

Sequent-Prover: Training Agents for Formal, Checkable SMT-based Reasoning

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🤖 Sequent-Prover-{4B, 9B, 27B} 🧠 proofofthought

Abstract

We introduce Sequent-Prover, a family of verifier-using language-model agents, and the post-training framework that produces them. Agents reason in an executable SMT-LIB scratchpad: they write complete SMT-LIB programs, query Z3 across turns, inspect solver feedback, repair formalizations, and terminate with an explicit answer. Our recipe, verifier-conditioned trajectory distillation, bootstraps from a noisy teacher proposal policy by sampling multi-turn trajectories, retaining the successful tail that is correct, non-trivial, grounded, and executable, then applying supervised fine-tuning and trajectory-level preference optimization. Because solver success alone does not guarantee semantic faithfulness, we evaluate task accuracy, k-sample reliability, and SMT program quality. Across 37 benchmarks and 7,254 problems per model family, post-training raises the 4B model’s per-attempt accuracy from 85.6% to 97.0% and four-sample all-correct reliability ($pass^4$) from 64.1% to 91.7%. It also reduces average SMT calls from 4.10 to 2.52 and tokens per attempt from 18.97k to 12.72k. Sequent-Prover agents give answers backed by executable formal artifacts.

1 Introduction

Large language models can produce fluent multi-step reasoning, but fluency is not executable evidence that a conclusion follows from the stated assumptions (Wei et al., 2022; Kojima et al., 2022; Turpin et al., 2023; Lanham et al., 2023). A natural alternative is to make reasoning *checkable*: translate an input into explicit assumptions, constraints, and claims, then verify the resulting obligation with a symbolic backend (Pan et al., 2023; Ye et al., 2023; Olausson et al., 2023; Ganguly et al., 2024). SMT solvers such as Z3 are especially attractive because SMT-LIB can encode logical entailment, arithmetic, datatypes, arrays, bit-vectors, and program constraints, while solver execution returns

concrete feedback such as sat, unsat, countermodels, parser errors, and type errors (de Moura and Bjørner, 2008; Barrett and Tinelli, 2018; Barrett et al., 2025).

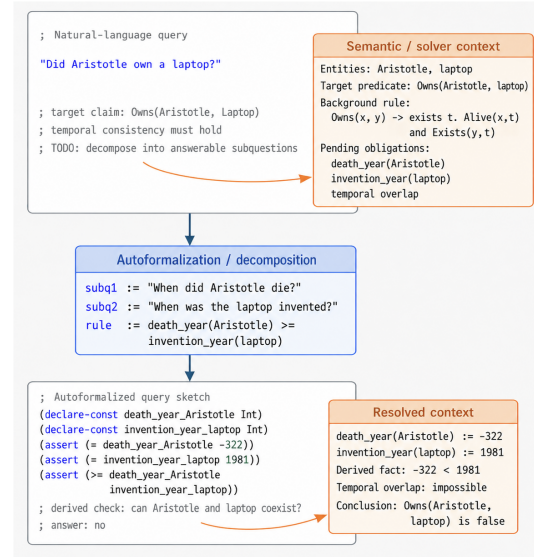


Figure 1: A toy example showing how Sequent-Prover turns an underspecified commonsense claim into a grounded, checkable inconsistency.

However, solver access alone does not make reasoning reliable. Recent work by Ganguly et al. (2026) shows that SMT-based autoformalization can improve premise-explicit reasoning but degrade performance when the generated artifact misrepresents the input. The core problem is the *autoformalization gap*: real tasks arrive as natural language, specifications, and informal intent, not as already-correct proof obligations. A solver checks only the artifact it receives; if the model omits a premise, reverses entailment, introduces inconsistent assumptions, or encodes a different problem, the solver may return a valid result for the wrong object. Reliable verifier use therefore requires models that construct grounded, non-vacuous, executable formalizations through multi-turn tool use.

We introduce *Sequent-Prover*, a family of post-trained language models for SMT-verifiable reasoning. Rather than using the solver as a test-time wrapper (Ganguly et al., 2024; Singh et al., 2026; Feng et al., 2025), we use solver interaction as a training substrate: we sample candidate formalization traces, execute them with Z3, reject malformed or vacuous artifacts, and convert supported traces into supervised and preference data. Post-training turns inconsistent SMT tool use into more dependable multi-sample correctness and yields agents that write more grounded, non-trivial, executable SMT programs. In particular, we find that:

1. Post-training substantially improves SMT reasoning: models that previously achieved only inconsistent solver-aided success become more reliable under multi-sample evaluation.
2. Supervised fine-tuning improves *proof discipline*: tuned agents produce higher-quality SMT-LIB programs that are better grounded in the input, less vacuous, and structurally more consistent across samples.
3. Post-training strengthens more demanding proof strategies, including refutational reasoning, where agents increasingly formulate proof obligations by contradiction and correctly interpret the resulting unsat evidence.
4. Preference optimization with DPO further improves stability, grounding, proof quality, and efficiency beyond supervised fine-tuning: DPO-tuned agents solve problems earlier, with fewer tokens and fewer solver-interaction rounds per correct answer.

2 Methodology

Overview. We frame verifier-guided reasoning as success-conditioned policy bootstrapping. A teacher model need not be reliable on every rollout; it only needs a nonzero probability on trajectories that use the verifier correctly. Sampling the teacher in an SMT-LIB scratchpad exposes a sparse successful tail: rollouts that construct and revise executable formalizations, use Z3 feedback, and terminate with the correct answer. We condition on this tail so the student learns verifier-mediated search behavior that the teacher produces only sporadically.

Our method, *verifier-conditioned trajectory distillation*, has three stages. First, we sample teacher trajectories under the same z3-solve/finish interface used at evaluation time. Second, we curate

them with an acceptance predicate that combines answer correctness with executable scratchpad evidence: solver execution, non-triviality, grounding, and protocol completion. Third, we train by supervised fine-tuning on accepted traces and by DPO on trajectory-level pairs that contrast accepted traces with rejected or weakly supported ones. The resulting policy amortizes rare successful verifier-mediated searches into more reliable inference.

2.1 Problem Formulation

Each task instance is $x = (\mathcal{C}, q)$, where $\mathcal{C} = \{p_1, \dots, p_m\}$ contains the natural-language premises, program fragments, or specifications, and q is the query. The model must produce a final answer y matching y^* and, when possible, support it with an executable SMT-LIB artifact. We use ϕ for the model’s formalization map: $\phi_{\mathcal{C}}$ encodes assumptions from $\mathcal{C}$, and ϕ_c encodes a candidate claim c . Context consistency is checked by $\text{SAT}(\phi_{\mathcal{C}})$; claim compatibility by $\text{SAT}(\phi_{\mathcal{C}} \wedge \phi_c)$; and entailment by refutation, requiring $\text{SAT}(\phi_{\mathcal{C}}) = \text{true}$ and $\text{SAT}(\phi_{\mathcal{C}} \wedge \neg \phi_c) = \text{false}$. Thus, a claim is entailed only when the formalized context is satisfiable and the claim’s negation is inconsistent with it, making unsat the proof signal for refutational entailment.

2.2 The SMT Scratchpad Interface

A central design choice is to expose the SMT-LIB backend from Ganguly et al. (2024) as an *executable scratchpad*: a workspace in which the model constructs, tests, and revises formal artifacts across turns. We define the tool environment as $\mathcal{E}_{\text{SMT}} = (\mathcal{A}, \mathcal{O})$, $\mathcal{A} = \{\text{z3-solve}, \text{finish}\}$. The z3-solve action executes a self-contained SMT-LIB script with declarations and a check-sat command. A single action can express context checks, candidate-claim tests, counterexample searches, model construction, semantic-equivalence checks, or refutational proof obligations. Z3 returns an observation in $\mathcal{O}$, such as sat, unsat, unknown, a model, a parser/type error, or a timeout, which is appended to the interaction history.

The finish action is separate from scratchpad execution: it terminates the episode with a final answer after the model judges that the accumulated solver feedback is sufficient. A trajectory is therefore a multi-turn record $\tau = (m_1, o_1, \dots, m_T, o_T)$, where model messages may contain natural-language reasoning, SMT-LIB code, tool calls, repairs, or the final answer, and the

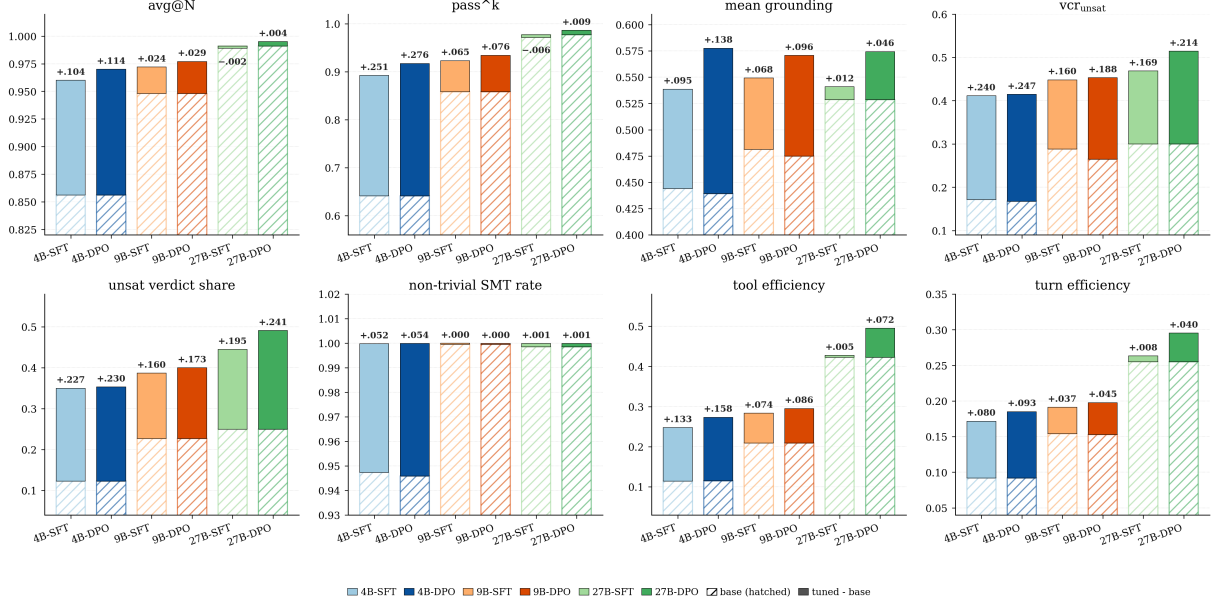


Figure 2: Post-training, especially DPO, shifts the models from merely sampling correct answers toward more reliable solver-grounded proof behavior. Hatched bars show each base checkpoint and colored overlays show the tuned-model gain: avg@N measures per-sample correctness, pass^k measures consistency by requiring all sampled trajectories to be correct, mean grounding measures how much of the problem is represented in SMT, and vcr_unsat measures correct answers backed by non-trivial UNSAT/refutation certificates. The remaining panels show the mechanism behind the gains: tuned models more often drive Z3 to UNSAT verdicts, avoid trivial SMT calls, and obtain more verified progress per tool call and per reasoning turn.

policy π_θ maps the current history to the next token or tool action. We do not require every scratchpad call to constitute a complete proof; early calls may be exploratory or diagnostic. However, accepted training traces must contain meaningful scratchpad evidence: at least one Z3 invocation with an executable, non-trivial, input-grounded SMT-LIB program that obtains a real solver verdict.

2.3 Verifier Feedback and Trace Semantics

Each `z3-solve` call converts a generated SMT-LIB artifact into a structured observation that the agent can use to interpret, repair, or commit to a formalization. We write $\text{SAT}(\psi) \in \{\text{sat}, \text{unsat}, \text{unknown}\}$ for the Z3 verdict on a formula or script ψ , and treat parser errors, type errors, timeouts, malformed scripts, and solver failures as unresolved observations. For a candidate claim c_i with context encoding ϕ_C and claim encoding ϕ_{c_i} , the verifier first separates inconsistent contexts from meaningful claim statuses: if $\text{SAT}(\phi_C) = \text{unsat}$, the artifact receives the inconsistent-context status σ_I . Otherwise, when $\text{SAT}(\phi_C) = \text{sat}$, the claim is entailed (σ_E) if $\text{SAT}(\phi_C \wedge \neg\phi_{c_i}) = \text{unsat}$, contradictory (σ_C) if $\text{SAT}(\phi_C \wedge \phi_{c_i}) = \text{unsat}$, and possible but not forced (σ_P) if both $\text{SAT}(\phi_C \wedge \phi_{c_i}) = \text{sat}$ and

$\text{SAT}(\phi_C \wedge \neg\phi_{c_i}) = \text{sat}$. We use σ_U for states in which no semantic status can be assigned, including unknown, timeout, solver failure, or parse/type errors. The same scratchpad also supports comparison between alternative formalizations: given formulas ϕ_a and ϕ_b over a shared signature and background theory Σ , they are equivalent when $\text{SAT}_\Sigma(\neg(\phi_a \leftrightarrow \phi_b)) = \text{unsat}$.

Solver feedback is richer than a binary success signal: `sat` can expose a witness or countermodel, `unsat` provides refutational evidence, and execution errors identify artifacts that must be repaired. When constraints are labeled or instrumented, this feedback can be augmented with `unsat` cores or approximate correction sets, but the main training signal comes from executable SMT-LIB interaction and solver feedback.

Teacher Rollout Generation. For each training instance $x = (\mathcal{C}, q)$, we sample K_T trajectories from a teacher proposal model M_T under the same SMT-scratchpad interface used at evaluation time. The teacher is given only the problem instance x , not the target answer y^* ; the target is reserved for offline curation. Thus, the teacher is not an oracle annotator, but a stochastic proposal distribution $p_T(\tau \mid x)$ over executable reasoning traces. Each sampled trajectory is stored

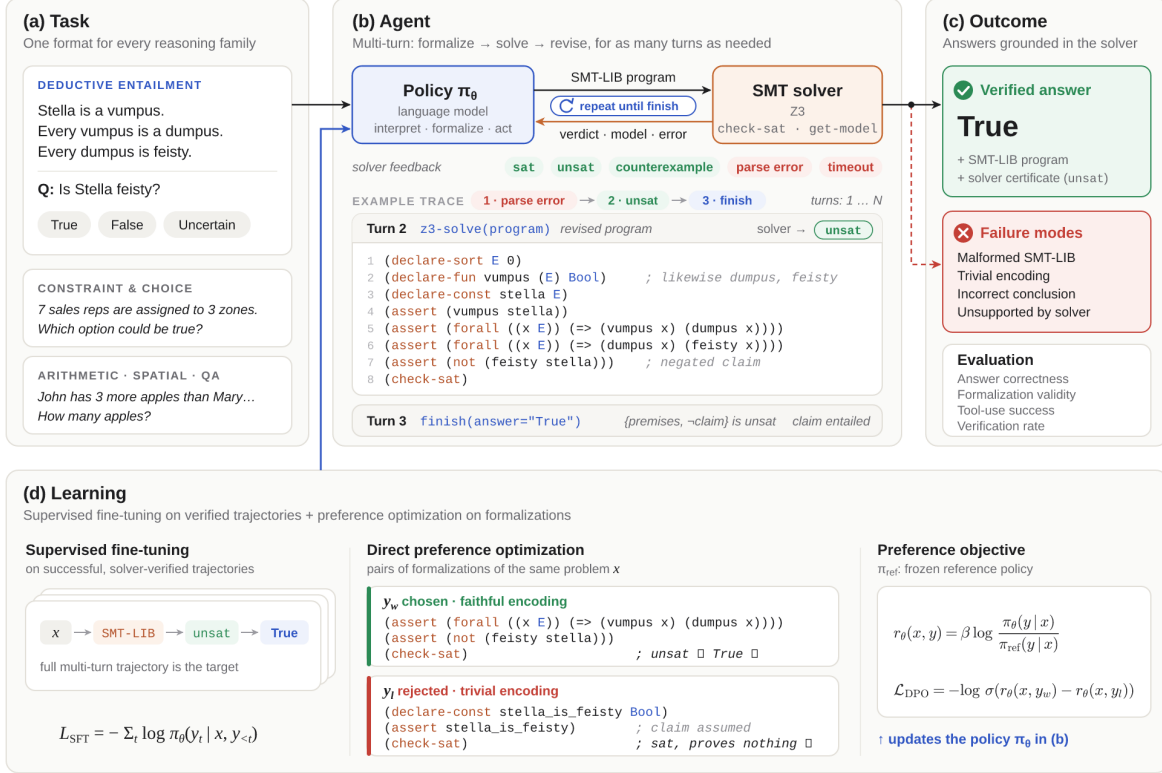


Figure 3: Sequent-Prover overview. The agent formalizes a task in SMT-LIB, calls Z3 over several turns, and uses solver feedback until it finishes. Successful trajectories train SFT, and preferred versus rejected trajectories train DPO.

as $\tau = (x, h_T, \hat{y}, \mu)$, $h_T = (m_1, o_1, \dots, m_T, o_T)$, where h_T is the interaction transcript, $\hat{y}$ is the answer emitted through finish, and μ records protocol metadata such as solver-call counts, solver verdicts, execution errors, token usage, and termination status. These rollouts provide the candidate pool from which verifier-conditioned trace curation selects training examples.

Verifier-Conditioned Trace Curation. The offline curator converts teacher rollouts into training data by applying a trajectory-level acceptance predicate $A(x, y^*, \tau)$. A rollout is accepted when it reaches the correct task answer through finish and contains meaningful scratchpad evidence: an executable z3-solve interaction that obtains a solver verdict and passes the structural checks for non-triviality, grounding, and detectable non-vacuity. We label accepted rollouts as *Gold*. Rollouts that reach the correct answer but provide weak formal support are labeled *Silver*, while rollouts that fail the answer, protocol, executability, or scratchpad-quality conditions are labeled *Rejected*. This partition separates task success from verifier-use quality, so that only fully supported traces become demon-

strations while weak or failed traces remain available as a contrastive signal.

Formally, if $p_T(\tau | x)$ is the teacher proposal distribution and $Z_T(x) = \Pr_{\tau \sim p_T(\cdot | x)}[A(x, y^*, \tau) = 1]$, then the gold supervision distribution is $p_{\text{gold}}(\tau | x) = \frac{p_T(\tau | x) A(x, y^*, \tau)}{Z_T(x)} = p_T(\tau | x, A(x, y^*, \tau) = 1)$. Trace curation defines the success-conditioned target that SFT imitates, with Silver and Rejected traces supplying the negative side of the preference data.

2.4 Training

SFT on Executable Traces. Gold trajectories define the supervised corpus $\mathcal{D}_{\text{gold}}$. We train the student policy π_θ by maximum likelihood over the model-generated actions in each accepted transcript:

$$\mathcal{L}_{\text{SFT}}(\theta) = - \sum_{\tau \in \mathcal{D}_{\text{gold}}} \sum_{t \in \mathcal{I}(\tau)} \log \pi_\theta(a_t | h_t),$$

where h_t is the interaction history, a_t is the next model token or tool-use action, and $\mathcal{I}(\tau)$ indexes positions generated by the model. Solver observations remain in the conditioning history but are

masked from the prediction loss. SFT amortizes the curated successful tail: the student learns the action patterns by which accepted traces construct evidence, respond to verifier feedback, and commit to a correct answer.

Trajectory Preference Optimization. After SFT, we further refine the student with trajectory-level preferences. For each problem x with at least one Gold trace and at least one contrastive trace, we form triples (x, τ^+, τ^-) , where τ^+ is an accepted trajectory and τ^- is either Rejected or Silver. Silver traces serve as hard negatives because they may reach the correct answer with weaker scratchpad support; Rejected traces capture answer, protocol, executability, or verifier-use failures. Gold traces define what to favor, while Silver and Rejected traces define nearby behaviors to suppress.

We optimize Direct Preference Optimization with the frozen SFT model π_{ref} as reference. Let $r_\theta(\tau, x) = \log \pi_\theta(\tau | x) - \log \pi_{\text{ref}}(\tau | x)$. The objective is $\mathcal{L}_{\text{DPO}}(\theta) = -\mathbb{E}_{(x, \tau^+, \tau^-)} \log \sigma(\beta [r_\theta(\tau^+, x) - r_\theta(\tau^-, x)])$, where β controls the strength of the preference update. As in SFT, trajectory likelihoods are computed over model-generated tokens and tool actions, with solver observations used as conditioning context. This stage raises the probability of complete, verifier-supported traces and lowers the probability of correct-looking but weakly supported or failed ones.

Policy Bootstrapping. The success-conditioned bootstrapping mechanism relies on the teacher’s *support* under the acceptance predicate. Let $Z_T(x) = \Pr_{\tau \sim p_T(\cdot|x)}[A(x, y^*, \tau) = 1]$ denote the teacher’s probability of producing an accepted trajectory for problem x . When $Z_T(x) > 0$, sampling K_T rollouts exposes at least one Gold trace with probability $1 - (1 - Z_T(x))^{K_T}$. Trace curation turns these rare successes into a high-precision target: SFT amortizes accepted search behavior, while DPO uses Silver and Rejected traces to suppress nearby failure modes.

Inference. At test time, the student uses the same SMT-scratchpad protocol as teacher sampling and training. Given $x = (\mathcal{C}, q)$, it generates tool-use actions until finish or the interaction budget. We score the final answer against y^* and retain the full trajectory for process analysis: solver invocation, verdict quality, non-triviality, grounding, and protocol completion. For multi-sample evaluation, we

draw K independent trajectories per problem and aggregate answer and scratchpad-quality metrics.

3 Results

Evaluation protocol. We present evaluations of nine Sequent-Prover checkpoints: base, supervised fine-tuned (SFT), and further preference-optimized (DPO) variants of the Qwen3.5–4B, Qwen3.5–9B, and Qwen3.6–27B families. Each model is evaluated with $K = 4$ stochastic SMT-scratchpad trajectories per problem. The final paired pool contains 37 benchmarks and 7,254 problems per model family. The suite covers logical and constraint reasoning (ProofWriter, RuleTaker, FOLIO, ProntoQA, AR-LSAT, ZebraLogic, ReClor, StrategyQA), arithmetic and spatial reasoning (GSM8K, SVAMP, MAWPS, AS-Div, AQUA-RAT, DROP, SpartQA-MC, SpartQA-YN, StepGame), NLI and multiple-choice commonsense/knowledge tasks (ANLI, BoolQ, Winogrande, CommonsenseQA, OpenBookQA, QASC, ARC-Challenge, MMLU, HellaSwag), domain and reading-comprehension QA (MedQA, MedMCQA, PolicyQA, HotpotQA, MuSiQue, SQuAD, TriviaQA), and AGIEval LSAT/SAT subsets (LSAT-LR, LSAT-RC, SAT-Math, SAT-EN).

Metrics. We report $\text{avg}@N$, the per-attempt accuracy over all sampled trajectories; $\text{pass}@4$, the probability that at least one of four samples solves the problem; and pass^4 , the probability that all four samples solve the problem. We use pass^4 as the main reliability metric because deployed stochastic agents should produce correct behavior consistently, rather than only under best-of- K selection. We also report $\text{stability} = \text{pass}^4 / \text{pass}@4$, which normalizes all-sample consistency by four-sample coverage. For process analysis, we use the SMT-quality metrics from Section 2: first-pass Z3 validity, non-trivial grounded solver use, solver verdict distributions, and token/iteration efficiency.

Research questions. The results address four questions. **RQ1:** does verifier-conditioned post-training improve single-sample accuracy and multi-sample reliability, beyond what extra test-time sampling gives? **RQ2:** how do SFT and DPO differ, and which parts of the recipe matter? **RQ3:** how does post-training change the solver-agent process? **RQ4:** where are the gains largest across tasks and model scales?

Aggregate performance							pass@ <i>k</i> scaling				
Family	Ckpt.	avg@ <i>N</i> ↑	pass@4 ↑	pass^4 ↑	Stab. ↑	Δpass^4	Model	@1	@2	@3	@4
4B	BASE	0.856	0.970	0.641	0.661	–	4B BASE	0.856	0.943	0.963	0.970
4B	SFT	0.960	0.995	0.892	0.896	+0.251	4B DPO	0.970	0.992	0.996	0.998
4B	DPO	0.970	0.998	0.917	0.919	+0.277	9B BASE	0.948	0.984	0.991	0.994
9B	BASE	0.948	0.994	0.858	0.863	–	9B DPO	0.977	0.994	0.997	0.998
9B	SFT	0.972	0.997	0.923	0.926	+0.065	27B BASE	0.991	0.996	0.998	0.998
9B	DPO	0.977	0.998	0.934	0.936	+0.076	27B DPO	0.995	0.999	1.000	1.000
27B	BASE	0.991	0.998	0.977	0.979	–					
27B	SFT	0.989	0.999	0.971	0.972	−0.006					
27B	DPO	0.995	1.000	0.986	0.986	+0.009					

Table 1: Aggregate performance and pass@*k* scaling on the 37-benchmark paired evaluation pool. Each family is compared against its own base checkpoint. pass@4 measures four-sample coverage, while pass^4 requires all four sampled trajectories to be correct. The pass@*k* panel shows that a single 4B DPO sample matches the 4B base model’s four-sample coverage. Bold marks the best checkpoint within each family; Δ is relative to BASE, with gains and losses colored.

3.1 Aggregate accuracy and reliability

Table 1 shows the main comparison. Verifier-conditioned post-training yields the largest improvements for the 4B model: DPO raises avg@*N* from 0.856 to 0.970 and pass^4 from 0.641 to 0.917, a 27.7-point increase in all-sample reliability. The 9B model follows the same pattern from a stronger starting point, with DPO improving pass^4 from 0.858 to 0.934. The 27B base model is already close to saturation on the aggregate pool, and DPO still reaches the best value in the family for every reported metric, including pass^4 of 0.986.

The distinction between pass@4 and pass^4 captures the central effect. The base checkpoints already have high four-sample coverage, especially at 9B and 27B, due to the multi-option nature of most of these tasks. Post-training concentrates probability mass on the correct trajectory mode, producing large gains in pass^4 and stability. For 4B, stability rises from 0.661 to 0.919 under DPO; for 9B, it rises from 0.863 to 0.936. This shows that the trained policies improve single-run reliability as well as best-of-four coverage.

Table 1 presents the pass@*k* curves for the base and DPO checkpoints. The 4B result is especially clear: 4B DPO reaches 0.970 at pass@1, matching the 4B base model’s pass@4 value. At 9B, DPO raises pass@1 from 0.948 to 0.977 and nearly saturates pass@4. At 27B, the curve is near the ceiling throughout, with DPO reaching 1.000 by pass@3.

3.2 Problem-level consistency transitions

Table 2a decomposes the reliability gains by the number of correct base-model attempts. Across 4B and 9B, the largest source of improvement is the

middle tier: problems with one to three correct base attempts become correct on all four tuned attempts. For 4B DPO, this happens for 86.7% of the 2,379 partially correct base problems. For 9B DPO, it happens for 78.7% of the 970 partially correct base problems. The tuned models also preserve most problems that were already reliable: DPO retains 96.5%, 96.2%, and 99.3% of base 4/4 problems in the 4B, 9B, and 27B families, respectively.

The transition table also clarifies the role of scale. The 4B base model has 2,379 partially correct problems, so post-training has substantial headroom to improve reliability. The 27B base model has only 153 partially correct problems and 13 zero-correct problems, so aggregate gains are naturally smaller. Within that restricted headroom, DPO still improves the middle-tier conversion rate from 53.6% under SFT to 71.9%, and it preserves nearly all already reliable examples.

3.3 Training-stage ablation

Table 2b compares DPO directly against SFT on shared datasets. Preference optimization improves every family on avg@*N*, pass^4, stability, and verified-correct rate. The coverage overlap is large: for example, 4B SFT and 4B DPO both solve 7,213 problems at least once, while DPO adds 25 DPO-only problems and SFT adds 8 SFT-only problems. The most consistent DPO gains are in reliability and trace quality. At 27B, the DPO stage is particularly useful: it adds +0.012 macro pass^4, +0.016 stability, +0.046 verified-correct rate, and +0.050 UNSAT-restricted verified-correct rate over SFT.

(a) Problem-level transitions grouped by base-model correctness.

Family	Tune	$n_{0/4}$	$P(\geq 1/4 \mid 0/4)$	$n_{1-3/4}$	$P(4/4 \mid 1-3/4)$	$n_{4/4}$	$P(4/4 \mid 4/4)$
4B	SFT	215	0.930	2,379	0.839	4,660	0.942
4B	DPO	215	0.958	2,379	0.867	4,660	0.965
9B	SFT	41	0.780	970	0.753	6,243	0.954
9B	DPO	41	0.780	970	0.787	6,243	0.962
27B	SFT	13	0.846	153	0.536	7,088	0.982
27B	DPO	13	0.923	153	0.719	7,088	0.993

(b) Direct DPO–SFT comparison. Deltas are macro-averages over shared datasets; DPO-only/SFT-only paired solves are 25/8, 13/8, and 7/0 for 4B, 9B, and 27B.

Family	$\Delta\text{avg}@N$	$\Delta\text{pass}@4$	$\Delta\text{stab.}$	ΔVCR	$\Delta\text{VCR}_{\text{UNSAT}}$
4B	+0.005	+0.009	+0.007	+0.050	+0.013
9B	+0.006	+0.008	+0.011	+0.036	+0.026
27B	+0.005	+0.012	+0.016	+0.046	+0.050

Table 2: Performance transition and direct-comparison analyses for tuned checkpoints. In Table 2a, the three rates measure coverage gained on base-unsolved problems, conversion of partially correct problems to 4/4 correctness, and retention of already 4/4 problems. In Table 2b, metric deltas are macro-averages over shared datasets; DPO-only and SFT-only count paired problems solved by one tuned checkpoint and not the other.

Method	K	$\text{pass}@K \uparrow$	$\text{pass}^K \uparrow$	Tokens $\downarrow$
Base	4	0.848	0.395	110.9k
Base	16	0.911	0.139	443.7k
Base + self-refine	4	0.928	0.572	219.5k
Post-trained 4B	1	0.869	0.869	16.7k
Post-trained 4B	4	0.948	0.743	66.9k

Table 3: Post-training versus test-time sampling (4B, 180 held-out problems from six families). Tokens are per problem. Shaded rows are post-trained. Full results are in Table 16.

Checkpoint	$\text{avg}@N \uparrow$	$\text{pass}@4 \uparrow$	$\text{pass}^4 \uparrow$	Stab. $\uparrow$
SFT (ours)	0.585	0.715	0.409	0.572
DPO (ours)	0.603	0.718	0.439	0.611
Answer-only SFT	0.407	0.654	0.126	0.193
Solver-verdict SFT	0.583	0.712	0.403	0.566
Continued SFT	0.591	0.700	0.440	0.629
Gold/Silver/Rej. DPO	0.586	0.699	0.419	0.599

Table 4: Data-selection ablations for 4B on five hard held-out benchmarks.

3.4 Test-time sampling and data ablations

Post-training versus more samples. We compare the 4B models on 180 held-out problems, 30 each from ProntoQA, FOLIO, AR-LSAT, GSM8K, StepGame, and ARC-Challenge, with the same harness, temperature, and iteration limit for every method (Appendix D.2). At matched $K = 4$, post-training raises $\text{pass}@4$ by 10.1 points and pass^4 by 34.8 points over base self-consistency, with fewer tokens (Table 3). The post-trained model at $K = 4$ also beats the base model at $K = 16$ on both metrics while using $6.6\times$ fewer tokens. From $K = 1$ to $K = 16$, base $\text{pass}@K$ rises from 0.657 to 0.911, but pass^K falls from 0.657 to 0.139. Extra sampling raises coverage but lowers all-sample reliability. Self-refinement, the strongest prompting baseline, still trails the post-trained model while using $3.3\times$ more tokens.

Which parts of the recipe matter. We train four 4B variants and evaluate them on ReClor, SpartQA-

MC, SpartQA-YN, StepGame, and AGIEval-LSAT-LR (200 problems each, $K = 4$; Table 4). *Answer-only SFT* keeps only the final answer, and pass^4 falls from 0.409 (full-trajectory SFT) to 0.126, so the gains do not come from label exposure. *Solver-verdict SFT* removes the grounding and non-triviality checks and is lower on every metric. *Continued SFT* replaces DPO with more SFT at matched training FLOPs. It ties DPO on pass^4 and is slightly higher on stability, but lower on $\text{avg}@N$ and $\text{pass}@4$. *Gold/Silver/Rejected DPO* uses the three tiers as separate preference levels and is below our DPO on every metric. Our full recipe leads on $\text{avg}@N$ and $\text{pass}@4$.

3.5 Solver-agent process quality

Table 5a shows that post-training improves the computational profile of the agent loop. The 4B DPO checkpoint reduces average iterations from 4.97 to 3.51, SMT calls from 4.10 to 2.52, and tokens from 18.97k to 12.72k. Normalizing by successful attempts, tokens per correct answer fall from 26.73k to 13.89k. The 9B and 27B families show the same pattern, with DPO giving the lowest token and solver-call cost within each family.

The first verified-correct SMT signal also arrives earlier after tuning. The mean first-success iteration drops from 2.00 to 1.29 for 4B DPO, from 1.62 to 1.22 for 9B DPO, and from 1.19 to 1.07 for 27B DPO. Under a two-iteration budget, 4B DPO reaches a verified-correct rate of 0.902, above the 4B base model’s full-budget rate of 0.765; 9B DPO reaches 0.929 by iteration two, above the 9B base full-budget rate of 0.908. These early-exit curves show that post-training improves both endpoint accuracy and the efficiency frontier of the interactive solver loop.

Table 5b shows that the tuned policies emit cleaner SMT programs and obtain stronger solver signals. For 4B, the error rate falls from 41.3%

(a) Solver-agent efficiency.							(b) SMT scratchpad diagnostics.						
Family	Ckpt.	Iter. ↓	SMT calls ↓	Tokens ↓	Tok./correct ↓	First VC iter. ↓	Family	Ckpt.	Error ↓	No verdict ↓	UNSAT ↑	UNSAT correct ↑	Calls
4B	BASE	4.97	4.10	18.97k	26.73k	2.00	4B	BASE	41.3%	10.3%	12.3%	12.9%	122.6k
4B	SFT	3.76	2.78	13.70k	15.23k	1.33	4B	SFT	19.1%	0.3%	35.0%	36.2%	82.0k
4B	DPO	3.51	2.52	12.72k	13.89k	1.29	4B	DPO	19.0%	0.4%	35.3%	36.3%	73.3k
9B	BASE	4.37	3.41	15.69k	19.53k	1.62	9B	BASE	26.0%	0.4%	22.7%	23.0%	101.1k
9B	SFT	3.52	2.54	12.31k	14.16k	1.24	9B	SFT	13.9%	0.3%	38.7%	39.7%	75.0k
9B	DPO	3.33	2.34	11.85k	13.27k	1.22	9B	DPO	14.4%	0.4%	40.0%	41.0%	68.7k
27B	BASE	2.92	1.90	9.51k	10.28k	1.19	27B	BASE	16.9%	0.7%	25.0%	25.2%	59.8k
27B	SFT	2.85	1.86	7.92k	8.52k	1.08	27B	SFT	5.8%	0.1%	44.5%	45.8%	55.0k
27B	DPO	2.58	1.58	6.78k	7.01k	1.07	27B	DPO	5.5%	0.1%	49.1%	49.7%	46.9k

Table 5: Efficiency and SMT-scratchpad diagnostics. In Table 5a, Iter. is the average number of model iterations, SMT calls is the average number of solver invocations, Tokens is the average token count per problem attempt, Tok./correct normalizes token use by correct-attempt rate, and First VC iter. is the mean iteration index of the first verified-correct SMT signal. In Table 5b, Error and no-verdict measure unusable calls, UNSAT is the share of calls returning an UNSAT verdict, UNSAT|correct restricts to calls inside correct attempts, and Calls is total solver calls.

Fam	Dataset	Base $pass^4$	DPO $pass^4$	Δ
4B	ProofWriter	0.074	0.926	+0.852
4B	ZebraLogic	0.120	0.715	+0.595
4B	RuleTaker	0.395	0.985	+0.590
4B	AR-LSAT	0.035	0.560	+0.525
4B	ProntoQA	0.445	0.955	+0.510
4B	SpartQA-MC	0.250	0.745	+0.495
9B	AR-LSAT	0.235	0.640	+0.405
9B	ZebraLogic	0.410	0.790	+0.380
9B	QASC	0.840	1.000	+0.160
9B	WinoGrande	0.845	0.990	+0.145
9B	CommonsenseQA	0.845	0.980	+0.135
9B	ARC-Challenge	0.870	0.985	+0.115
27B	ZebraLogic	0.825	0.965	+0.140
27B	AR-LSAT	0.785	0.920	+0.135
27B	StepGame	0.825	0.860	+0.035
27B	ReClor	0.960	0.985	+0.025

Table 6: Largest representative dataset-level DPO gains on $pass^4$ within each model family.

to 19.0% under DPO, and no-verdict calls fall from 10.3% to 0.4%. For 27B, the error rate falls from 16.9% to 5.5%. The same table shows a shift toward refutational solver evidence: UNSAT calls increase from 12.3% to 35.3% for 4B DPO, from 22.7% to 40.0% for 9B DPO, and from 25.0% to 49.1% for 27B DPO. The UNSAT share inside correct attempts rises in parallel, reaching 49.7% for 27B DPO. The grounding gains hold with SMT comments removed (+0.081 for 4B DPO; Appendix D.3). Post-training also lowers the structural (PCFG) divergence between independent SMT programs in all six Base-to-tuned comparisons, with every 95% interval below zero (Appendix D.4). Such structural variation predicts autoformalization errors (Ganguly et al., 2026).

3.6 Benchmark-level trends

Table 6 reports representative per-dataset improvements for DPO. The highest-gain tasks are those

with explicit symbolic or relational structure. At 4B, $pass^4$ improves by 85.2 points on ProofWriter, 59.0 points on RuleTaker, 52.5 points on AR-LSAT, 59.5 points on ZebraLogic, and 51.0 points on ProntoQA. At 9B, the largest gains include AR-LSAT, ZebraLogic, QASC, WinoGrande, CommonsenseQA, and ARC-Challenge. At 27B, aggregate scores are close to ceiling, and the clearest remaining gains are on ZebraLogic and AR-LSAT. On AR-LSAT, all three DPO students exceed the teacher’s $pass^3$ of 0.484, reaching 0.560, 0.640, and 0.920 (Appendix D.5).

These dataset-level results align with the scratchpad interpretation of the method. The strongest gains occur where the task contains latent constraints, relational structure, arithmetic structure, or entailment structure that can be made executable. Broad QA and domain tasks also benefit in the smaller model families, with improvements on ARC-Challenge, CommonsenseQA, MedQA, MedMCQA, OpenBookQA, QASC, and WinoGrande. The aggregate effect spans more than formal logic benchmarks while remaining largest on tasks with natural SMT structure. At 4B and 9B, DPO improves $avg@N$ and $pass^4$ in every category, and hard reasoning is only 14.5% of the pool (Appendix D.1).

Post-training and scale. Table 7 compares tuned smaller models against larger base models. The 4B tuned agents compare favorably with the 9B base model: 4B SFT wins 61.2% of non-tied paired comparisons, and 4B DPO wins 69.9%. The 27B base model sets a higher aggregate ceiling, and the tuned 9B models win fewer non-tied comparisons against it. Combining scale and post-training gives the strongest overall checkpoints: 27B DPO has the highest aggregate $avg@N$, $pass@4$, $pass^4$,

Comparison	Small	Large	Tie	Win $\uparrow$
4B SFT / 9B BASE	816	518	5,920	0.612
4B DPO / 9B BASE	840	361	6,053	0.699
9B SFT / 27B BASE	73	490	6,691	0.130
9B DPO / 27B BASE	90	420	6,744	0.176
4B DPO / 27B BASE	84	532	6,638	0.136

Table 7: Cross-scale paired comparison. Small and Large count paired problems where the smaller tuned checkpoint or larger base checkpoint has more correct attempts out of four. Win is the smaller tuned checkpoint’s non-tie win rate.

and SMT-quality profile in the sweep.

4 Related Works

Verifier-Integrated Formal Reasoning. Recent progress in formal theorem proving has shown that language models can benefit greatly from emitting machine-checkable artifacts rather than only natural-language explanations. Systems such as Goedel Prover, DeepSeek-Prover, and Kimina-Prover combine long-form reasoning with formal verification and iterative refinement (Lin et al., 2025b; Ren et al., 2025; Wang et al., 2025). Our work is inspired by this paradigm, but shifts the target language from Lean to SMT-LIB and focuses on compact models that must operate across multiple SMT logics.

Solver-Based Symbolic Reasoning and Synthesis. SMT solvers such as Z3 and cvc5 are central tools for formal verification, constraint solving, and program synthesis (de Moura and Bjørner, 2008; Barbosa et al., 2022). Classical symbolic systems rely on hand-written encodings or search procedures over formal objects. Our setting is different: the primary generator is a language model trained to emit solver-executable artifacts directly, while a symbolic backend is used to validate outputs and to synthesize additional training data. The use of SymPy as a controllable symbolic front-end is especially important for scaling supervision across diverse logic fragments (Meurer et al., 2017).

Self-Repair and Tool Feedback. Outside theorem proving, a large body of work studies how models can improve outputs by reacting to execution feedback, compiler messages, or explicit critics (Yao et al., 2023; Shinn et al., 2023; First et al., 2023; Olausson et al., 2024; Chen et al., 2024; Bouzenia et al., 2025). We adopt the same high-level intuition, but the repair channel here is solver-native: parser errors, sort mismatches, failed

assertions, and counterexamples. This makes the feedback more structured than free-form critique and better aligned with symbolic correctness.

Reinforcement Learning for Verifiable Outputs.

RL has recently become an effective way to improve pass@1 performance in reasoning and formal generation tasks (Shao et al., 2024; Liu et al., 2025; Yu et al., 2025). Our work extends that lesson to SMT-native generation. Because repair is serial and strongly depends on first-round quality, improving the expected solver reward is particularly useful in our setting.

5 Conclusion

Sequent-Prover shows that solver use can be learned as a formal-reasoning policy. By filtering trajectories with executable SMT feedback and contrasting successful traces against failed ones, our method amortizes solver-guided search into the model: it learns to formulate proof obligations, encode them in SMT-LIB, interpret Z3 responses, repair formal artifacts, and commit with evidence. Empirically, post-training turns sample-dependent solver successes into stable behavior, produces more grounded and non-vacuous SMT programs, and shifts correct traces toward refutational UNSAT evidence. The scale-dependent results show where this training helps most: when a base model has latent solver-use competence but lacks consistency. Training on solver interaction is a direct way to make language-model reasoning checkable.

6 Limitations

Sequent-Prover inherits a basic limit of formal verification: a solver can check a formal artifact, but it cannot decide whether the chosen abstraction is the intended one. Expert verification engineers face the same problem when they decide what to model, what to omit, which predicates or theories to use, and which specification captures the informal task. Sequent-Prover makes this step executable and learnable: it trains models to build grounded, non-trivial SMT artifacts, use solver feedback, and revise failed abstractions. Most tasks have no reference formalization, and semantic equivalence is co-NP-complete in propositional logic and undecidable in first-order logic. No automatic check can certify meaning preservation, so our grounding, non-triviality, and structural-uncertainty measures (Appendix D.4) serve as strong proxies for

it. Learning the right level of formal abstraction remains an open problem.

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A Background: Agent, Solver & Training

We first describe three pieces of background: (1) *what Z3 actually computes*, (2) *how the language model interacts with it*, and (3) *how the two post-training recipes differ in what they teach*. The metric definitions that follow depend on these choices.

A.1 The Z3 SMT Solver and the Two Proof Modes

Z3 is a Satisfiability Modulo Theories (SMT) solver: given a quantifier-free first-order formula φ over a combination of background theories (linear/non-linear integer or real arithmetic, fixed-size bit-vectors, equality with uninterpreted functions, strings, arrays, datatypes), Z3 returns one of

- SAT: φ has a model under the theory combination, and Z3 (optionally) returns it as (define-fun $x \dots$) definitions;
- UNSAT: φ is unsatisfiable in the theory combination, so no assignment can make it true;
- UNKNOWN: Z3’s procedure was incomplete or timed out;

- an ERROR string: the input did not parse, used a symbol Z3 cannot interpret, or violated the declared sort.

Our agent uses Z3 in two operationally distinct modes; the distinction is central to how to read the verdict mix in §B.3.

Direct constraint-satisfaction (the SAT mode).

The agent encodes the problem’s premises as constraints and the candidate answer as an additional assertion, then queries (check-sat). A return of SAT establishes that the candidate answer is *consistent* with the premises and the agent may extract a satisfying model from Z3’s (get-model) output. For an open-ended constraint-search task (e.g., ZebraLogic, where the agent must find “the house with the cat”), the witness *is* the answer. For a deductive task (e.g., ProofWriter, where the agent must say whether “ p entails q ”), a SAT verdict on $\Gamma \cup \{q\}$ only tells us that q is *not* contradicted by Γ , which is a strictly weaker claim than $\Gamma \models q$.

Proof by contradiction (the UNSAT mode).

The deductively-strong way to use Z3 for an entailment question is to assert the premises together with the *negation* of the candidate conclusion, $\Gamma \cup \{\neg q\}$, and check satisfiability. A return of UNSAT establishes that $\Gamma \cup \{\neg q\}$ has no model, i.e. $\Gamma \models q$ in the theory. This is the standard refutational argument that underlies essentially all SMT-based theorem proving. An agent that systematically produces UNSAT verdicts on deductive tasks is using the solver as a *prover*; an agent that systematically produces SAT verdicts on the same tasks is using it as a *model-finder* and is, in the worst case, verifying nothing more than internal consistency.

The pair of metrics vcr and $\text{vcr}_{\text{UNSAT}}$ introduced in §B.3 make this distinction quantitative: a delta in $\text{vcr}_{\text{UNSAT}}$ larger than the delta in vcr indicates that post-training is shifting the agent toward refutational rather than model-finding use of the solver, even when the underlying correctness count is unchanged.

A.2 The Tool-Calling Agent Loop

The agent harness uses an *OpenAI-compatible* tool-calling protocol exposed by vLLM. At each iteration τ the model receives the cumulative conversation $\mathcal{H}_\tau = (m_1, m_2, \dots)$ consisting of the system prompt, the user problem, every prior model turn, and the structured payload from every prior tool call. The model may emit one of two output shapes:

1. a *tool call* to `z3_solve` with arguments `{smt_code :  $s_\tau$ , timeout : int}`, parsed by Qwen’s `qwen3_coder` tool grammar; or
2. a *free-text* reply containing the final answer.

Reasoning content (Qwen’s `<think>...</think>` block) is parsed by the `qwen3` reasoning parser and is not re-emitted to subsequent turns; only the model’s user-visible reply plus its tool-call schema is carried forward, which prevents the context window from growing quadratically in $T_{\max}$.

When the agent calls `z3_solve`, the harness runs s_τ under a 30-second wall-clock cap and a 256-MB memory cap, captures the full stdout/stderr, and returns the payload (success, output, sat_result, error) as a tool-response message. The `sat_result` field is taken from the exit line of (check-sat) and normalised to one of {SAT, UNSAT, UNKNOWN}. The original output is kept separately so the agent (and our analyzer) can recover (get-model) witnesses or error stacks.

The loop terminates when the model produces an attempt without a tool call (success), when it explicitly emits the special finish terminator (success), or when τ reaches $T_{\max} = 10$ (timeout). The terminating attempt’s free-text answer is run through the dataset-specific `check_correct` extractor (which handles option-letter matching for MCQ, multiset matching for ZebraLogic, exact-string for arithmetic, etc.) to produce the correctness bit $c_p^{(k)}$.

The boolean attribute `verified` on each JSONL row is *not* the correctness bit. It records whether the agent terminated by producing an answer (`verified = TRUE`) or by hitting the iteration cap and being forced to extract whatever the model last said (`verified = FALSE`); the metrics *soundness* and *completeness* in §B.2 treat `verified` as a confidence/commitment signal, not as correctness.

A.3 Datasets.

We sample 1,000 questions each from the training splits of three hard reasoning datasets: AR-LSAT (Zhong et al., 2021), FOLIO (Han et al., 2024), and ZebraLogic (Lin et al., 2025a). For diversity, we add a small hand-curated set from BBEH (Kazemi et al., 2025), CASP (Hertzberg et al., 2025), Code2Inv (Si et al., 2018), Dafny-Bench (Loughridge et al., 2024), and SV-COMP (Beyer, 2023). The three core datasets were chosen because they require different forms of symbolic reasoning: AR-LSAT consists of analytical

reasoning problems with combinatorial constraints, FOLIO requires reasoning over first-order logical premises, and ZebraLogic contains constraint-satisfaction puzzles with latent entity–attribute assignments. This gives the model supervision on problems where success typically requires constructing and checking an explicit symbolic state, rather than relying only on lexical or parametric associations.

At evaluation time, we enforce strict decontamination: no problem used for training is included in evaluation, and evaluation examples are drawn from held-out splits whenever labels are available. We therefore distinguish between *in-distribution* evaluation, which consists only of held-out examples from the same three core dataset families used during training, and *out-of-distribution* evaluation, which covers all other benchmark sources. This distinction is at the dataset-family level rather than the instance level: even the in-distribution setting is instance-disjoint from training, while the out-of-distribution setting tests transfer to new annotation protocols, answer formats, domains, and reasoning styles.

The evaluation suite is broad. In addition to the three core training benchmarks, we evaluate on logical and rule-based reasoning tasks such as StrategyQA, ProntoQA, ProofWriter, ReClor, and RuleTaker; arithmetic and discrete reasoning benchmarks such as GSM8K, SVAMP, MultiArith, ASDiv, AQUA-RAT, MAWPS, and DROP; spatial reasoning tasks including SpartQA and StepGame; commonsense and science multiple-choice benchmarks including OpenBookQA, QASC, ARC-Challenge, CommonsenseQA, and WinoGrande; broad-coverage multiple-choice benchmarks including MMLU, HellaSwag, BoolQ, MedQA, and MedMCQA; and free-form multi-hop QA datasets including HotpotQA, MuSiQue, SQuAD, and TriviaQA (Geva et al., 2021; Saparov and He, 2022; Tafjord et al., 2021; Han et al., 2024; Zhong et al., 2024; Lin et al., 2025a; Yu et al., 2020; Clark et al., 2020; Cobbe et al., 2021; Patel et al., 2021; Roy and Roth, 2015; Miao et al., 2020; Ling et al., 2017; Koncel-Kedziorski et al., 2016; Dua et al., 2019; Mirzaee et al., 2021; Shi et al., 2022; Mi-haylov et al., 2018; Khot et al., 2020; Clark et al., 2018; Talmor et al., 2019; Sakaguchi et al., 2021; Hendrycks et al., 2020; Zellers et al., 2019; Clark et al., 2019; Yang et al., 2018; Trivedi et al., 2022; Rajpurkar et al., 2016; Joshi et al., 2017). The resulting split tests whether post-training on a small

set of hard symbolic datasets teaches a general solver-use behavior that transfers beyond the observed training distribution, rather than merely improving accuracy on the benchmark families used for supervision.

A.4 Reproducibility.

We have released the SMT harness and its code (<https://github.com/DebaghaG/proofofthought>). We have also released the post-trained 4B, 9B, and 27B models (<https://huggingface.co/saccha-ai>).

Training Data. Our trajectory dataset contains a total of 14,307 training samples, containing 10,785 gold, 2,995 reject, 464 silver, and 63 discarded infrastructure-error trajectories. This data was generated by using our verifier-conditioned trajectory distillation from a Deepseek-v4 Flash model. Temperature during both dataset curation and all evaluation was maintained at 0.9.

Teacher rollouts and acceptance. We sample three teacher rollouts per training problem (eight for part of AR-LSAT). Table 8 gives the teacher’s answer accuracy on the three core datasets. A trace is Gold only when the final answer matches the label, the trace ends with `finish`, the SMT is non-trivial and grounded, and Z3 returns a real verdict. Labels are checked with the same dataset-specific `check_correct` extractor used at evaluation. Rejected traces are incorrect, unfinished, malformed, or have no SMT call. On AR-LSAT, many rejected traces keep calling `z3-solve` without finishing; others contain real formalization errors. A few correct answers were rejected because they did not match the exact output format. On FOLIO, failures are more semantic: the teacher translates premises into predicates inconsistently and mixes up True, False, and Uncertain. We de-duplicate training and evaluation problems, so no training problem appears in the test pool.

Dataset	pass@3	pass^3
AR-LSAT	0.727	0.484
FOLIO	0.853	0.738
ZebraLogic	0.999	0.901

Table 8: Teacher (DeepSeek-v4 Flash) answer accuracy with three rollouts per training problem.

Training details. We post-train models using full-parameter fine-tuning with DeepSpeed ZeRO-3. For supervised fine-tuning, we optimize

the autoregressive language-modeling objective over pre-rendered Qwen3.5 chat-template traces. Each training example is stored as a single text sequence that preserves the exact interleaving of reasoning, tool calls, and solver outputs, including `<think>...</think>`, `<tool_call>...</tool_call>`, and `<tool_response>...</tool_response>` spans. We use this representation directly rather than converting through an OpenAI- or ShareGPT-style message format, since the latter can discard assistant reasoning text when tool calls are also present. This ensures that the model is trained on the same rendered traces that it is expected to produce at inference time.

The supervised run uses the base checkpoint, full fine-tuning, bfloat16 precision, gradient checkpointing, FlashAttention, and a maximum sequence length of 16,384 tokens. We enable sequence packing for throughput. Training is performed for two epochs on 8×A100-80GB GPUs or H200 GPUs according to SLURM availability with per-device batch size 1 and gradient accumulation 8, giving an effective batch size of 64. We use AdamW with learning rate 3×10^{-6} , cosine learning-rate decay, warmup ratio 0.03, weight decay 0, and gradient clipping at norm 1.0. Checkpoints are saved every 10 optimizer steps, retaining the three most recent checkpoints.

We then apply Direct Preference Optimization (DPO) starting from the last supervised checkpoint, where the supervised loss had plateaued in our run. The preference data consists of chosen/rejected SMT autoformalization traces. We train on the entire assistant trajectory, while preserving the same Qwen3.5 chat-template rendering. We use the standard sigmoid DPO objective with $\beta = 0.1$. DPO is also performed with full-parameter fine-tuning, bfloat16 precision, gradient checkpointing, FlashAttention, and DeepSpeed ZeRO-3. Because DPO evaluates chosen and rejected continuations separately and does not use packing, we set the cutoff length to 8,192 tokens and discard preference pairs whose prompt-plus-response length exceeds this limit, avoiding truncation inside tool-call spans. The DPO run is trained for two epochs on 8×A100-80GB GPUs with per-device batch size 1 and gradient accumulation 4, giving an effective batch size of 32. We use learning rate 5×10^{-7} , cosine decay, warmup ratio 0.1, weight decay 0, and gradient clipping at norm 1.0.

Scope, Design Choices and Limitations We

note six scope decisions. First, Sequent-Prover builds on the SMT-LIB backend of Proof-of-Thought (Ganguly et al., 2024), and our contribution is the post-training recipe. We do not compare against Grammars of Formal Uncertainty or VERGE (Singh et al., 2026; Ganguly et al., 2026), which sample hundreds of programs per question, since a fair comparison would need a similar test-time budget. Section 3.4 instead compares our 4B model against its base model with up to sixteen samples and against three prompting baselines. Second, each component ablation needs a full post-training and evaluation sweep, so we do not ablate every design choice at every scale. We ablate the main data-selection choices at 4B on five held-out benchmarks (Section 3.4). Across nine checkpoints and 37 benchmarks, the full recipe improves reliability, grounding, and refutational solver use, and tuning individual components may give further gains. Third, the largest improvements are on the smaller models and on tasks with latent symbolic structure. The 27B Qwen3.6 base model is already a strong tool-caller and code generator, and post-training still brings it near ceiling on the aggregate pool. Most remaining headroom lies in tasks harder than the current suite. Fourth, we report most metrics in aggregate over 7,254 problems per family. This keeps the paper readable, and the pool is large enough for stable deltas. Appendix D.1 gives per-category results for all three families. The auxiliary measures in Appendix B, such as soundness, recovery, calibration, and skeleton diversity, follow the same trends as the main reliability and verifier-use metrics. The predicted SFT-induced drop in soundness does not lower reliability (Appendix D.6). Fifth, curation and evaluation share the same grounding and triviality predicates. These permissive, substring-based criteria apply equally to every checkpoint, including the untrained baselines, so if anything they favor the baselines. The grounding trends also hold when SMT comments are removed (Appendix D.3). Finally, all base models and the DeepSeek-v4-Flash teacher use their own chat-template format, and the mixed point-release versions within a family are the actual base models we used. We have released the SMT harness and the post-trained checkpoints, and will release the 14,307 curated trajectories and prompts, so that the results can be reproduced with the same harness.

License, Compute Budget, Risks All evaluation benchmarks are public datasets used under

their original research licenses; Z3 (MIT), and the Qwen base models (under their respective open model licenses) are used in accordance with their terms, and our harness and checkpoints are released, and our trajectories will be released, under a permissive open-source license for research use. Across prototyping, repeated iteration, teacher rollout generation, SFT, DPO for all three model families, the full sweep used approximately 1000 GPU-hours on A100-80GB/H200 hardware. Our work studies formal reasoning over public academic benchmarks and introduces no new data, deployment surface, or generative capability that poses foreseeable risk of harm.

B Evaluation Metrics

Let $\mathcal{D}$ be a dataset with $M = |\mathcal{D}|$ problems, each problem $x \in \mathcal{D}$ having a gold answer y_x^* . For each problem the model produces K i.i.d. samples; the r -th attempt yields a trace $\tau_x^{(r)}$ and a final answer $\hat{y}_x^{(r)}$, plus a sequence of SMT calls

$$(z_x^{(r,1)}, \dots, z_x^{(r,S_x^{(r)})})$$

with associated Z3 responses

$$(\rho_x^{(r,1)}, \dots, \rho_x^{(r,S_x^{(r)})}).$$

We write

$$\chi_x^{(r)} := \mathbb{1}[\text{check correct}(\hat{y}_x^{(r)}, y_x^*)]$$

$$\chi_x := \sum_{r=1}^K \chi_x^{(r)} \in \{0, 1, \dots, K\}.$$

χ_x is the number of correct attempts on problem x , and $\bar{\chi}_x := \chi_x/K$ is the per-problem accuracy. The boolean $\kappa_x^{(r)}$ denotes the commitment event, i.e. the verified JSONL field of §A.2; $T_x^{(r)}$ denotes the iteration count; and $S_x^{(r)}$ denotes the number of SMT calls. Unless stated otherwise we use $K = 4$.

We use r for the sample index and reserve n for the sample budget inside metrics such as $\text{pass}@n$ and $\text{maj}@n$. We group metrics into three families: *sample-based correctness* (§B.1), which depend only on the bag of correctness indicators $\{\chi_x^{(r)}\}$; *agent-reliability measures* (§B.2), which characterize the relationship between the agent’s internal commitment signal and its actual correctness; and *SMT-program quality* (§B.3), which inspect the structural and semantic properties of the SMT-LIB code that the agent emitted, independent of whether the final answer was correct.

B.1 Sample-Based Correctness

The metrics in this subsection depend only on the multiset $\{\chi_x^{(r)}\}_{x,r}$; they do not inspect the agent trace. Each is a moment of the per-problem correctness count $\chi_x \in \{0, \dots, K\}$ under a different functional form, designed to isolate different facets of the underlying success distribution.

avg@N. The per-attempt expected accuracy, computed as the empirical mean of $\bar{\chi}_x$ across problems:

$$\text{avg}@N = \frac{1}{M} \sum_{x \in \mathcal{D}} \bar{\chi}_x = \frac{1}{MK} \sum_{x,r} \chi_x^{(r)}. \quad (1)$$

avg@N has the operational interpretation of *single-attempt success*: if a user runs the agent once with one temperature draw, avg@N is the expected correctness of that single attempt. It is invariant to which of the K samples we examine, and it is an unbiased estimate of the per-attempt correctness probability.

pass@n. The unbiased estimator of [Chen et al. \(2021\)](#) for the probability that at least one of n samples is correct: for $n \leq K$,

$$\text{pass}@n = \frac{1}{M} \sum_{x \in \mathcal{D}} \left[1 - \frac{\binom{K-\chi_x}{n}}{\binom{K}{n}} \right]. \quad (2)$$

The estimator is unbiased under random subsampling without replacement from the K observed attempts: $\binom{K-\chi_x}{n}/\binom{K}{n}$ is the exact probability of drawing n *all-incorrect* attempts, and pass@n is one minus its expectation. pass@1 = avg@N exactly. For $n > 1$, the quantity reflects the user-facing utility of taking multiple samples and accepting the answer if any one is correct. This is an *ensemble-with-oracle-selection* ceiling. We report pass@4 throughout.

pass^n. The expected probability that *every* one of n samples is correct, under the same random-subsampling estimator:

$$\text{pass}^n = \frac{1}{M} \sum_{x \in \mathcal{D}} \frac{\binom{\chi_x}{n}}{\binom{K}{n}}. \quad (3)$$

$\binom{\chi_x}{n}/\binom{K}{n}$ is the probability that all n randomly drawn attempts are correct. Two checkpoints with the same pass@n may differ dramatically in pass^n : pass^n rewards *distributional concentration on the right answer*: high values arise iff the right answer is the modal answer across samples, low values arise when correctness is sample-dependent.

maj@n. The expected probability that majority vote among n samples agrees with the gold answer. Because check correct is binary, majority on correctness is well-defined and gives, under random subsampling without replacement and the conservative tie convention of strict majority, with ties broken against the agent,

$$\text{maj}@n = \frac{1}{M} \sum_{x \in \mathcal{D}} \frac{1}{\binom{K}{n}} \sum_{j=\lfloor n/2 \rfloor + 1}^n \binom{\chi_x}{j} \times \binom{K-\chi_x}{n-j}. \quad (4)$$

We report maj@4 throughout. The gap between maj@n and avg@N measures the benefit of self-consistency aggregation, which assumes wrong answers are more diverse than right ones.

Stability. The ratio of consistency to coverage,

$$\text{stability} = \frac{\text{pass}^n}{\text{pass}@n} \in [0, 1]. \quad (5)$$

A value of 1 indicates perfect consistency: any problem the model can solve, it solves on *every* attempt. A value of 0 indicates the right answer is purely sample-dependent. The stability delta summarizes the main post-training effect in one number.

AUC@n. A finer-grained “area-under-the-curve” summary of the full $(\text{pass}@j)_{j=1}^n$ trajectory, used internally by the reporting pipeline:

$$\text{AUC}@n = \frac{1}{n} \sum_{j=1}^n \text{pass}@j. \quad (6)$$

AUC@n favors checkpoints whose accuracy at small j is already high, i.e., checkpoints that do not depend on the oracle-selection ceiling. Equivalently, it can be read as a discount on later samples and so penalizes sample-greedy ensembles.

g-pass@n $_{\alpha}$. The threshold-pass variant: g-pass@n $_{\alpha}$ is the expected indicator that at least a fraction $\alpha \in (0, 1]$ of n random samples is correct. $\alpha = 1$ recovers pass^n , and $\alpha = 1/n$ recovers pass@n; we report the standard $\alpha \in \{0.25, 0.5, 0.75, 1.0\}$ ladder in the per-dataset reports of n12smt.analyze, but only the endpoints in the main tables below.

B.2 Agent-Reliability Measures

These metrics compare the agent’s behavior conditional on internal events. As established in §A.2, $\kappa_x^{(r)}$ is the *commitment* bit, namely the JSONL verified field: it is 1 iff the agent terminated the loop by producing an answer rather than hitting $T_{\max}$. $\chi_x^{(r)}$ is the correctness bit. $\kappa_x^{(r)} = 1, \chi_x^{(r)} = 0$ is the *overcommit* cell (the agent declared confidence in a wrong answer) and is the operative quantity in the soundness metric.

Soundness.

$$\text{soundness} = \mathbb{P}(\chi = 1 \mid \kappa = 1) = \frac{\sum_{x,r} \chi_x^{(r)} \kappa_x^{(r)}}{\sum_{x,r} \kappa_x^{(r)}}. \quad (7)$$

Among attempts where the agent chose to finish instead of refining further, what fraction were correct? Soundness is a measure of *decision quality*: it asks whether the agent’s internal stopping criterion is well-aligned with ground truth. Low soundness indicates an agent that “thinks it’s done” while still wrong; a drop in soundness during post-training is diagnostic of training-induced overconfidence, typically introduced by SFT on demonstration traces that all terminate successfully.

Completeness.

$$\text{completeness} = \mathbb{P}(\kappa = 1 \mid \chi = 1). \quad (8)$$

Among correct attempts, what fraction terminated voluntarily? Low completeness is the dual failure to soundness: the agent silently arrives at a correct answer but does not commit, instead burning iterations until it hits $T_{\max}$. In practice every reported configuration in this work has completeness ≥ 0.999 because Qwen reliably emits a finish terminator when satisfied. We retain completeness in the table for diagnostic value and because failure modes on novel datasets could plausibly violate it.

Recovery.

$$\text{recovery} = \mathbb{P}(\chi = 1 \mid \text{first SMT call errored}). \quad (9)$$

The conditioning event is precisely

$$\{\text{success}(z_x^{(r,1)}) = \text{FALSE}\}$$

or the first call’s `sat` result was missing; i.e., the very first Z3 invocation either failed to parse, ran out of resources, or did not return a verdict. *Recovery* measures the agent’s robustness to early failure:

a high value indicates the agent diagnoses the error from Z3’s `stderr` text, rewrites the SMT, and eventually arrives at the right answer; a low value indicates the agent commits to a wrong answer after the first bad call. The difference `recovery - avg@N` measures the *conditional* penalty of a first-call failure, with the unconditional accuracy as baseline.

Expected Calibration Error. With only a binary commitment signal κ and binary correctness χ , ECE collapses to the absolute gap between the per-attempt commitment rate and the per-attempt accuracy:

$$\text{ECE} = |\bar{\kappa} - \bar{\chi}|. \quad (10)$$

For a well-calibrated agent, the rate at which it commits matches the rate at which it is right. Lower values are better; the metric becomes a binned ECE if a continuous confidence signal is supplied via `att["confidence"]`, but none of our checkpoints expose one.

Tool efficiency and turn efficiency. Let $S_x^{(r)}$ be the number of SMT calls and $T_x^{(r)}$ the number of agent iterations in attempt (x, r) . Then

$$\begin{aligned} \text{tool eff.} &= \frac{\text{avg@N}}{\bar{S}}, & \text{turn eff.} &= \frac{\text{avg@N}}{\bar{T}}, \\ \bar{S} &= \frac{1}{MK} \sum_{x,r} S_x^{(r)}, & \bar{T} &= \frac{1}{MK} \sum_{x,r} T_x^{(r)}. \end{aligned} \quad (11)$$

Both ratios are dimensionally CORRECTNESS/COMPUTE-UNIT: tool efficiency is accuracy per SMT call, the marginal value of *querying the solver*; turn efficiency is accuracy per agent iteration, the marginal value of *calling the model*. Because $T_x^{(r)} \geq S_x^{(r)} + 1$, each SMT call consumes an iteration and the final answer takes another, turn efficiency is always the smaller of the two; the gap reflects how much non-solver agent work is being done per problem.

Wasted turns. For each correct attempt, let $\iota_x^{(r)}$ denote the smallest index i such that the i -th SMT call in the trace has

$$\rho_x^{(r,i)} \in \{\text{SAT}, \text{UNSAT}\},$$

i.e., the earliest call that emitted a real Z3 verdict. Then

$$\text{wasted turns} = \mathbb{E}_{x,r:\chi_x^{(r)}=1} \left[\max\{0, S_x^{(r)} - \iota_x^{(r)} - 1\} \right], \quad (12)$$

Attribute	Definition
any non trivial	$\bigvee_i \neg \text{is_trivial}(z_x^{(r,i)})$
any grounded	$\bigvee_i (\text{grounding}(z_x^{(r,i)}, x) \geq \lambda_{\text{gnd}})$
any real verdict	$\bigvee_i (\text{sat_result}_i \in \{\text{SAT}, \text{UNSAT}, \text{UNKNOWN}\})$
any unsat	$\bigvee_i (\text{sat_result}_i = \text{UNSAT})$
first pass valid	$\text{sat_result}_1 \in \{\text{SAT}, \text{UNSAT}, \text{UNKNOWN}\}$
max grounding	$\max_i \text{grounding}(z_x^{(r,i)}, x)$

Table 9: Per-attempt structural attributes computed from the sequence of SMT calls $(z_x^{(r,1)}, \dots, z_x^{(r,S_x^{(r)})})$ in trace $\tau_x^{(r)}$. The grounding threshold is $\lambda_{\text{gnd}} = 0.25$. `is trivial` is the predicate defined in §B.3.2.

the mean number of *additional* SMT calls the agent issued *after* an already-correct call’s verdict was returned. The metric captures retroactive exploration: it is invariant to how hard a problem was, only to whether the agent kept poking after the answer was implicitly already known. Lower is better; a value of zero indicates the agent commits the moment it has a real verdict.

Unique skeletons. For each problem we compute the SMT *skeleton*, the sequence of top-level command tokens of each call,

(declare-const, assert, check-sat, ...),

and count the number of distinct skeletons across the K attempts. The reported value is the mean of this count over $\mathcal{D}$; we also report the mean pairwise Jaccard distance between the skeleton sets of two attempts on the same problem. Both are diversity measures: large values mean the agent explores qualitatively different solution shapes on resampling; small values mean it produces structurally homogeneous attempts.

B.3 SMT-Program Quality

Sample-based correctness metrics measure *what answer* the agent produced; SMT-program quality metrics measure *how it got there*. Each is a function of the SMT-LIB code and Z3 verdicts in the trace, defined first per-call, then per-attempt via the lift in Table 9, then aggregated over $\mathcal{D}$. The per-attempt lift is materially important: an agent that emits a strong SMT call *somewhere* in its trace should not be penalized for also issuing weak exploratory calls, so we use disjunctions, e.g., “any non-trivial,” “any grounded,” and “any real verdict,” rather than averages over calls.

B.3.1 Grounding

The grounding score quantifies how much of the problem’s actual content the agent transferred into

the formal SMT representation. We extract *problem entities* $E(x)$ from the problem text x via the regular expression `[A-Za-z][A-Za-z0-9 ]{1,}` after lower-casing, then remove a small fixed list of ~ 50 English stopwords plus generic task scaffolding tokens, e.g., question, answer, true, false, and option. For an SMT call z , the grounding score is the fraction of problem entities that appear as substrings in the lower-cased SMT body:

$$\text{gnd}(z, x) = \frac{1}{|E(x)|} \sum_{e \in E(x)} \mathbb{1}[e \in \text{lc}(z)], \quad (13)$$

where $\text{lc}(z)$ is the lower-cased SMT body. We use the convention $\text{gnd} = 0$ when $|E(x)| = 0$, which is vanishingly rare in our datasets. Because the predicate “ e is a substring of z ” is permissive (it counts both `declare-const e Int` and any occurrence of e in a comment or sort name), grounding is a *liberal* measure of semantic transfer rather than a strict one, intentionally biased toward false positives so that the metric does not reward *intentional* obfuscation. The reported mean grounding is the per-attempt max grounding averaged over $\mathcal{D}$.

A grounding score of 0.05 on a problem with twenty named entities indicates the SMT references one of them: this is the *decorative-SMT* regime in which the agent has constructed a formally valid program that does not actually express the problem. A score of 0.5 indicates the SMT references roughly half the entities, sufficient for the program to be non-vacuous. The threshold $\lambda_{\text{gnd}} = 0.25$ used in Table 9 is empirically set: it cleanly separates inspection-verified decorative programs from inspection-verified content-loaded ones on a held-out development sample.

B.3.2 Triviality

An SMT call z is *trivial* if any of the following hold:

1. z is empty or whitespace.
2. z contains no `declare-const`, `declare-fun`, or `define-fun`, and contains fewer than two `(assert ...)` forms.
3. Every `(assert ...)` form in z matches a literal tautology or contradiction: `(assert true)`, `(assert (= n n))` with n a numeric literal, `(assert (not true))`, or `(assert (not false))`.
4. z has fewer than 40 non-whitespace characters and at most one assertion.

The first three cases catch *semantically empty* SMT, i.e., programs that contain Z3 syntax but no problem content. The fourth case catches *stub* programs that initiate an SMT-LIB block but abandon it before encoding anything substantive. The `trivial_smt_rate` reported in tables is the fraction of attempts whose *entire* SMT call sequence is trivial:

$$\text{triv}_{\text{SMT}} = \frac{1}{N_{\text{SMT}}} \sum_{(x,r)} \mathbb{1} \left[\bigwedge_i \text{triv}(z_x^{(r,i)}) \right], \quad (14)$$

where N_{SMT} is the number of attempts with at least one SMT call. A non-zero trivial-SMT rate is diagnostic of *performative SMT*: the agent emits Z3 calls to satisfy the tool-calling prior of its training distribution without actually using the solver to reason.

B.3.3 First-Pass Z3 Validity

The fraction of attempts, over those with at least one SMT call, whose very first invocation produced a real Z3 verdict:

$$\begin{aligned} \mathcal{V}_{\text{Z3}} &= \{\text{SAT}, \text{UNSAT}, \text{UNKNOWN}\}, \\ \mathcal{A}_{\text{rv}} &= \{(x, r) : \rho_x^{(r,1)} \in \mathcal{V}_{\text{Z3}}\}, \\ \mathcal{A}_{\text{SMT}} &= \{(x, r) : S_x^{(r)} \geq 1\}, \\ \text{fpv}_{\text{Z3}} &= \frac{|\mathcal{A}_{\text{rv}}|}{|\mathcal{A}_{\text{SMT}}|}. \end{aligned} \quad (15)$$

This isolates whether the agent’s very first SMT-LIB emission is syntactically well-formed under SMT-LIB v2 and well-typed under the declared theory. A delta on this metric during post-training reflects improvements in surface-level SMT syntax (fewer malformed parentheses, undeclared symbols, and theory mismatches), not in reasoning depth.

B.3.4 Verified-Correct Rate and its Unsat Restriction

We now define the strictest accuracy measure used in this work. An attempt is *verified-correct* if the conjunction

$$\begin{aligned} \text{VC}_x^{(r)} &:= \chi_x^{(r)} \\ &\quad \wedge \text{any real verdict}_x^{(r)} \\ &\quad \wedge \text{any grounded}_x^{(r)} \\ &\quad \wedge \text{any non trivial}_x^{(r)} \end{aligned} \quad (16)$$

holds, and verified-correct under UNSAT if it further satisfies any $\text{unsat}_x^{(r)}$. The aggregate rates are

$$\begin{aligned} \text{vcr} &= \frac{1}{MK} \sum_{x,r} \text{VC}_x^{(r)}, \\ \text{vcr}_{\text{UNSAT}} &= \frac{1}{MK} \sum_{x,r} \left(\text{VC}_x^{(r)} \wedge \text{any unsat}_x^{(r)} \right). \end{aligned} \quad (17)$$

vcr filters out four distinct failure modes that can all produce a passing $\chi_x^{(r)} = 1$:

- *No real verdict*: the agent answered correctly without ever obtaining a SAT/UNSAT/UNKNOWN verdict from Z3, i.e., all calls errored or produced no verdict.
- *Decorative SMT*: the agent obtained a verdict but the program had grounding below λ_{gnd} , i.e., the SMT did not reference the problem.
- *Trivial SMT*: every call was structurally trivial.
- *The conjunction held but on a vacuous verdict*: e.g., a SAT verdict on the empty constraint set.

Of these the most dangerous is the no-real-verdict case: it indicates a model that answers correctly via natural-language reasoning while only *performing* SMT use. The post-training vcr delta measures the shift from performative to real solver use.

$\text{vcr}_{\text{UNSAT}}$ goes further by requiring that at least one SMT call in the trace returned UNSAT. By the discussion in §A.1, this is the *refutational-proof* restriction: the agent must have attempted to derive a contradiction from the negation of its candidate answer, and the solver must have confirmed it. The pair $(\text{vcr}, \text{vcr}_{\text{UNSAT}})$ therefore separates correctness into three regimes: (i) correct but performative; (ii) verified-correct via SAT witness, i.e., model-finding; and (iii) verified-correct via UNSAT, i.e., refutational proof.

B.3.5 Z3 Outcome Distribution

For diagnostic and downstream analysis purposes, we additionally report the distribution of Z3 outcomes across every SMT call in the sweep:

$$\omega^{\text{Z3}} = (\omega_{\text{SAT}}, \omega_{\text{UNSAT}}, \omega_{\text{UNKNOWN}}, \omega_{\text{ERROR}}, \omega_{\text{NO-VERDICT}}),$$

with ω_u^{Z3} the fraction of all SMT calls whose sat result took value u . We treat NO VERDICT, where the call succeeded but no sat result token appeared in stdout, as distinct from ERROR, where

the call raised an exception or returned a non-zero exit.

C Comparison to Open-Weight Baselines

We additionally compare the DPO-tuned Sequent-Prover checkpoints against external open-weight baselines evaluated with the same SMT-scratchpad harness. The external baselines are Gemma-4B, GPT-OSS-20B, and GPT-OSS-120B. All deltas are Qwen-DPO minus the external model. Positive values indicate that the Qwen-DPO checkpoint is higher, except for lower-is-better diagnostic rates, such as the trivial-SMT rate, where the raw signed delta is still shown.

Aggregate correctness. Table 10 reports macro-averaged deltas over the shared canonical datasets. The 4B comparison shows a large separation from Gemma-4B: Qwen-4B-DPO improves $\text{avg}@N$ by +0.647, $\text{pass}@4$ by +0.524, and pass^4 by +0.640. The GPT-OSS-20B comparisons show the same pattern at a larger external baseline: Qwen-27B-DPO improves $\text{avg}@N$ by +0.326 and pass^4 by +0.648, while Qwen-9B-DPO improves $\text{avg}@N$ by +0.287 and pass^4 by +0.567. Against GPT-OSS-120B, the comparison is closer and therefore more informative: Qwen-27B-DPO remains positive on all correctness aggregates, including +0.027 $\text{avg}@N$, +0.021 $\text{pass}@4$, and +0.036 pass^4 ; Qwen-9B-DPO is close on coverage, with +0.006 $\text{pass}@4$ and +0.002 AUC@4, while GPT-OSS-120B has the advantage on $\text{avg}@N$ and pass^4 .

Verifier-use diagnostics. Table 11 compares solver-facing behavior. The largest gains are in the verified-correct rate, UNSAT-restricted verified-correct rate, and grounding. Qwen-4B-DPO improves verified-correct rate by +0.615 relative to Gemma-4B. Against GPT-OSS-20B, both Qwen-27B-DPO and Qwen-9B-DPO improve the verified-correct rate by +0.487. The GPT-OSS-120B comparison is particularly useful because it separates answer-level strength from verifier-use quality: Qwen-27B-DPO improves verified-correct rate by +0.341 and grounding by +0.274, while Qwen-9B-DPO improves verified-correct rate by +0.349 and grounding by +0.299. Trivial-SMT rate decreases in all five comparisons, indicating that the DPO checkpoints emit fewer structurally empty or decorative SMT programs.

Per-dataset breadth. Table 12 reports polarity-adjusted win/loss/tie counts across shared datasets. The Qwen-DPO checkpoints dominate the 4B and 20B external baselines across the main sample-based metrics: pass^4 and $\text{maj}@4$ win on every shared dataset in the Gemma-4B and GPT-OSS-20B comparisons. The GPT-OSS-120B comparison is more balanced. Qwen-27B-DPO wins $\text{avg}@N$ on 28 of 35 datasets and pass^4 on 24 of 35 datasets, with five pass^4 ties. Qwen-9B-DPO remains competitive on coverage, winning $\text{pass}@4$ on 22 of 33 datasets, while GPT-OSS-120B has the stronger all-sample consistency profile.

Category-level trends. Table 13 groups the $\text{avg}@N$ deltas by benchmark category. Qwen-DPO is substantially ahead of Gemma-4B and GPT-OSS-20B in every category. Against GPT-OSS-120B, Qwen-27B-DPO is strongest on AGIEval, NLI/MCQ, and hard-reasoning categories, and remains close on arithmetic and free-form QA. The 9B-DPO comparison to GPT-OSS-120B is near parity for AGIEval, arithmetic, hard reasoning, and NLI/MCQ, with larger gaps on spatial and free-form QA tasks.

These external comparisons support two conclusions. First, Sequent-Prover DPO checkpoints are substantially stronger than the same-scale or moderately larger open-weight baselines under the SMT-scratchpad evaluation, with especially large advantages in pass^4 and verifier-grounded correctness. Second, the comparison with GPT-OSS-120B shows that the 27B DPO checkpoint is competitive with a much larger open-weight model on answer-level metrics, while retaining a clear advantage in the use of executable, grounded solvers.

D Additional Analyses

This section gives full tables and details behind the main results: pool composition and per-category results, the test-time sampling study, grounding without comments, structural uncertainty of the generated SMT, teacher support, and soundness.

D.1 Pool Composition and Per-Category Results

Each benchmark contributes 200 problems, except ProofWriter, which contributes 54 because of a data-loader bug. This gives $36 \times 200 + 54 = 7,254$ problems per family. Table 14 groups the pool by category. Table 15 reports Base, SFT, and DPO results within each category. Values are micro-

Comparison	Shared	avg@N	pass@4	pass^4	maj@4	AUC@4	Cons.
Qwen-4B-DPO – Gemma-4B	33	+0.647	+0.524	+0.640	+0.720	+0.591	+0.568
Qwen-27B-DPO – GPT-OSS-20B	35	+0.326	+0.067	+0.648	+0.408	+0.151	+0.699
Qwen-9B-DPO – GPT-OSS-20B	33	+0.287	+0.053	+0.567	+0.372	+0.130	+0.625
Qwen-27B-DPO – GPT-OSS-120B	35	+0.027	+0.021	+0.036	+0.028	+0.023	+0.017
Qwen-9B-DPO – GPT-OSS-120B	33	-0.010	+0.006	-0.039	-0.008	+0.002	-0.054

Table 10: Macro-averaged correctness deltas against external open-weight baselines. Deltas are computed over shared canonical datasets and are reported as Qwen-DPO minus the external model. Cons. is $pass^4/pass@4$. Red marks a deficit.

Comparison	Sound.	Recov.	VCR	VCR-U	FP-Z3	Triv. SMT ↓	Ground.
Qwen-4B-DPO – Gemma-4B	+0.444	+0.490	+0.615	+0.399	+0.164	-0.016	+0.399
Qwen-27B-DPO – GPT-OSS-20B	+0.206	+0.181	+0.487	+0.345	+0.034	-0.032	+0.329
Qwen-9B-DPO – GPT-OSS-20B	+0.172	+0.152	+0.487	+0.321	+0.023	-0.031	+0.353
Qwen-27B-DPO – GPT-OSS-120B	-0.010	-0.020	+0.341	+0.196	+0.004	-0.011	+0.274
Qwen-9B-DPO – GPT-OSS-120B	-0.036	-0.032	+0.349	+0.179	-0.008	-0.011	+0.299

Table 11: Macro-averaged verifier-use deltas. Sound. is $P(\text{correct} \mid \text{finish})$; Recov. is correctness after an initial SMT error; VCR is verified-correct rate; VCR-U additionally requires an UNSAT verdict; FP-Z3 is first-pass Z3 validity; Triv. SMT is trivial-SMT rate, where lower is better; Ground. is mean SMT grounding. Negative entries are green where lower is better and red otherwise.

averages over the problems in each category, using the same four trajectories per problem as the main results.

Hard reasoning has the largest within-category gain at 4B (+0.493 $pass^4$). NLI/MCQ, the largest category, also contributes strongly. At 27B, most categories are near ceiling. The remaining gains are in hard reasoning and spatial reasoning, with small changes on saturated categories.

D.2 Post-Training versus Test-Time Sampling

We compare post-training with a larger test-time sampling budget. We fix 180 held-out problems, 30 each from ProntoQA, FOLIO, AR-LSAT, GSM8K, StepGame, and ARC-Challenge. We draw 16 trajectories per problem from the 4B base model and from the post-trained 4B model. We also draw four trajectories per problem with decomposed prompting, least-to-most prompting, and self-refinement on the base model. All methods share the solver harness, temperature 0.9, a limit of ten agent iterations, the answer checker, and the problem list. The study contains 7,920 trajectories. Table 16 reports equal-weight macro-averages over the six families. Intervals are stratified 95% bootstrap intervals over problems (10,000 resamples).

Beyond the results in Section 3.4, at matched $K = 4$ the post-trained model also improves accuracy by 21.2 points and majority-vote accuracy by 9.9 points over base self-consistency. Against the

base model at $K = 16$, majority-vote accuracies are statistically similar, but the base model needs $6.6\times$ more tokens to get there. Decomposed and least-to-most prompting do not improve over base self-consistency on this mixture.

D.3 Grounding without Comments

Grounding (Eq. 13) counts entity names anywhere in the SMT body, including comments. Comments often hold the only readable mapping from short SMT symbols to problem entities. For example, an AR-LSAT trace may name people J and K in code and explain them in comments. Still, a model could in principle raise grounding by writing entity names in comments. Grounding is not part of any training reward; it is only a filter for Gold traces. To test this concern, we strip comments and recompute grounding on about 50k trajectories from the runs in the main paper. Table 17 shows that post-trained models still have higher grounding than their base models.

D.4 Structural Uncertainty of SMT Programs

Full semantic validation of a formalization is not possible in general (see Limitations). As a symbolic proxy, we follow Ganguly et al. (2026), who show that structural variation in sampled SMT-LIB programs predicts autoformalization errors. We ask whether post-training reduces this structural uncertainty.

Comparison	avg@N	pass@4	pass^4	maj@4	FP-Z3	Sound.	Recov.
Qwen-4B-DPO — Gemma-4B	32/0/1	32/1/0	33/0/0	33/0/0	33/0/0	27/1/5	27/1/5
Qwen-27B-DPO — GPT-OSS-20B	35/0/0	33/1/1	35/0/0	35/0/0	35/0/0	29/1/5	27/2/6
Qwen-9B-DPO — GPT-OSS-20B	33/0/0	31/1/1	33/0/0	33/0/0	29/3/1	26/2/5	23/5/5
Qwen-27B-DPO — GPT-OSS-120B	28/7/0	23/9/3	24/6/5	25/9/1	22/9/4	16/14/5	14/15/6
Qwen-9B-DPO — GPT-OSS-120B	16/17/0	22/8/3	6/26/1	15/16/2	3/29/1	8/20/5	6/21/6

Table 12: Per-dataset win/loss/tie counts. Each entry is W/L/T after metric polarity is applied. Higher is better for all displayed metrics.

Category	4B—Gemma	27B—OSS20	9B—OSS20	27B—OSS120	9B—OSS120
AGIEval	+0.795	+0.376	+0.331	+0.069	+0.024
Arithmetic	+0.686	+0.304	+0.283	+0.021	+0.001
Free-form QA	+0.611	+0.363	+0.312	+0.002	−0.049
Hard reasoning	+0.659	+0.346	+0.317	+0.035	−0.003
NLI/MCQ	+0.647	+0.346	+0.301	+0.044	+0.003
Other	+0.816	+0.307	+0.285	−0.008	−0.022
Spatial	+0.341	+0.143	+0.110	−0.037	−0.071

Table 13: Mean avg@N delta by dataset category. Values are macro-averages over shared canonical datasets in each category.

Category	Bench.	Problems	%
Hard reasoning	6	1,054	14.53
Arithmetic	6	1,200	16.54
Spatial	3	600	8.27
NLI/MCQ	12	2,400	33.09
Free-form QA	4	800	11.03
Other	2	400	5.51
AGIEval	4	800	11.03
Total	37	7,254	100.00

Table 14: Composition of the 37-benchmark evaluation pool.

For task x and independent trajectory j , let $s_{x,j}$ be its final parseable SMT call. We parse $s_{x,j}$ with the SMT-LIB ANTLR grammar and count each grammar production r , giving counts $c_{x,j}(r)$ and the distribution $P_{x,j}(r) = c_{x,j}(r) / \sum_{r'} c_{x,j}(r')$. The lexer drops comments, and all identifiers map to one token class. The measure is unchanged by comments or renaming, but it is sensitive to declarations, quantifiers, operators, and term structure. We compare two distributions with the Jensen–Shannon divergence JSD in base 2, so $\text{JSD} \in [0, 1]$. A value of zero means equal production frequencies. It does not imply identical syntax trees or semantics.

Across trajectories. For trajectory j , we pool the counts of its peers into $\bar{P}_{x,-j}(r) = \sum_{i \neq j} c_{x,i}(r) / \sum_{i \neq j} \sum_{r'} c_{x,i}(r')$ and score $D_{x,j} = \text{JSD}(P_{x,j} \parallel \bar{P}_{x,-j})$. The scored program never enters its own reference. We require at least two parsed peers and average $D_{x,j}$ within each task to get D_x .

Within a trajectory. For a trajectory with $L \geq 2$ parseable SMT calls with distributions $P^{(1)}, \dots, P^{(L)}$, we measure first–last drift $\text{JSD}(P^{(1)} \parallel P^{(L)})$ and mean adjacent drift $\frac{1}{L-1} \sum_{\ell=1}^{L-1} \text{JSD}(P^{(\ell)} \parallel P^{(\ell+1)})$.

For each measure we average over eligible trajectories in a task and report the paired task-level change $\Delta = \mathbb{E}_x[D_x^{\text{tuned}} - D_x^{\text{base}}]$. Negative values mean lower structural variation after post-training.

In Table 18, the drop in peer divergence is largest at 4B and 9B and smaller at 27B, whose base model is already stable. Both within-trajectory drifts also fall in all six comparisons, so tuned models make smaller structural revisions during repair.

D.5 Teacher Support and Student Capability

SFT imitates the teacher conditioned on success, $p_T(\tau \mid x, A(x, y^*, \tau) = 1)$ (Section 2). With K_T samples, at least one accepted trace appears with probability $1 - (1 - Z_T(x))^{K_T}$. The student is bounded by the teacher’s *support* over successful strategies. The same idea underlies expert iteration, where an expensive search procedure produces behavior that a policy learns to reproduce (Ren et al., 2025). It explains why the students exceed the teacher’s all-sample reliability on AR-LSAT (Section 3.6, Table 8). The students learn reusable steps, such as datatype encodings and push/pop option testing (Appendix E.4, E.5). The teacher is weak on some SMT fragments, such as fixed-size bit-vector arithmetic. Our benchmarks do not test these fragments, and a stronger teacher there would extend the same recipe.

Family	Category	avg@N $\uparrow$			pass^4 $\uparrow$		
		BASE	SFT	DPO	BASE	SFT	DPO
4B	Hard reasoning	0.630	0.920	0.931	0.346	0.820	0.839
	Arithmetic	0.899	0.984	0.987	0.691	0.952	0.964
	Spatial	0.735	0.867	0.895	0.372	0.670	0.708
	NLI/MCQ	0.899	0.977	0.985	0.688	0.926	0.953
	Free-form QA	0.956	0.988	0.992	0.841	0.960	0.971
	Other	0.950	0.988	0.997	0.815	0.950	0.988
9B	AGIEval	0.900	0.955	0.970	0.729	0.863	0.911
	Hard reasoning	0.853	0.938	0.951	0.687	0.859	0.871
	Arithmetic	0.979	0.988	0.989	0.945	0.970	0.973
	Spatial	0.896	0.891	0.916	0.707	0.712	0.777
	NLI/MCQ	0.968	0.987	0.987	0.887	0.959	0.960
	Free-form QA	0.987	0.993	0.997	0.949	0.978	0.989
27B	Other	0.973	0.992	0.996	0.905	0.968	0.988
	AGIEval	0.953	0.976	0.976	0.863	0.911	0.914
	Hard reasoning	0.967	0.974	0.992	0.924	0.926	0.976
	Arithmetic	0.996	0.996	0.998	0.993	0.988	0.994
	Spatial	0.962	0.945	0.974	0.912	0.858	0.923
	NLI/MCQ	0.998	0.998	0.999	0.993	0.994	0.995
	Free-form QA	1.000	0.999	0.999	1.000	0.996	0.995
	Other	0.999	0.999	1.000	0.998	0.998	1.000
	AGIEval	0.998	0.993	0.997	0.988	0.980	0.990

Table 15: Per-category results for Base, SFT, and DPO checkpoints. Values are micro-averages over the problems in each category.

D.6 Soundness after SFT

Soundness is the accuracy of attempts on which the agent chooses to finish (Eq. 7). All SFT demonstrations are successful traces that end cleanly. This may teach the model to commit early, which would lower soundness. We do not see a broad drop in reliability that would confirm this effect. After DPO, soundness improves, together with fewer malformed SMT calls and more refutational solver use (Table 5b).

E Qualitative Improvements

The quantitative improvements we observe in our metrics, such as verified correct rate with unsat (i.e., appropriate proof by contradiction) correlate to formalizations that, on manual inspection, follow the problem statement much more closely. Throughout the harness and metric development, as well as during intermediate checkpoints, we took care to ensure there is no reward hacking and quantitative improvements actually manifest via qualitative improvements in formal artifact quality. In this section, we attempt to elucidate a few learnt capabilities in our trained models.

E.1 Capability 1: SMT-LIB language fluency

The most basic gap is that the 4B Qwen Base model emits malformed SMT-LIB on the first attempt and

rarely recovers, whereas Sequent-Prover-4B recovers in one turn. We illustrate with ProntoQA-95 (gold = B): a chain of universal implications resolves to “Stella is not feisty.”

Qwen-4B. The first tool call is itself malformed at the envelope level: the model wraps its payload as `{"arguments": "{\smt_code\":"\smt_code\}"}`, which the harness’s JSON-strict tool parser cannot decode, so the next tool message is empty. Inside the inner payload the SMT-LIB is also incorrect:

```
(set-logic QF_LIA)
(declare-const IsFeisty Pred)
(declare-const IsVumpus Pred)
(declare-const Stella Bool)
(assert (= (IsVumpus Stella) True))
(assert (forall ((x Pred))
  (=> (= (IsVumpus x) True)
    (= (IsDumpus x) True))))
```

Several things are wrong: `Pred` is not a sort, `IsVumpus` is declared as a constant but applied as a function, `Stella` is a `Bool` but used as an individual, and `True` appears as a value rather than the SMT-LIB literal `true`. Over the next eight turns the model loses track of the original problem and cycles through `(set-logic QF_LIA)`, `(set-logic QF_BV)`, `(set-logic QF_NIA)`, and `(set-logic QF_ARITH)`, the last of which is not a recognized logic name. It exhausts its 10-turn

Method	K	Accuracy [95% CI] $\uparrow$	pass@ K $\uparrow$	pass [^] K $\uparrow$	Stab. $\uparrow$	Tokens/problem $\downarrow$	Cost $\downarrow$
Base + self-consistency	1	0.657 [0.623, 0.689]	0.657	0.657	1.000	27,729	1.7 $\times$
Base + self-consistency	2	0.657 [0.623, 0.689]	0.780	0.533	0.683	55,458	3.3 $\times$
Base + self-consistency	4	0.657 [0.623, 0.689]	0.848	0.395	0.466	110,915	6.6 $\times$
Base + self-consistency	8	0.657 [0.622, 0.689]	0.887	0.257	0.290	221,831	13.3 $\times$
Base + self-consistency	16	0.657 [0.623, 0.689]	0.911	0.139	0.152	443,662	26.5 $\times$
Base + decomposed	4	0.590 [0.550, 0.629]	0.828	0.267	0.322	117,070	7.0 $\times$
Base + least-to-most	4	0.621 [0.578, 0.661]	0.828	0.317	0.383	121,933	7.3 $\times$
Base + self-refine	4	0.772 [0.733, 0.811]	0.928	0.572	0.617	219,491	13.1 $\times$
Post-trained 4B	1	0.869 [0.836, 0.899]	0.869	0.869	1.000	16,718	1.0 $\times$
Post-trained 4B	2	0.869 [0.835, 0.899]	0.924	0.813	0.880	33,437	2.0 $\times$
Post-trained 4B	4	0.869 [0.836, 0.900]	0.948	0.743	0.784	66,873	4.0 $\times$
Post-trained 4B	8	0.869 [0.836, 0.900]	0.960	0.663	0.690	133,747	8.0 $\times$
Post-trained 4B	16	0.869 [0.835, 0.900]	0.967	0.572	0.592	267,493	16.0 $\times$

Table 16: Post-training versus test-time sampling on 180 held-out problems from six benchmark families. Cost is token use relative to the post-trained model at $K = 1$.

Family	SFT – Base	DPO – Base
4B	+0.074	+0.081
9B	+0.051	+0.051
27B	+0.039	+0.036

Table 17: Change in mean grounding after removing SMT comments.

Comparison	Across traj.	Within traj.	
	Δ [95% CI] $\downarrow$	First-last $\downarrow$	Adjacent $\downarrow$
4B Base→SFT	-0.023 [-0.025, -0.021]	-0.029	-0.011
4B Base→DPO	-0.019 [-0.021, -0.016]	-0.027	-0.008
9B Base→SFT	-0.019 [-0.021, -0.017]	-0.024	-0.010
9B Base→DPO	-0.016 [-0.018, -0.015]	-0.026	-0.010
27B Base→SFT	-0.008 [-0.010, -0.006]	-0.006	-0.005
27B Base→DPO	-0.004 [-0.006, -0.002]	-0.011	-0.006

Table 18: Paired change in structural (PCFG production) divergence over 1,382 tasks per comparison. Across-trajectory intervals use 4,000 bootstrap resamples, stratified by dataset. Within-trajectory columns are point estimates.

budget without emitting finish.

Sequent-Prover-4B. The first tool call is a first-order encoding with a sort for animals and one predicate per type:

```
(declare-sort Animal)
(declare-fun vumpus (Animal) Bool)
(declare-fun dumpus (Animal) Bool)
(declare-fun rompus (Animal) Bool)
;; ... (one declare-fun per category) ...
(declare-const Stella Animal)

(assert (forall ((x Animal))
  (=> (vumpus x) (dumpus x))))
;; ... the full implication chain ...
(assert (forall ((x Animal))
  (=> (zumpus x) (not (feisty x)))))
(assert (vumpus Stella))
(assert (feisty Stella)) ; proof by
  contradiction
```

```
(check-sat) ; --> unsat
```

The first pass surfaces two ordering errors (yumpus and floral are used before they are declared). The Sequent-Prover-4B model reads the Z3 error, hoists the missing declare-funs to the top, re-runs, and finishes in four turns. The base model, given the same kind of error, could not localize the fix and drifted into unrelated probes.

E.2 Capability 2: First-order over propositional encoding

When a problem quantifies over individuals, a propositional encoding (one boolean per ground proposition) often forces the model to fill in unstated relations by hand. The base model does this routinely; Sequent-Prover does not.

Example: FOLIO-1361 (gold = Uncertain).

Premises describe animals near a campus and ask whether “Rockie is a turtle.” The critical step is to notice that the premises constrain Rockie to be a rabbit *or* a squirrel but say nothing about whether rabbit and turtle are disjoint categories.

Qwen-4B. Declares one Bool per (Rockie, property) pair, with no animal sort. Lacking a way to express “the only animals spotted are rabbits and squirrels” as a universal, it instead injects two disjointness axioms that are nowhere in the premises:

```
; Assumption: Rabbits and turtles are mutually
  exclusive
(assert (=> RockieRabbit (not RockieTurtle)))

; Assumption: Squirrels and turtles are mutually
  exclusive
(assert (=> RockieSquirrel (not RockieTurtle)))
```

With those injected axioms, the negation of the conclusion becomes unsat and the base model answers “False”, but the false answer is caused by the hallucinated premise, not by the actual problem.

Sequent-Prover-4B. Uses a sort `Animal` and universally quantified predicates over animals; the conditions in the prompt translate mechanically. We discuss Sequent-Prover’s full reasoning trajectory under Capability 6 below, because it turns on the same diagnostic move used in another problem.

E.3 Capability 3: Bidirectional satisfiability for ternary verdicts

When the verdict space is ternary (True / False / Uncertain) or four-valued (entailment / contradiction / neutral / self-contradiction), a single satisfiability query is not enough. The textbook idiom is two queries: assert the conclusion, then assert its negation. The base model asks one and stops; Sequent-Prover-4B asks both.

Example: FOLIO-476 (gold = False). “Tom will rent at the Olive Garden,” under premises constraining the deposit to be at least the monthly rent (\$2000) and renting to require deposit $\leq$ \$1500.

Qwen-4B. Spends three turns repairing surface syntax (`Boolean→Bool`; `->` to `implies`; `(= oliveWillRent False)→(not oliveWillRent)`). Its single satisfiability query is on the negation of the conclusion, which is sat, so it concludes “the conclusion does not necessarily follow” and answers **Uncertain**. It never asks the dual question.

Sequent-Prover-4B. Runs both queries:

```
;; First: can the conclusion FAIL under the
premises?
(assert (not (and allowed_to_move_in_with_fluffy
  (<= deposit_at_olive 1500.0))))
(check-sat) ; sat ==> not forced True

;; Second: can the conclusion HOLD under the
premises?
(assert (and allowed_to_move_in_with_fluffy
  (<= deposit_at_olive 1500.0)))
(check-sat) ; unsat ==> forced False
```

(sat, unsat) is the signature of a forced-false conclusion. Sequent-Prover-4B finishes with **False** and the explanation reproduces the informal argument: “\$2000 > \$1500, so the deposit condition cannot be satisfied at the Olive Garden.” Both models know `check-sat`. Sequent-Prover-4B differs in its choice of *which experiment to run next*.

E.4 Capability 4: Declarative encoding via datatypes

For problems with explicit finite enumerations (LSAT analytical reasoning, ZebraLogic), the canonical idiom is `declare-datatypes` for each finite domain and a function from one to another.

Example: AR-LSAT-82 (gold = E). Seven sales reps assigned to three zones, with same-zone pairings and an XOR-style either-but-not-both rule. “If more reps work in Zone 1 than in Zone 3, which one of the following *could* be true?”

Qwen-4B. Encodes zones as unbounded integers and emits a wall of errors over all ten turns. The actual bugs we observe across calls are: bare `<=` 1 zone_Kim 3 expressions at top level (outside any `assert`); ad-hoc `[zone_Kim]` bracket syntax; `(select zone_Kim 1)` on a `Bool` as if to extract an indicator; the conjunction `(and (= zone_Kim 1) (= zone_Kim 2) (= zone_Kim 3))` used where the intent is disjunction; `(if ...)` instead of `(ite ...)`; and a multi-variable `declare`, `(declare-const zK zM zP zQ zS zT zU Int)`, which is not legal SMT-LIB. None of these get repaired; each turn re-issues a near-copy of the previous one. The model never reaches the option-testing stage.

Sequent-Prover-4B. First turn uses two algebraic datatypes and an abstract assignment function:

```
(declare-datatypes ()
  ((Rep Kim Mahr Parra Quinn Stuckey Tiao Udall))
)
(declare-datatypes ()
  ((Zone Zone1 Zone2 Zone3)))
(declare-fun zone (Rep) Zone)

(assert (xor (= (zone Parra) Zone1)
  (= (zone Tiao) Zone1))) ; rule 1
(assert (xor (= (zone Tiao) Zone2)
  (= (zone Udall) Zone2))) ; rule 2
(assert (= (zone Parra) (zone Quinn))) ; rule 3
(assert (= (zone Stuckey) (zone Udall))) ; rule
4

(define-fun count1 () Int
  (+ (ite (= (zone Kim) Zone1) 1 0)
    (ite (= (zone Mahr) Zone1) 1 0)
    ;; ... ite per rep ...
  ))
```

The first pass writes `(not = =)` (the constraint is “exactly one of X, Y”) and gets a Z3 arity error; the next turn replaces the offending subtree with `xor`. This is the same local-edit recovery pattern as in Capability 1.

E.5 Capability 5: Push/pop for multi-option queries

LSAT analytical-reasoning items ask “which of the following could / must be true?” This is a sequence of independent satisfiability tests on a shared base theory, the SMT idiom is push/pop.

Example: AR-LSAT-60 (gold = E). Four riders and four bicycles across two days, with same-day distinctness and cross-day pairing. “If Theresa tests G on day 2, which one of the following *must* be true?”

Qwen-4B. Across all ten turns the same family of errors recurs. Seven of the ten calls contain (assert (= T1 H) or (= T2 H)), with or as a top-level token outside the assert, which Z3 reports as “invalid assert command,) expected.” The base model treats the error as a request to reformat the body (one call collapses the entire program onto a single line; another swaps in unbounded integer ranges), but never rewrites the offending node as the prefix expression (or (= T1 H) (= T2 H)). It never reaches the option-testing stage, and extraction_method = none.

Sequent-Prover-4B. A single first-turn body emits five push/pop blocks, one per answer choice. Each block asserts the *negation* of the choice: the canonical pattern for “must be true” (if the negation is unsat, the choice must hold). (echo ...) labels the output so the five results are self-describing.

```
(declare-datatypes () ((Rider R Seamus T Yuki)))
(declare-datatypes () ((Bike F G H J)))
(declare-fun d1_rider (Bike) Rider)
(declare-fun d2_rider (Bike) Rider)

(assert (distinct (d1_rider F) (d1_rider G)
                  (d1_rider H) (d1_rider J)))
;; ... base theory ...
;; Yuki's day-1 bike is Seamus' day-2 bike:
(assert (exists ((b Bike))
  (and (= (d1_rider b) Yuki)
        (= (d2_rider b) Seamus))))

(echo "Testing (A): Reynaldo tests H, day 1")
(push) (assert (not (= (d1_rider H) R)))
       (check-sat) (pop)
;; ... (B), (C), (D) similarly ...
(echo "Testing (E): Yuki tests H, day 2")
(push) (assert (not (= (d2_rider H) Yuki)))
       (check-sat) (pop)
```

The single Z3 reply ends with “Testing (E) ... unsat,” the unique negation that is unsat, so (E) is the choice that must hold. Sequent-Prover finishes in three iterations, having issued one comprehensive query instead of five separate ones.

Three sub-skills are bundled into this first turn: (i) scaffolding with (echo ...) so the Z3 output is self-describing; (ii) knowing *which* direction to negate for “must” vs. “could” questions; (iii) knowing that push/pop make the five queries independent. Note also the encoding choice: inverting the natural direction to d1_rider : Bike → Rider lets the day-1 distinctness collapse to a single distinct call instead of six pairwise inequalities, and lets the cross-day pairing be expressed as a one-variable existential.

E.6 Capability 6: Diagnosing inconsistent premise sets

A further capability is noticing that *both* directions of a hypothesis are unsatisfiable, and inferring from that signature that the premise set itself is inconsistent. This move appears in two superficially unrelated transcripts.

Example A: Self-Contradiction We used an example where there were twelve atomic facts and twelve quantified rules over seven named people. The base model spends ten turns rewriting an 80-line SMT body, hitting the same family of errors each time (missing Harold_brainy declaration, mis-parenthesized rules 4 and 9). At turn 22 the harness sends a follow-up nudge, “Z3 returned a clean UNSAT result, so now emit the finish tool call”, but the base ignores it and issues yet another z3_solve on turn 23.

Sequent-Prover-4B arrives at the verdict in three steps. First, it tests both directions of the hypothesis:

```
;; (a) Is the hypothesis decidable?
(push) (assert R_huge)
       (check-sat) (pop) ; unsat
(push) (assert (not R_huge))
       (check-sat) (pop) ; unsat
```

Both unsat is the diagnostic signature of an inconsistent premise set. Sequent-Prover-4B confirms by querying the premises alone:

```
;; (b) Are the premises consistent?
;; ... (only premises asserted) ...
(check-sat) ; unsat
```

and finishes with self_contradiction. The explanation it produces names the diagnostic explicitly: “Z3 returns UNSAT for the premises alone, and both R_huge and (not R_huge) are UNSAT when added to the premises. This means the premises contain an inherent contradiction.”

Example B: FOLIO-1361, revisited. Recall that the base model on this problem (§E.2) injected ad-hoc disjointness between rabbits, squirrels, and turtles and got an unsound “False.” Sequent-Prover’s trajectory uses the same diagnostic move as Example A:

1. No injected axioms; both directions of (turtle Rockie) return sat. (Sequent-Prover-4B does not yet realize this is the “Uncertain” answer.)
2. Adds disjointness axioms; the negation goes unsat, suggesting the conclusion is forced True.
3. Runs both push/pop queries on the disjointness-augmented theory: *both* are unsat.
4. Recognizes both-unsat as a contradiction signature, checks the augmented premises alone, gets unsat.
5. Concludes the disjointness axioms are not licensed; removes them.
6. Re-runs both directions: sat and sat.
7. Finishes with Uncertain and a verbal argument: “The premises do not state that rabbits and turtles are disjoint categories.”

The same meta-strategy, both UNSAT $\Rightarrow$ suspect your own axioms, recovers the correct answer on a problem where the base model is led astray by the axioms it injected. This strategy spans several solver calls, and the model learns it as a policy over them.

E.7 Capability 7: Error text as actionable feedback

The recovery patterns we observe in Sequent-Prover-4B follow a recurring structure: read the Z3 error string, locate the named node in the previous body, and patch it without disturbing the rest of the program. The base model either re-emits a near-identical body or escalates the change in unhelpful directions (switching logics, collapsing onto one line). Three mini-patterns:

ProntoQA-95.

Error: “unknown constant yumpus (Animal).”
 Fix: move (declare-fun yumpus (Animal) Bool) above its first use. Nothing else changes.

FOLIO-476.

Error: “unknown sort Boolean.” Sequent-Prover-4B patches each occurrence to Bool in one turn. The base model takes two extra turns to also re-discover that $\rightarrow$ is not a keyword

(use $\Rightarrow$ or implies) and that the constant is lower-case false.

AR-LSAT-82.

Error: “Wrong number of arguments (2) passed to function not.” Fix: replace the entire (not (= ...) (= ...)) subtree with (xor (= ...) (= ...)), which is both shorter and correct.

The base model knows the SMT-LIB tokens. It does not use the parse tree, symbol table, or scopes to locate an error, and it retries the whole program instead.