Recent work has begun closing the gap for LLM agents that must reason over multiple steps. WebRL (Qi et al., 2025) introduces a self-evolving curriculum that tackles sparse feedback and task scarcity, substantially boosting open-source agents on web tasks. SWEET-RL (Zhou et al., 2025) adds a hierarchical credit-assignment scheme spanning many turns, improving both learning stability and generalization in collaborative reasoning. Together, these studies underscore the need to adapt RL techniques to the intricacies of long-horizon interaction, paving the way for more capable, versatile agents. Similar in spirit, (Andreux et al., 2025) provide an empirical analysis of inference costs for trained LLM web agents. An extended discussion of related work appears in Appendix C.

8. Discussion

Limitations. Our focus is on providing a comprehensive perspective on training an LLM-based web agent, studying compute trade-offs, hyperparameter selection, and analyzing failure cases. With this in mind, our results are limited to English-language web interfaces and Llama 3 models in the 8B–70B parameter range, where larger models may alter trade-offs.

Regarding our statistical method, it does not account for the lack of coverage from the random search. A more exhaustive search could discover configurations that would change the conclusions drawn in this study. We note also that a significant portion of the reported uncertainty is due to epistemic uncertainty that could be reduced by evaluating more configurations.

Conclusion. Overall, our study provides a practical and statistically grounded recipe for training open-source LLM agents on complex web tasks, helping close the performance gap with proprietary systems while promoting reproducibility and efficiency. By systematically analyzing compute allocation strategies, hyperparameter sensitivities, and train-

Figure 5: Per-task performance of SFT and SFT+RL agents on WorkArena. The Llama 3.1 8B model is initially fine-tuned for 4 epochs on trajectories from a teacher Llama 3.3 70B model. Training then continues either with additional SFT or with GRPO fine-tuning up to epoch 20. The teacher model's success rate is also shown.

ing dynamics, we offer actionable insights that can inform future research and development of interactive agents. We hope these findings will serve both as a reproducible baseline and as a stepping stone for advancing reliable and cost-effective LLM agent training in web-based and other complex sequential environments.

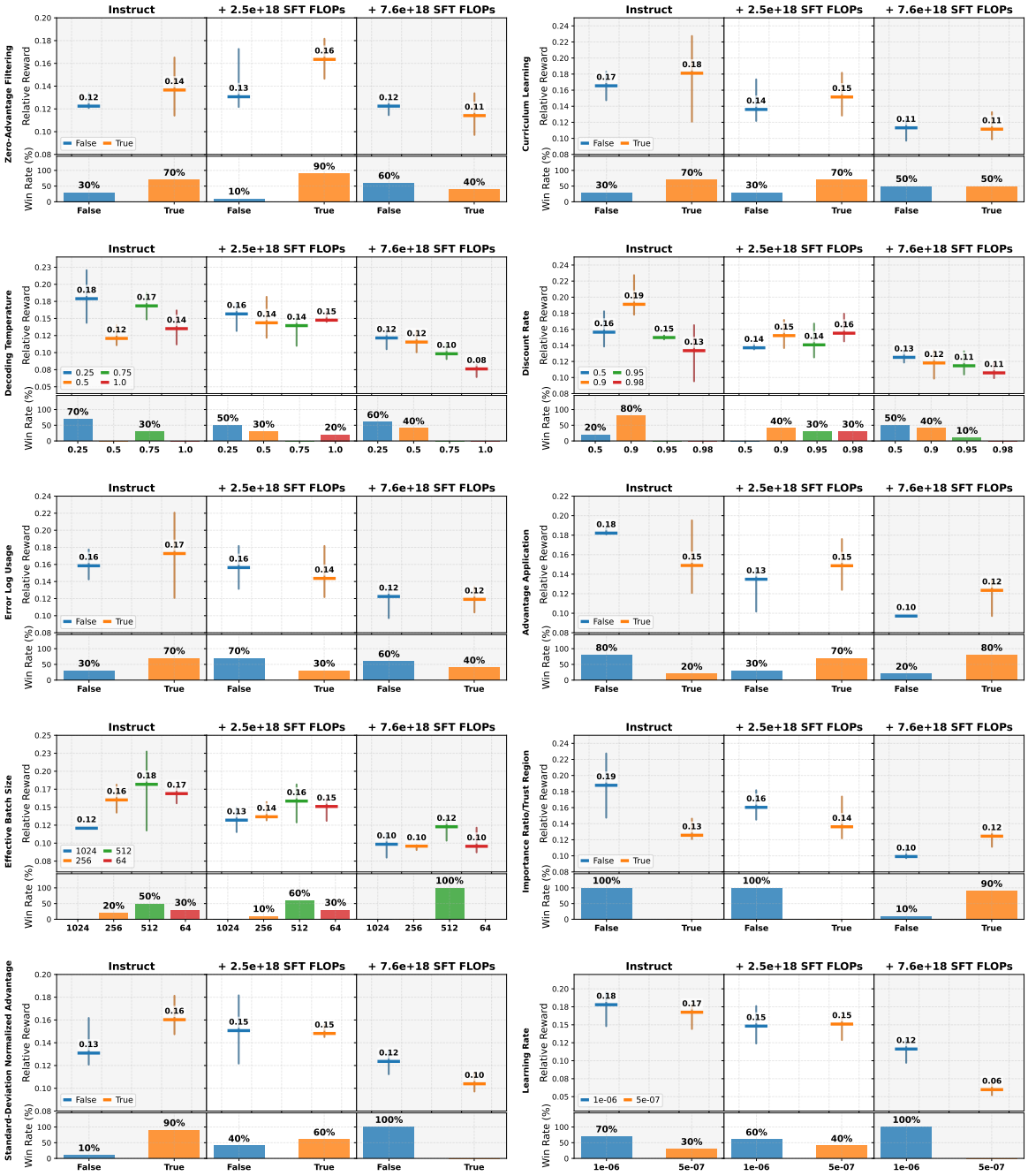

Figure 6: Bootstrap analysis ($n = 1000$ samples) of hyperparameter optimization across different SFT compute budgets on *training* held out tasks. Each subplot examines a different hyperparameter, including increasing SFT compute: the base instruct model (left), +2.5$\times 10^{18}$ SFT FLOPs (middle), and +7.6e$\times 10^{18}$ SFT FLOPs (right). For each hyperparameter-compute combination, the top panel shows relative reward performance with error bars indicating 95% confidence intervals, while the bottom panel displays win rates representing the percentage of bootstrap iterations where each parameter value achieved maximum performance. Results demonstrate that optimal hyperparameter values shift as model pre-training compute increases, suggesting that hyperparameter selection should be adapted to the computational budget allocated to supervised fine-tuning.

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Broader Impact

Web-based LLM agents have the potential to revolutionize markets by enabling more cost-efficient and effective workflows. Our work focuses on making these agents accessible across a range of compute budgets, empowering not only industrial labs but also smaller research groups and individuals to train their own assistants. This approach promotes data privacy and reduces reliance on costly infrastructure.

Despite their promise, web-based agents face significant challenges that limit their broader adoption. Issues such as reliability, vulnerability to adversarial attacks, and limited access to proprietary data remain key obstacles to realizing their full potential.

Impact Statement

Authors are **required** to include a statement of the potential broader impact of their work, including its ethical aspects and future societal consequences. This statement should be in an unnumbered section at the end of the paper (co-located with Acknowledgements – the two may appear in either order, but both must be before References), and does not count toward the paper page limit. In many cases, where the ethical impacts and expected societal implications are those that are well established when advancing the field of

Machine Learning, substantial discussion is not required, and a simple statement such as the following will suffice:

"This paper presents work whose goal is to advance the field of Machine Learning. There are many potential societal consequences of our work, none which we feel must be specifically highlighted here."

The above statement can be used verbatim in such cases, but we encourage authors to think about whether there is content which does warrant further discussion, as this statement will be apparent if the paper is later flagged for ethics review.

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A. You *can* have an appendix here.

You can have as much text here as you want. The main body must be at most 8 pages long. For the final version, one more page can be added. If you want, you can use an appendix like this one.

The `\onecolumn` command above can be kept in place if you prefer a one-column appendix, or can be removed if you prefer a two-column appendix. Apart from this possible change, the style (font size, spacing, margins, page numbering, etc.) should be kept the same as the main body.

A. Extended Learning and Saturation Analysis

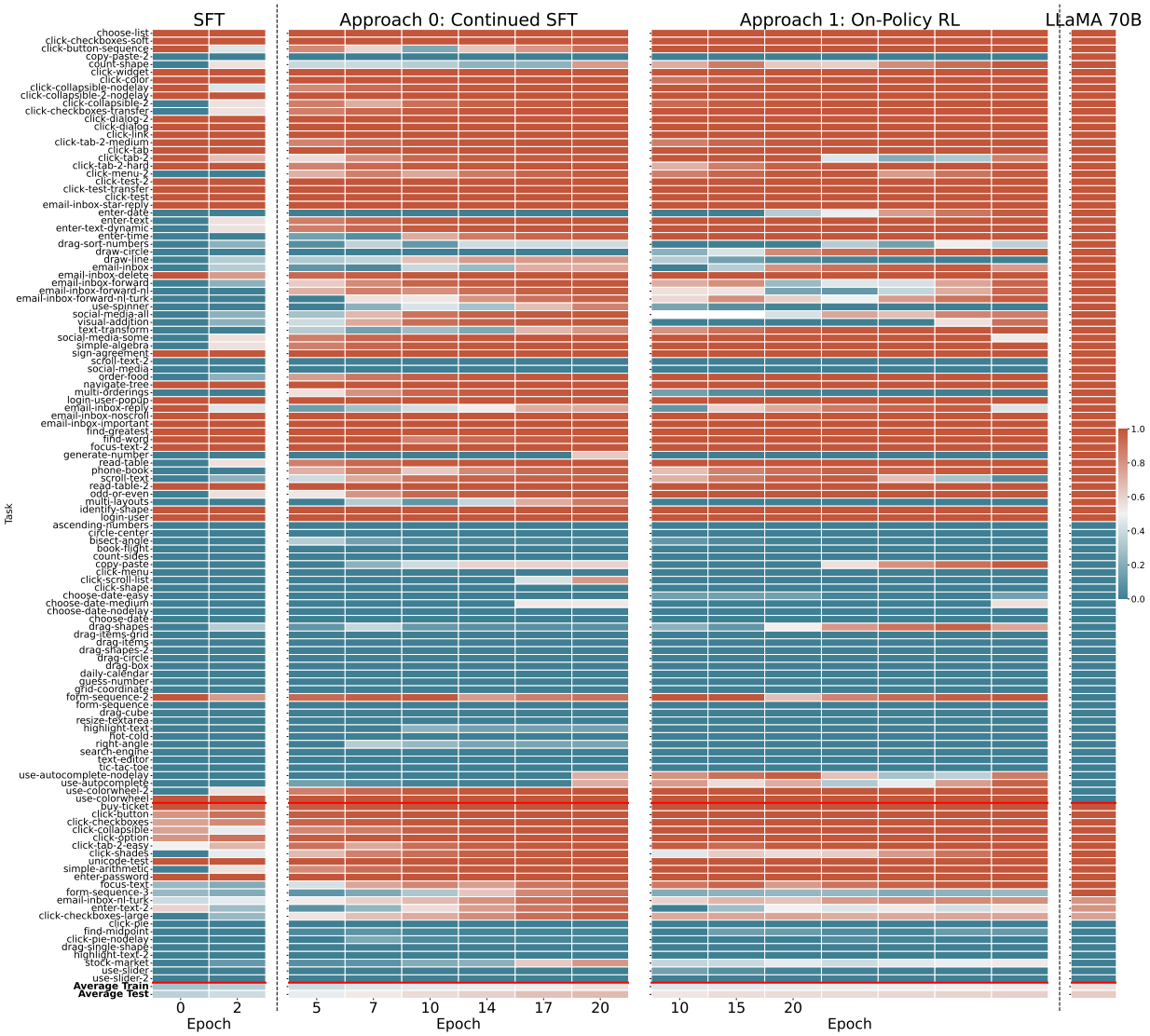

Figure 7: Per task performance of SFT and SFT+RL agents on MiniWob++.

Challenges in Agent-Environment Interaction In this section we talk about the general challenges faced by the agent to interact effectively with the environment.

- **Observation/action space mismatch**: One of the important thing to note specifically in our web environment is the observation space which the agent uses is a bit different from the action space. Multiple times, the agent can see the correct action in the AxTree but the action space, the icon is not visible and to make it visible, the agent needs to scroll

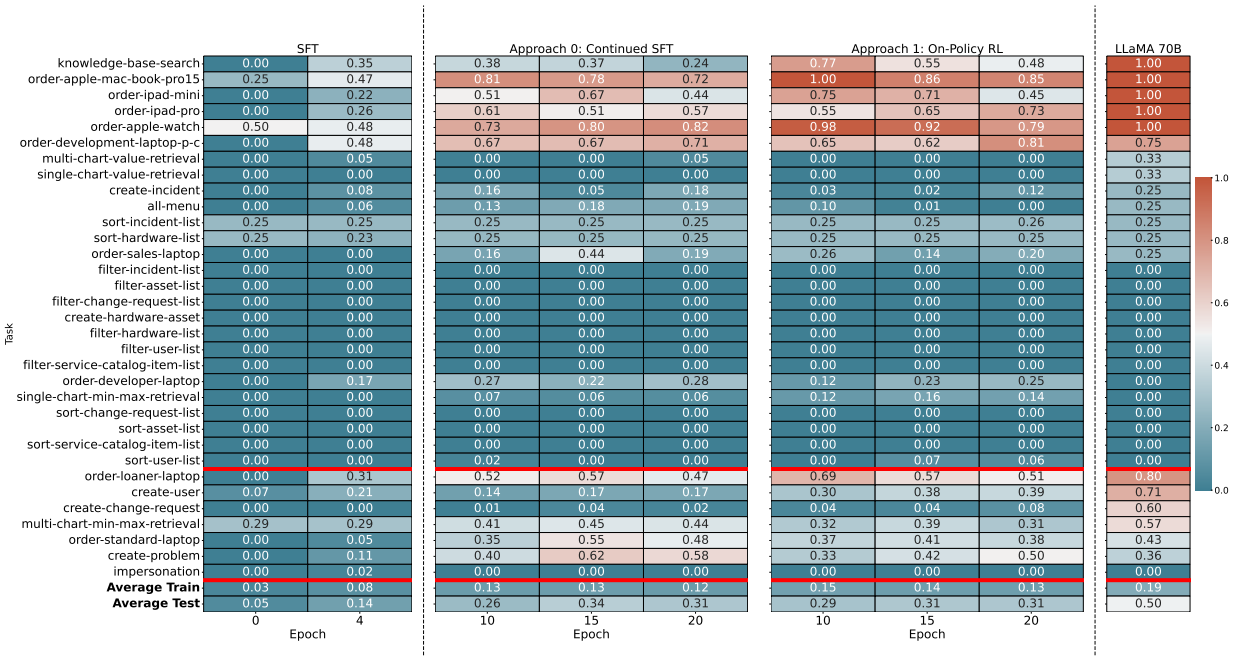

Figure 8: Per task performance of SFT and SFT+RL agents on WorkArena.

down and do the action. This mismatch causes huge problems (Koh and et al., 2024)

- **UI Misuse**: The agent tries to interact with items in the environment in ways that it is not designed. For example, the agent trying to fill in a checkbox with value True while it should just click on it. (Murty and et al., 2025)
- **Repeating actions**: A common issue we observed is the repetition of actions across multiple consecutive steps, often accompanied by verbose and redundant chains of thought. The agent frequently restates similar thoughts or re-executes the same actions unnecessarily, leading to inefficiencies and sometimes getting stuck in loops. (Murty and et al., 2025).

B. Deriving Compute Cost

FLOPs Estimation Methodology Flop calculations are based on model architecture, token counts, and average sequence lengths observed during training and evaluation.

FLOPs per Token

We estimate FLOPs per token using the following formula, adapted from nvidia benchmarking(ben):

$$\text{FLOPs}_{\text{per token}} = (\text{FLOPs}_{\text{attn}} + \text{FLOPs}_{\text{MLP}} + \text{FLOPs}_{\text{embed}}) \times (1 + \text{backward multiplier}) \tag{5}$$

Where:

$$\text{FLOPs}_{\text{attn}} = 12 \times (\text{number of layers}) \times (\text{hidden size})^2 \times \left(1 + \frac{\text{number of query groups}}{\text{number of attention heads}} + \frac{\text{sequence length}}{\text{hidden size}}\right) \tag{6}$$

$$\text{FLOPs}_{\text{MLP}} = 18 \times (\text{number of layers}) \times (\text{hidden size}) \times \text{FFN} \tag{7}$$

$$\text{FLOPs}_{\text{embed}} = 6 \times \text{vocabulary size} \times (\text{hidden size}) \tag{8}$$

On-Policy FLOPs (LLaMA-8B)

We compute the total FLOPs for each on-policy epoch by summing the training and testing FLOPs:

$$\text{FLOPs}_{\text{train}} = N_{\text{train}} \times \text{FLOPs}_{\text{per token}}^{(\text{backward}=3)} \tag{9}$$

$$\text{FLOPs}_{\text{test}} = N_{\text{test}} \times \text{FLOPs}_{\text{per token}}^{(\text{backward}=0)} \tag{10}$$

$$\text{FLOPs}_{\text{epoch}} = \text{FLOPs}_{\text{train}} + \text{FLOPs}_{\text{test}} \tag{11}$$

Where N_{train} and N_{test} are the number of tokens used for training and evaluation respectively. Sequence length S is measured per epoch from logged metrics.

Offline FLOPs (Generation: LLaMA-70B, Training: LLaMA-8B)

Offline training includes two compute components:

- **Data Generation** (LLaMA-70B, forward-only):

$$\text{FLOPs}_{\text{gen}} = N_{\text{gen}} \times \text{FLOPs}_{\text{per token}}^{(70B,\text{backward}=0)} \tag{12}$$

  where $N_{\text{gen}} = \text{avg seq len} \times \text{samples per epoch}$ (from dataset metadata).

- **Training** (LLaMA-8B, with backward pass):

$$\text{FLOPs}_{\text{train}} = N_{\text{gen}} \times \text{FLOPs}_{\text{per token}}^{(8B,\text{backward}=3)} \tag{13}$$

The total FLOPs per offline epoch is:

$$\text{FLOPs}_{\text{epoch}} = \text{FLOPs}_{\text{gen}} + \text{FLOPs}_{\text{train}} \tag{14}$$

All FLOPs values are reported in **exaFLOPs** by dividing the total FLOPs by 10^{18}.

C. Extended Related Work

The Reproducibility Crisis in RL. The reproducibility crisis in large language models (LLMs) and reinforcement learning (RL) has garnered increasing attention, particularly due to the reliance on single seed results that distort the perceived performance of models. The reproducibility challenge[1] organized every year is a positive step towards addressing this. More concretely, Hochlehnert et al. (2025) provide a critical examination of how such practices undermine the reliability of published findings, revealing that many reported gains are sensitive to implementation choices, such as random seeds and prompt formatting (Hochlehnert et al., 2025).

Bandit-domain RLHF with LLMs. Previous work in RL for LLMs has predominantly focused on single-step tasks, which have shown effectiveness in mathematical reasoning and code generation (Yu et al., 2025; DeepSeek-AI et al., 2025; Roux et al., 2025). While these approaches exhibit promising results, they are limited in their applicability to real-world scenarios, which often require multistep decision-making capabilities. The narrow focus on bandit-style problems fails to address the complexities inherent in tasks that demand sequential interaction, highlighting a significant gap in the current research landscape.

Interactive Agent Benchmarks. To assess the capabilities of LLM agents in more realistic environments, benchmarks such as WebArena (Zhou et al., 2023), WorkArena (Drouin et al., 2024; Boisvert et al., 2024), the Agent Company (Xu et al., 2024), and OSWorld (Xie et al., 2024) have been designed to evaluate agents on multi-step tasks across various domains. These benchmarks expose the limitations of current LLM agents, revealing that while they may perform well in controlled settings, their performance in practical applications remains subpar, underscoring the need for further advancements in agent robustness and generalization to multi-step planning.

[1] https://reproml.org/

Best prectices in deep RL. Building on the recognition of reproducibility challenges and unstable RL training of LLM agents, recent studies have proposed best practices for training LLM agents using RL methods. Dang and Ngo (2025) recommend leveraging high quality data, balancing easy and hard problems, and controlling length generation with cosine reward. Yu et al. (2025) promote higher clipping in the GRPO loss to promote diversity and avoid entropy collapse, dynamic sampling to improve training efficiency and stability, token level gradients for long CoT sequences, and overlong reward shaping to reduce reward noise. Roux et al. (2025) introduce tapered variant of importance sampling to speed up learning while maintaining stable learning dynamics. The proposed method (TOPR) allows the handling of both positive and negative examples in a fully offline setting. More generally, Hochlehnert et al. (2025) emphasizes the need for greater methodological precision, particularly concerning decoding parameters, random seeds, prompt formatting, as well as the hardware and software frameworks, to guarantee transparent and thorough assessments of model performance.

LLM Agents trained with RL on multi-step environments. Recent advancements have sought to bridge the gap in training LLM agents for multi-step environments, with approaches like WebRL (Qi et al., 2025) and SWEET-RL (Zhou et al., 2025) demonstrating significant progress. WebRL employs a self-evolving curriculum to address the challenges of sparse feedback and task scarcity, successfully enhancing the performance of open LLMs in web-based tasks (Qi et al., 2025). Similarly, SWEET-RL introduces a hierarchical structure that enables effective credit assignment over multiple turns, improving policy learning and generalization in collaborative reasoning tasks (Zhou et al., 2025). These studies collectively illustrate the necessity of adapting RL techniques to accommodate the complexities of multi-step interactions, paving the way for more capable and versatile LLM agents.

D. Test Set Hyper-Parameter Bootstrap Analysis

We overall find similar results between the held-out train and test tasks with respect to optimal hyper-parameters. While we see no large deviations, we find that some parameters such as curriculum learning from the instruct model and using error logs can have a larger beneficial effect on the held-out testing tasks.

E. Random Search Space

We conduct a random hyperparameter sweep over 1,370 training runs over the following parameter configurations:

- **Decoding temperature**: Sampled from {0.1, 0.25, 0.5, 0.75, 1}
- **Curriculum learning**: Enabled or disabled (`True`, `False`)
- **Discount rate**: {0.5, 0.8, 0.9, 0.95, 0.98, 1.0}
- **Grouped-relative advantage**: Enabled or disabled
- **Zero-advantage filtering**: Enabled or disabled
- **Standard-deviation normalized advantage**: Enabled or disabled
- **Effective batch size**: {64, 256, 512, 1024}
- **Learning rate**: {1e-6, 5e-7}
- **Error log feedback**: Enabled or disabled
- **Importance ratio**: Enabled or disabled

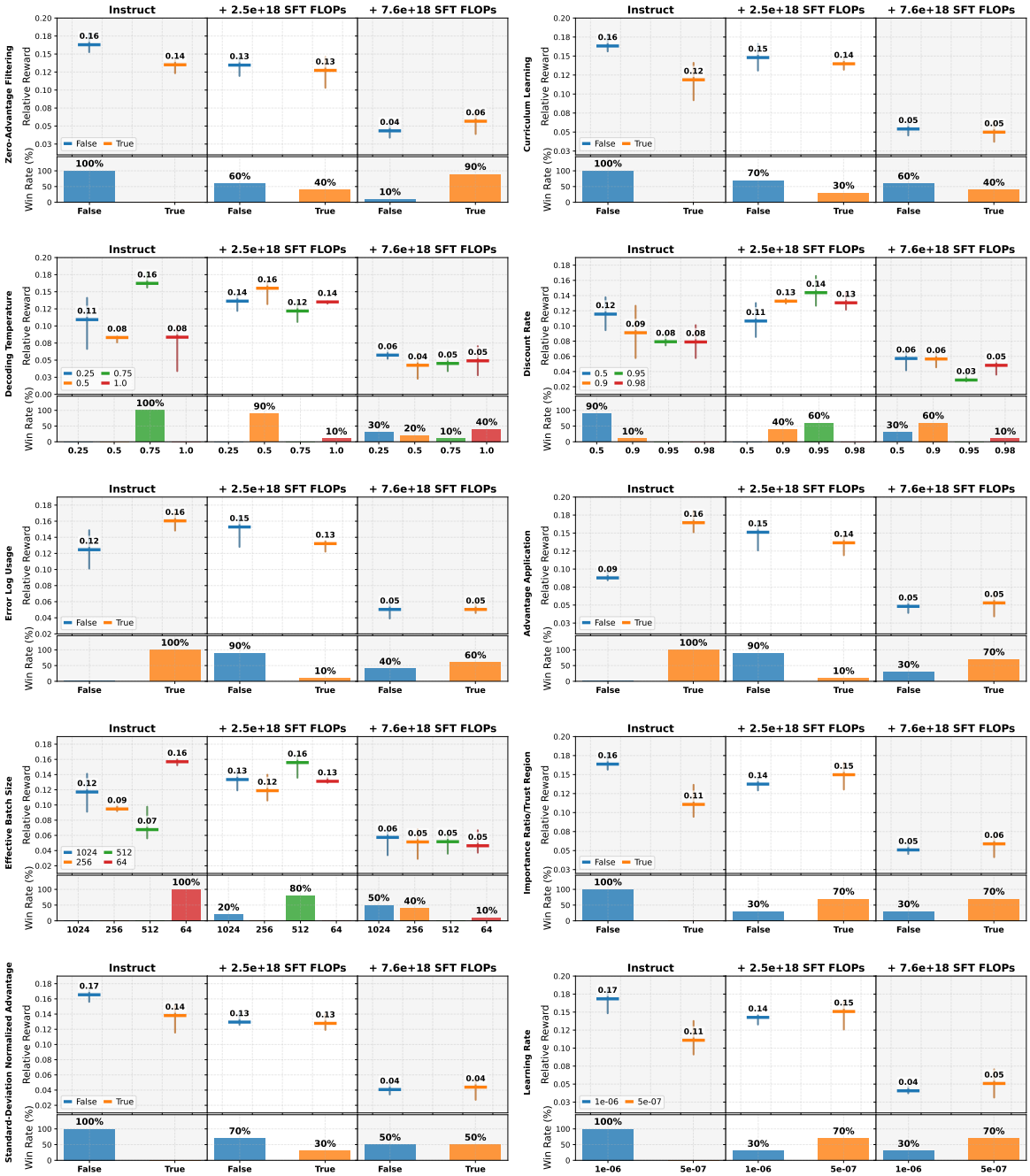

Figure 9: Bootstrap analysis ($n = 1000$ samples) of hyperparameter optimization across different SFT compute budgets on *test* held out tasks. Each subplot examines a different hyperparameter, including increasing SFT compute: the base instruct model (left), +2.5e+18 SFT FLOPs (middle), and +7.6e+18 SFT FLOPs (right). For each hyperparameter-compute combination, the top panel shows relative reward performance with error bars indicating 95% confidence intervals, while the bottom panel displays win rates representing the percentage of bootstrap iterations where each parameter value achieved maximum performance. Results demonstrate that optimal hyperparameter values often shift as model pre-training compute increases, suggesting that hyperparameter selection should be adapted based on the computational budget allocated to SFT.