AI ALIGNMENT VIA INCENTIVES AND CORRECTION

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ABSTRACT

We study AI alignment through law-and-economics models of deterrence, in which misconduct is a strategic response to the probability of detection and the severity of punishment. The same logic arises in agentic AI pipelines: a solver may gain from a persuasive but incorrect answer, while an auditor must decide whether costly monitoring is worthwhile. This creates a coupled incentive problem: stronger penalties deter the solver but can weaken the auditor's incentive to inspect, since once the solver seems reliable, auditing mostly incurs cost and rarely catches errors. This perspective also changes what should count as a post-training signal: rather than rewarding the final answer alone, a solver–auditor pipeline lets rewards target the full correction event. We formalize this in a two-agent model in which a principal chooses these rewards, making reward design a bilevel optimization problem judged by the behavior it induces. We propose a bandit-based outer loop that searches over reward profiles using noisy interaction feedback. On an LLM coding pipeline, adaptive reward profiles maintain oversight pressure and improve held-out principal value over static hand-designed rewards, reducing hallucinated incorrect attempts by 52%.

1 INTRODUCTION

In this paper, we study AI alignment to human values from the perspective of methodologies proposed to align humans to human values. Our starting point is the 1968 paper by Becker (Becker, 1968), that transformed the study of criminal behavior by introducing the "Rational Offender" hypothesis. He argued that the decision to commit a violation is an expected-utility calculation: a rational actor weighs the gain from misconduct against the probability of detection and the severity of punishment. This allowed a mechanistic basis for social welfare maximization based on game theoretic principles.

Classical work extended this logic by asking how enforcement itself should be chosen (Stigler, 1970) and by emphasizing that enforcers also require incentives (Becker & Stigler, 1974). For example, to avoid collusion between enforcers, (Becker & Stigler, 1974) proposed sufficient rewards to render collusion and its dangers irrational from a rational offender perspective. This shifts the societal paradigm of enforcer selection by "ethical behavior" to a rational selection process, relying on purely game theoretic assumptions.

The Analogy to AI Alignment. Analogously, in AI alignment, a model may obtain a small gain from misaligned behavior—e.g., by giving a persuasive but incorrect answer, hiding uncertainty, or exploiting spurious shortcuts—while facing some chance of review and penalty if caught. In LLMs, such penalties are implemented through post-training updates. The analogy is especially natural in agentic pipelines, where a *solver* produces an answer and a *verifier* or *auditor* decides whether to escalate the output to a more extensive resolution procedure such as debate (Irving et al., 2018). We use incentive language operationally: an agent is "incentivized" toward a behavior when the training signal locally increases the probability of that behavior, not when the model consciously reasons about rewards.

Endogenous Monitoring. A key difference from the simplest deterrence story is that monitoring is itself chosen by another learned policy, such as a verifier or reward model. Alignment is therefore a fixed-point problem: stronger penalties can deter the solver, but they can also weaken the incentive to audit, since auditing then mainly incurs cost and false-positive risk on a population that is already mostly aligned. The same tension appears in the economics of enforcement and organizations, where

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regulators choose monitoring intensity and supervisors may collude with the agents they oversee (Mookherjee & Png, 1992; Tirole, 1986; Kofman & Lawarree, 1993). Solver and verifier policies may likewise collude or strategically accommodate one another (Dang et al., 2026). Thus our setting treats monitoring itself as strategic: the solver's incentive to misbehave and the auditor's incentive to inspect are jointly determined by the mechanism.

A Strategic Signal for Post-Training. In standard post-training, feedback is often attached to the final answer alone. But in a solver–auditor pipeline, the important object is the full correction event: whether the solver erred, whether the auditor chose to inspect, whether the error was caught, etc. These cases are behaviorally distinct, yet standard scalar feedback often conflates them. In particular, rewarding correctness alone can encourage guessing rather than admitting uncertainty (Kalai et al., 2026). By assigning rewards to joint solver–auditor outcomes, we obtain a richer training signal that distinguishes aligned success, informative catches, and silent failures.

This richer signal is especially relevant for alignment. A post-training scheme should not only reduce misbehavior, but should do so without removing incentives to monitor. Recent monitoring and critic-based systems show that intermediate oversight can help detect bugs, hallucinations, reward hacking, and other forms of misbehavior (Wen et al., 2026; Baker et al., 2025; McAleese et al., 2024). Our mechanism makes the incentive tradeoff explicit and therefore allows post-training to optimize for correction, not just for surface-level performance.

Relation to Prior Work. Our work connects to proxy objectives and scalable oversight: optimized feedback can diverge from the designer's goal, while oversight methods seek better supervision for hard-to-evaluate tasks (Amodei et al., 2016; Christiano et al., 2017; Irving et al., 2018). We study how rewards induce the joint behavior of an adaptive solver and auditor. Bilevel reward design and multi-agent incentive learning provide related tools (Chakraborty et al., 2023; Du et al., 2019; Yang et al., 2020); our focus is how reward choices affect both misconduct and monitoring. The framework also complements AI control and untrusted monitoring (Greenblatt et al., 2024b; Griffin et al., 2024; Shlegeris, 2024) by treating the auditor's policy as induced by the reward mechanism. The central coupling is that rewards deterring solver misconduct can simultaneously make auditing less valuable. Appendix I provides the fuller discussion and additional comparisons.

1.1 Our Contributions

A Principled Game-Theoretic Model for AI Alignment. In Section 3, we propose a theoretical model to study the mechanism design of a simple two-agent AI pipeline, a solver and an auditor. Every round, the two agents give a response, and we assign rewards according to a design that the end user can choose dynamically every round. We explain why dynamic rewards are better for user outcomes over static rewards. Using known techniques in (bi-level) bandit optimization, we describe an algorithm that adaptively searches for high-value reward profiles.

Reward Design for Agentic LLM Pipelines. In section 4 we propose a new theoretically motivated reinforcement learning framework to train agentic pipelines. We apply this framework to train a small coding pipeline. One of the agents solves a programming problem while the other checks the first agent's code. We show that our approach has more aligned outcomes than standard reward design. On the matched-distribution validation evaluator, this leads to a 52% reduction in coding hallucinations compared to the best main baseline; on downstream LiveCodeBench, it yields a 32% reduction relative to the fixed-binary baseline.

2 A Toy Example: Solver–Auditor Game

Before developing the general theory, we illustrate the techniques on a small self-contained example. It has the three ingredients the rest of the paper generalizes: an agentic task, a basic model of the induced game, and an outer-loop algorithm that searches over reward designs.

Setup. In this example, we describe a game that gives rise to more aligned agents from a generic utility perspective. In Section 4 we give a concrete generalization of this setting to coding agents, and Section 5 shows how they become less prone to hallucination. The two players in our toy game are the

solver and the *auditor*, each small MLPs mapping scalars to logits. The scalar inputs represent a set of tasks, each with a "difficulty level" scalar $\tau \in \mathbb{R}$ providing a noisy signal for the solver's success probability $\hat{p} \approx p(\mathsf{correct} \mid \tau)$. Based only on this signal, the solver outputs a distribution over attempting a solution or abstaining from responding, $a_S \in \{\mathsf{attempt}, \mathsf{abstain}\}$. The auditor uses the same scalar with the solver's solution and outputs a distribution over whether to *audit* (flagging the solution as wrong) or *abstain* (declaring the solution correct). Finally, we update the solver and auditor weights using a *reward profile*, a small vector of scalar rewards attached to the outcome of the task. We denote this process an *inner round*; pseudocode is given in Appendix F.1.

The mechanism designer, the human(s) overseeing the training, specifies a value function over outcomes, representing the principal's true objective. This need not be the same as the reward function given to the agents during training: rewards are only a tool for steering behavior, and our goal is to choose them so that the resulting adapted policies maximize the principal's value.

For this task, we explicitly define the value matrix in Appendix F.4. In particular, denote a batch of $\mathcal{I} > 0$ inner rounds as an *outer round*. Notably, the reward profile $\mathbf{r}$ is constant within each fixed outer round, but in general may change *across* different outer rounds. We learn a *meta-controller* policy which selects between different predefined reward vectors. The full co-evolution process is given in Appendix F, Algorithm 4. The numerical values in Table 12 are not meant to be canonical. They encode one operational preference ordering: aligned unaudited answers are best, caught failures are preferable to silent failures, false positives are costly, and abstention is safer than a hallucinated attempt. Our qualitative claim is that the reward mechanism should be optimized against such a principal value function, rather than that these particular constants are uniquely correct.

We use the toy setup to compare different choices for multi-armed bandit (MAB) meta-controllers, described in Appendix C. The tested controllers reach similar principal values, with Thompson sampling improving earlier in these runs (see Appendix F.3).

3 Mechanism Design for Alignment

The toy example above illustrates the main phenomenon: rewards do not act directly on the principal's objective. They first induce behavior by the solver and the auditor, and only then determine the principal's value. We now formalize a basic model of this mechanism-design problem.

The Basic Solver-Auditor Game. Let μ be the distribution over prompts, and take $x \sim \mu$ to be a prompt. Suppose the solver has parameters θ_1. For each such prompt, the solver has a latent alignment probability $p_{\text{align}}(x) \in [0, 1]$ leading to outcomes $Z \sim \text{Bernoulli}(p_{\text{align}})$, where $Z = 1$ corresponds to alignment and $Z = 0$ to misalignment (for clarity, we will interchangeably use $A \equiv 1$ and $M \equiv 0$ as the outcomes). Now suppose the auditor has parameters θ_2, and suppose it chooses to audit with probability $p_{\text{audit}}(x) \in [0, 1]$. If an audit occurs, a reward mechanism assigns training rewards to realized correction events.

This is a deliberately minimal, prompt-conditioned spot-checking abstraction: the audit probability depends on x, not on the solver's realized output. It keeps the utility and equilibrium analysis tractable, while the LLM implementation in Section 4 uses a richer auditor that sees the generated output.

In the minimal model, the solver receives reward $r_A(x)$ after audited verdict $Z = A$ (or lack of an audit) and signed punishment $r_M(x)$ after audited verdict $Z = M$, with $r_A(x) > r_M(x)$. The auditor receives reward $r_{\text{catch}}(x)$ for correctly catching a misaligned response, pays cost $r_{\text{fp}}(x)$ for a false positive, and pays direct audit cost $r_{\text{audit}}(x) < r_{\text{catch}}(x)$.

The richer reward profiles used in the experiments are larger versions of this setup. They assign rewards to more granular outcomes such as aligned success, caught failure, silent failure, false positive, abstention, and invalid audit. In general (non-verifiable) settings, the verdict could be the result of a debate or judge protocol (Irving et al., 2018), which itself is inherently noisy. Furthermore, the solver could itself have noise, as may be the case for e.g. a complicated LLM solving pipeline. For simplicity, we elide those concerns in this section and give an analysis of these cases in Appendix A.

3.1 THE PRINCIPAL'S OBJECTIVE

The principal (a human/entity overseeing training) does not directly care about the agents' training rewards, but rather cares about the behavior induced by those rewards. Let

$$W(\theta_1, \theta_2) = \mathbb{E}_{x\sim\mu}[V(x, p_{\text{align}}(x;\theta_1), p_{\text{audit}}(x;\theta_2))] \tag{1}$$

be the principal's value. The function V evaluates the full correction outcome, not only the final answer; for instance, in the hallucination example, the outcome would track aligned success, caught failure, silent failure, false positive, abstention, and any other correction events relevant to the task.

For a solver-auditor pipeline, it may make sense that $V(x,1,0) > V(x,0,0)$ and $V(x,1,0) > V(x,1,1)$. This indicates that the principal is happier when the solver is aligned over not, but *is also happier when the auditor doesn't audit unnecessarily* (auditing might be relatively expensive, as we have to run a verdict resolution).

Let $\mathbf{r}$ denote a reward mechanism, drawn from a feasible class $\mathcal{R}$ (e.g., the available reward vectors available to the Thompson meta-controller). For each $\mathbf{r}$, the solver and auditor adapt under the induced training game. After a specified inner adaptation procedure, we write the resulting policies as $(\theta_1^*(\mathbf{r}), \theta_2^*(\mathbf{r}))$.[1] The principal's reward-design problem is therefore

$$\max_{\mathbf{r}\in\mathcal{R}} \ \Phi(\mathbf{r}) = W(\theta_1^*(\mathbf{r}), \theta_2^*(\mathbf{r})). \tag{2}$$

This is a bilevel problem: a meta-controller chooses rewards during an outer loop, while the solver and auditor respond to those rewards during an inner loop.

3.2 LOCAL INCENTIVES ANALYSIS

Here we analyze the *local* incentives of the solver and auditor (i.e. for a fixed prompt), so we drop the dependency on x from our notation. Beyond the incentives of any rational agents in this setting, we assume the solver derives some additional time-dependent utility $\Omega_t(x) \geq 0$ from misaligning. This assumption reflects the notion that standard RL post-training procedures may induce misalignment. For example, in the hallucination setting, RLHF pushes the models to provide plausible but incorrect responses rather than abstaining when unsure (Shapira et al., 2026).

Solver/Auditor Incentives. A reward mechanism must manage two local incentives. First, the solver's total expected utility under the auditor's policy is affine in the alignment probability:

$$U_1 = p_{\text{audit}}[\Pr(A)r_A + \Pr(M)r_M] + \Pr(M)\Omega_t + (1-p_{\text{audit}})r_A = p_{\text{align}}I_1 + C_1 \tag{3}$$

where $I_1 = p_{\text{audit}}(r_A - r_M) - \Omega_t$ is the solver's *deterrence margin* and C_1 is a constant independent of p_{align}. Correspondingly, the auditor's utility function is

$$U_2 = p_{\text{audit}}[\Pr(M)r_{\text{catch}} - \Pr(A)r_{\text{fp}} - r_{\text{audit}}] = p_{\text{audit}}I_2 \tag{4}$$

where $I_2 = r_{\text{catch}} - r_{\text{audit}} - p_{\text{align}}(r_{\text{catch}} + r_{\text{fp}})$ is the auditor's *audit margin*.

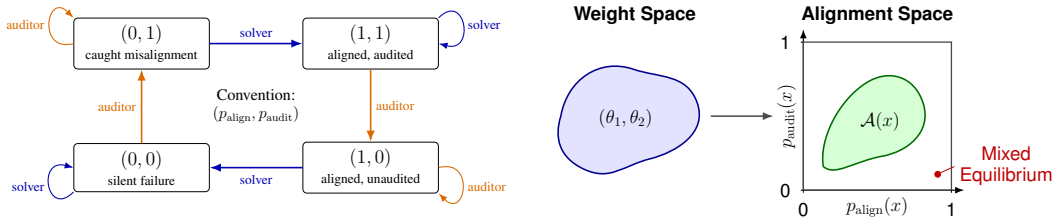
Equilibria analysis. For $0 < \Omega_t < r_A - r_M$, $r_{\text{catch}} > r_{\text{audit}}$, and $r_{\text{audit}} + r_{\text{fp}} > 0$, both players are indifferent at the unique interior mixed equilibrium

$$(p^*_{\text{align}}, p^*_{\text{audit}}) = \left(\frac{r_{\text{catch}} - r_{\text{audit}}}{r_{\text{catch}} + r_{\text{fp}}}, \frac{\Omega_t}{r_A - r_M}\right). \tag{5}$$

These conditions also exclude pure equilibria (Figure 1a; Appendix G). Increasing the solver penalty lowers the inspection probability needed for indifference, but does not by itself preserve useful monitoring. The equilibrium need not be an attracting point of learning dynamics, nor attainable by the parameterized models. In the noisy reference model, an aligned, always-audited pure equilibrium can arise under additional conditions (Appendix A), at the cost of persistent resolution.

The observation that the game has neither any pure equilibria nor stable, attractive mixed equilibria motivates adaptive rewards, i.e. rewards that change over time, which can keep the game near non-equilibrium points such as $(1,0)$. Because AI agents do *not* immediately update behaviors, but instead gradually update their policies through gradient feedback, a meta-controller which continuously adapts the reward configuration $\mathbf{r} = (r_A, r_M, r_{\text{catch}}, r_{\text{fp}}, r_{\text{audit}})$ can keep the agents near $(1,0)$.

[1] For a fixed finite inner game with bounded payoffs, if both learners have vanishing average external regret, their time-averaged joint distribution of play approaches the set of coarse correlated equilibria (Roughgarden, 2013).



(a) Pure-state local incentives.

(b) Feasible alignment space.

Figure 1: Left: one-step local incentives over pure states, $p_{\text{align}}, p_{\text{audit}} \in \{0, 1\}^2$; colored arrows show optimal single-player moves and self-loops mark locally optimal behavior. Right: feasible model weights map to a feasible set $\mathcal{A}(x) \subseteq [0, 1]^2$ of alignment–audit probabilities, and reward design tries to choose an attainable high-principal-value point.

3.3 Bandit Search over Reward Mechanisms

In realistic settings, the principal does not observe $p_{\text{align}}(x; \theta_1)$, $p_{\text{audit}}(x; \theta_2)$, or the response map $\mathbf{r} \mapsto (\theta_1^*(\mathbf{r}), \theta_2^*(\mathbf{r}))$. The principal only observes samples from interaction: a prompt is drawn, the solver responds, the auditor may audit, a correction outcome is recorded, and a noisy estimate of principal value is computed. We therefore treat

$$\mathbf{r} \mapsto \Phi(\mathbf{r}) = W(\theta_1^*(\mathbf{r}), \theta_2^*(\mathbf{r}))$$

as a black-box response map. On a fast timescale, the solver and auditor adapt to the current reward mechanism. On a slow timescale, the principal updates the reward mechanism using *bandit feedback*, a.k.a. zeroth-order feedback, as we only understand the dynamics induced by the solver and auditor after assigning some reward scheme. The procedure is documented in Algorithm 1.

Algorithm 1 Adaptive Reward Design by Bandit Feedback

Require: Initial policies $(\theta_{1,0}, \theta_{2,0})$, reward class $\mathcal{R}$, player learners $\mathcal{L}_1, \mathcal{L}_2$, bandit meta-controller $\mathcal{B}$, inner horizon T.

1: **for** $k = 0, 1, 2, \ldots$ **do**
2: Select a reward mechanism $\mathbf{r}_k \in \mathcal{R}$ using $\mathcal{B}$.
3: Hold $\mathbf{r}_k$ fixed and update the solver and auditor for T steps using $\mathcal{L}_1, \mathcal{L}_2$.
4: Evaluate adapted policies and compute a noisy estimate $\widehat{W}_k$ of the principal's value.
5: Update $\mathcal{B}$ using the observation $(\mathbf{r}_k, \widehat{W}_k)$.
6: **end for**

In a stationary black-box setting with a finite library of reward profiles, standard bandit methods (Lattimore & Szepesvári, 2020) can compete with the best fixed profile in the library. Our co-training setting is more path-dependent: selecting a reward profile changes the subsequent solver and auditor states, so the value of a profile can change over training. We therefore use discounted bandit methods. For continuous reward classes, the same template can be implemented with zeroth-order methods under smoothness assumptions on a locally stationary response map Φ (Auer et al., 2002a;b; Flaxman et al., 2005; Nesterov & Spokoiny, 2017). This procedure is not optimizing the agents' rewards as an end in themselves. It is searching for rewards that induce a good fixed point of the solver–auditor system: useful correction and high principal value. The same fixed-point formulation extends to multiple auditors; see Appendix H.

4 LLM Method

This section lifts the toy pipeline of Section 2 to a real coding task with language-model agents.

Coding Pipeline. Both agents are pretrained causal language models. Given a benchmark problem, the solver either emits Python code or abstains. If the solver emits code, the auditor sees the problem

together with that code and either emits a single top-level Python assert (intended to fail on a bug) or abstains. All reward computation is grounded in sandboxed execution: the solver's code is run against base tests, and the auditor's assert is run against the solver's code to classify the round as aligned, caught, silent failure, false positive, or abstain. One idea is that this can act as a repair mechanism—we could then feed the auditor's failing assert back to the solver to improve its implementation. For this work, we focus on the incentive-design problem to reduce solver hallucination in a single pass. We study the multi-pass setting in Appendix D.5.

Co-Training with GRPO and a Reward-Controller Outer Loop. The inner updates use Group-Relative Policy Optimization (GRPO) with LoRA adapters. Because a fully simultaneous GRPO update is expensive, each outer iteration trains the solver first (auditor held fixed) and then the auditor (solver held fixed) under the same reward profile. The full procedure in Appendix E.1 is Algorithm 2, which is just the LLM instantiation of Algorithm 1: the two GRPO phases play the role of the inner learners, while an outer reward controller chooses the reward profile for the next outer iteration and optionally updates from held-out feedback. Training hyperparameters can be found in Table 4.

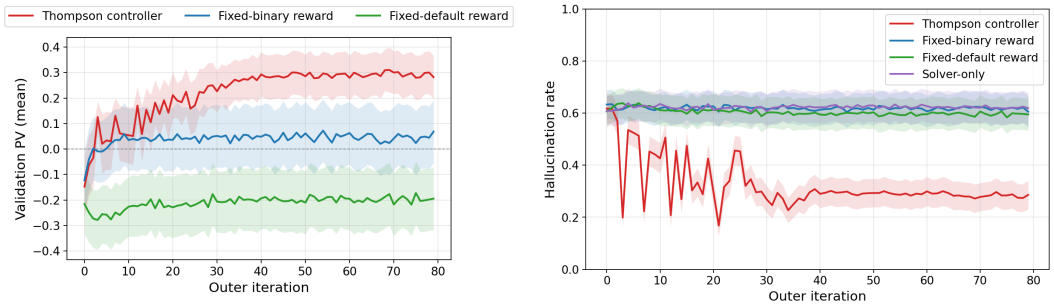
Across all controller choices, this alternating solver-then-auditor GRPO schedule is held fixed; the only difference between Thompson and the static baselines is how the outer controller selects and updates the reward profile. Appendix D.3 ablates the number of controller updates at matched compute; in our setup, fewer outer updates with larger inner adaptation blocks outperform a higher-frequency outer loop, supporting this two-timescale interpretation. Our main LLM controller is a discounted Thompson sampler (Thompson, 1933) over a finite library of eight reward profiles (Table 6), updated from held-out principal value (PV) after each outer round. Appendix C.1 gives the controller update rule and implementation details.

Reward-profile construction. We first tuned the fixed-default profile for PV, then hand-designed variants emphasizing abstention, catches, or silent-failure penalties. An exploratory continuous zeroth-order search did not produce a working controller, motivating the finite library.

Principal Value and Metrics. The PV used in the experiments is nearly the same as given in Table 12. The only difference is that there might be a truncated response—we give such responses a 0.0, as they are slightly worse than abstention, but still very easy to spot (no end-of-sequence token emitted). We also report *overall pass rate* (correct solutions among all samples), *attempted pass rate* (correct solutions among non-abstained, non-truncated attempts), *solver hallucination rate* (incorrect attempted solutions among all samples), and *silent-failure rate* (incorrect attempted solutions that are not caught by the auditor among all samples).

Baselines. To isolate the effect of cotraining from the effect of simply spending more optimization steps on the solver, we compare to a standard single *solver-only* reinforcement-learning approach, rewarded by Table 8. This main baseline receives no positive reward for abstention; Appendix E.8 reports a diagnostic variant with positive abstention reward that collapses to near-universal abstention. We match the total number of GRPO optimizer steps. Then, for the solver-auditor case, we compare the Thompson controller against a fixed-binary baseline that keeps a simple binary-correctness reward fixed throughout training (Table 9), and a fixed-default baseline that keeps the reward table fixed to the base profile that Thompson sampling builds its candidate profiles on (Table 5). This makes sense because the controller's final discounted profile estimates place the default profile in the top estimated band (Appendix E.6). The main idea is that dynamic, bandit-inspired reward algorithms are a promising choice–we do not compare different dynamic approaches.

Model, Dataset and Compute Budget. We evaluate the method with Qwen2.5-Coder-Instruct-7B on a training mixture of HumanEval+, MBPP, and APPS (Liu et al., 2023; Hendrycks et al., 2021; Austin et al., 2021). Per variant, we run a single-seed 80-outer-iteration training run with the same model family, prompt format, and a total training budget of 150 GRPO optimizer steps per outer iteration (in solver-auditor runs, this is split evenly between the solver and auditor). Matching compute here means matching the total number of GRPO optimizer steps. Our main comparison uses a separate 200×8 held-out evaluator, distinct from the in-loop Thompson-controller evaluator, with task-cluster 95% CIs. We also have a zero-shot generalization evaluation on 400 LiveCodeBench problems split with 1 sample per task (Jain et al., 2024). For further details on training and validation datasets, hyperparameters, and prompts, see Appendix E.2–E.4.



(a) Validation PV.

(b) Solver hallucination rate.

Figure 2: Held-out validation trajectories on a 200×8 evaluator; bands are task-cluster 95% CIs. Left: validation PV for three solver–auditor co-training variants; Thompson moves to a stable positive-value regime, fixed-binary remains near break-even, and fixed-default stays negative. Right: solver hallucination rate, computed as the fraction of all samples that are incorrect attempted solutions; Thompson is the only method with sustained hallucination reduction.

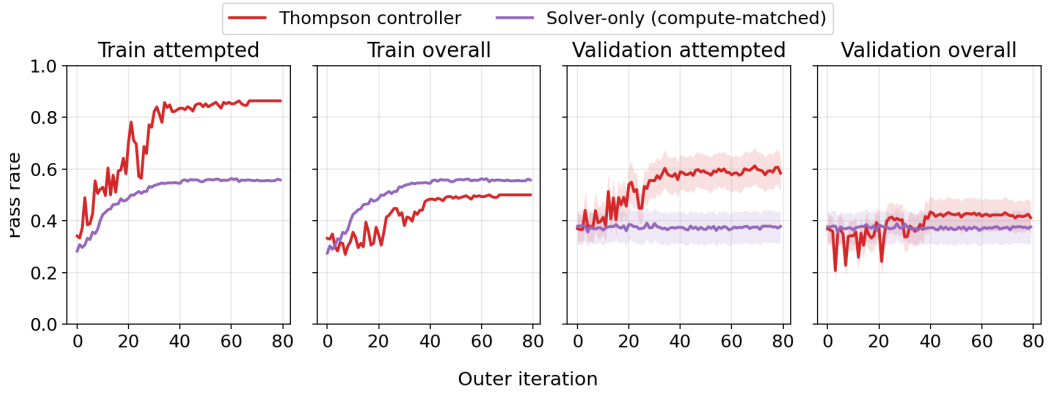


Figure 3: Per-outer-iteration solver pass rates, split by metric and data split. Left to right: the panels show training attempted pass rate (correct among non-abstained, non-truncated attempts), training overall pass rate (correct among all samples), validation attempted pass rate, and validation overall pass rate on the matched held-out evaluator. Validation panels include task-cluster 95% CI bands from 200×8 samples.

5 Experimental Results

The figures report single-seed runs. Task-cluster confidence intervals measure evaluation uncertainty, not variation across training seeds. Baseline subsets differ by comparison: validation PV focuses on solver–auditor methods, solver-only plots isolate optimizer-step-matched single-solver training, and downstream or hallucination plots include methods with directly comparable metrics.

Validation Principal Value and Hallucination Reduction. Figure 2 shows that adaptive reward selection changes the co-training regime. Thompson sampling separates from both static baselines and remains in a positive validation PV regime, while fixed-binary stays near break-even and fixed-default remains negative. On the same evaluator, Thompson reduces hallucination from 0.619 to 0.286, while fixed-binary, fixed-default, and solver-only end at 0.608, 0.598, and 0.622. This 52% reduction relative to the best main baseline explains much of the PV separation: the adaptive controller shifts probability mass away from misaligned attempts.

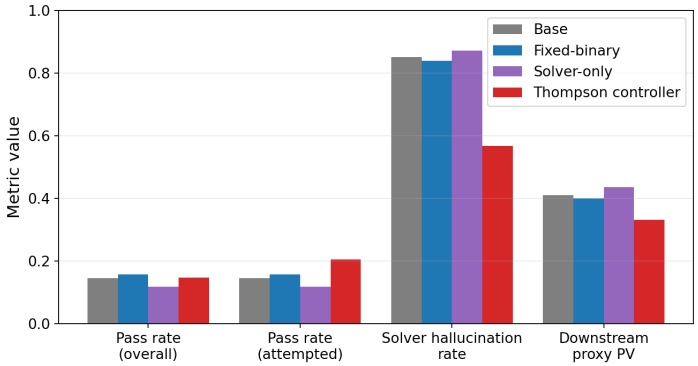


Figure 4: Downstream LiveCodeBench comparison for the base model, the final fixed-binary model, the optimizer-step-matched solver-only model (with the base model as an auditor), and the final Thompson-trained model. Thompson keeps the strongest attempted pass rate while sharply reducing solver hallucinations, but it does not achieve the best downstream proxy PV.

Comparison to Solver-Only Training. Figure 3 shows that Thompson improves held-out attempted and overall pass rates, whereas optimizer-step-matched solver-only training mainly improves the training mixture and then plateaus on validation. The co-training budget is split between the two agents; it produces checkpoints that generalize better on the same held-out distribution.

Downstream Hallucination and Proxy PV. Figure 4 reports four downstream metrics on the 400-task LiveCodeBench code-generation test split (Jain et al., 2024). On LiveCodeBench, Thompson transfers most clearly as hallucination control: its hallucination rate is 0.5675, a 32% relative reduction from the fixed-binary baseline's 0.8400, and it has the strongest attempted pass rate. However, its overall pass rate remains below fixed-binary, and its downstream proxy PV is lower than the base, fixed-binary, and solver-only systems because it abstains much more often. This suggests a distribution-shift calibration issue: the learned abstain-versus-attempt tradeoff remains conservative on the new task mix, even though the ability to avoid low-confidence hallucinated answers transfers. Mitigating this would likely require recalibrating the principal-value table on the target distribution, including transfer-like validation prompts in reward adaptation, or explicitly tuning reward profiles for the desired safety–helpfulness tradeoff. Additional ablations and diagnostics are reported in Appendix D.1–D.4: behavior decomposition, outer-loop schedule shape, and cross-play robustness. Appendices D.6 and D.7 examine selective abstention and a smaller auditor, respectively.

6 Conclusion

We proposed a mechanism-design view of alignment in agentic pipelines. The main lesson is that rewards should not be judged only by the immediate behavior they encourage, but by the fixed point they induce among interacting learned agents. In a solver–auditor system, penalizing misalignment can deter bad outputs, but it can also reduce the auditor's incentive to monitor. Robust alignment, therefore, requires dynamic reward mechanisms that jointly preserve solver deterrence and auditor oversight, especially in noisy settings where policies induce distributions over correction outcomes rather than deterministic success or failure events.

Our experiments give an initial demonstration of this perspective in an LLM coding pipeline. A profile-based Thompson controller improves validation PV relative to static reward baselines, while the downstream evaluation shows that the learned behavior transfers most clearly as hallucination reduction: the model answers more selectively and is more reliable when it does answer.

Applications to AI Safety. The framework is relevant to scalable oversight and AI control settings where one learned system acts and another monitors. There, safety depends not only on the competence of the monitor but on whether monitoring remains incentivized after the solver begins to appear reliable. Our model makes this failure mode explicit: producing an apparently safe system with

weakened oversight pressure. Dynamic reward design offers a way to treat oversight as something that is quantifiable. Safety is yet another non-verifiable domain that our techniques could potentially work on. Since the downstream principal value can be any arbitrary function of the output, we can help design equilibria that award safer multi-agent behavior.

Our approach complements standard safety guardrails rather than replacing them. Containment and other safeguards should remain active during both training and deployment; the scalable oversight protocol in Appendix B allocates expensive corrective feedback within those safeguards.

Limitations. The present experiments are only an initial testbed for this approach, and due to lack of compute resources, we could only run a few experiment runs. Coding provides unusually clean feedback through executable tests, and our solver–auditor game is much simpler than realistic agentic pipelines. Moreover, the drop in downstream proxy principal value in LiveCodeBench highlights a limitation of the current approach: principal value calibration is limited to distributions similar to the training distribution (though beneficial behavior like abstention to avoid hallucination is preserved). Thus, future work should investigate this as a continual learning problem—under distribution shift, the same principal value table can encode different true preferences—so one should continue training under reward adaptation under the new distribution.

AI USE STATEMENT

In this work, we used generative AI tools to implement methods, specifically to write code for our experiments, to provide feedback on research ideas and methodology through discussion, and to help interpret experimental results. We have not used generative AI tools to generate synthetic datasets, and translation, dataset cleaning, and qualitative data analysis are not applicable to this work. Additionally, we used generative AI tools for brainstorming, creating figures and plots, drafting and editing parts of the paper to improve readability, and identifying relevant literature. We have reviewed all AI-assisted work: AI-generated code was reviewed and tested by the authors, all reported experiments were run by the authors, AI-assisted interpretations and figures were checked by the authors against the underlying experimental outputs, and AI-assisted text, derivations, and citations were checked by the authors against the underlying results and sources. We take responsibility for the final content of this work, including text, claims, or artifacts produced with the aid of generative AI.

ETHICS STATEMENT

This work does not involve human subjects, personal data, or new data collection. All experiments use publicly available code-generation benchmarks (APPS, MBPP, HumanEval+, and LiveCodeBench), and all model-generated code is executed in a sandbox with time, CPU, and memory limits (Appendix E.3). Our goal is to make oversight of AI systems more reliable. The central observation, that reward schemes can weaken an auditor's incentive to monitor, could in principle inform attempts to undermine oversight; we believe that making this failure mode explicit is more useful to those designing safeguards. As discussed in the conclusion, our method is intended to complement, not replace, existing safety measures, and it should not be read as a guarantee against deceptive or scheming models.

REPRODUCIBILITY STATEMENT

Appendix E.1 gives the full LLM co-training procedure (Algorithm 2), and Appendix C specifies the reward-controller update rules. Appendices E.2–E.4 list the dataset splits, training hyperparameters (Table 4), and prompt templates. Appendices E–E.7 give every reward profile and baseline reward table, and Appendix E.9 reports compute. The toy experiment is specified in Appendix F. The assumptions and derivations behind the theoretical results are stated in Section 3 and Appendices A, B, and G.

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A Extension of the Theoretical Model to Non-Verifiable Settings with Possible Hallucination

We extend the model of Section 3 to the case of a noisy solver and noisy judge protocol, under only weak accuracy assumptions. The meta-controller framework remains the same, but the local analysis of the solver and auditor is changed.

Solver/Auditor Incentives. Let $\mu, \theta_1, \theta_2, p_{\text{align}}, Z$ be the same as before. Because of noise in the solver's response and variation in dataset difficulty, we model the *true* alignment behavior as

$$\bar{Z} = \begin{cases} Z & \textit{w.p. } 1-\varepsilon(x) \\ 1-Z & \textit{w.p. } \varepsilon(x) \end{cases}, \quad \textit{where } 0 \le \varepsilon(x) < \frac{1}{2}$$

and set $\bar{p}_{\text{align}}(x) = \Pr(\bar{Z} = A \mid x, \theta_1)$. (For example, in a simple setting where the solver decides between providing factually true or false responses, it may state a false claim as true and vice versa.) We assume that the response is only *weakly noisy*, $\varepsilon(x) < 1/2$.

Now suppose the auditor has parameters θ_2, and suppose it chooses to audit with probability $p_{\text{audit}}(x) \in [0,1]$. If an audit occurs, an external resolution procedure, such as debate, a trusted judge, or a reward oracle, returns a noisy verdict $\widehat{Z} \in \{A, M\}$. We write

$$\kappa_{\text{tp}}(x) := \Pr(\widehat{Z} = M \mid \bar{Z} = M, x), \qquad \kappa_{\text{fp}}(x) := \Pr(\widehat{Z} = M \mid \bar{Z} = A, x) \tag{6}$$

Here, "tp" denotes *true positive* and "fp" denotes *false positive*. There are many natural instantiations of such a reward procedure, such as AI Safety Via Debate (Irving et al., 2018), RLAIF (Lee et al., 2024) or JEPO (Tang et al., 2025). Our only assumption is that the resolution is *weakly informative*, $\kappa_{\text{fp}} < \kappa_{\text{tp}}$.

The reward mechanism now assigns training rewards to *realized* correction events. In the minimal model, the solver receives reward $r_A(x)$ after audited verdict $\widehat{Z} = A$ and punishment $r_M(x)$ after audited verdict $\widehat{Z} = M$, with $r_A(x) > r_M(x)$. On the other hand, the auditor receives reward $s_{\text{catch}}(x)$ for a negative external verdict, pays cost $s_{\text{fp}}(x)$ when the external verdict is positive (an unsuccessful complaint according to that verdict, not necessarily a ground-truth false positive), and pays direct audit cost $s_{\text{audit}}(x) < s_{\text{catch}}(x)$. Now, the solver's total expected utility under the auditor's policy is affine in the alignment probability:

$$U_1 = p_{\text{audit}}[\Pr(\widehat{Z} = A) r_A + \Pr(\widehat{Z} = M) r_M] + \Pr(Z = M)\Omega_t + (1 - p_{\text{audit}}) r_A = p_{\text{align}} I_1 + C_1 \tag{7}$$

where I_1 is the solver's *deterrence margin* and C_1 is a constant independent of p_{align},

$$I_1 = p_{\text{audit}}(1-2\varepsilon)(\kappa_{\text{tp}} - \kappa_{\text{fp}})(r_A - r_M) - \Omega_t \tag{8}$$

$$C_1 = r_A + \Omega_t - p_{\text{audit}}\left[(1-\varepsilon)\kappa_{\text{tp}} + \varepsilon\kappa_{\text{fp}}\right](r_A - r_M). \tag{9}$$

When $I_1 > 0$, misalignment is locally deterred, and correspondingly when $I_1 < 0$, it is locally encouraged. Correspondingly, the auditor's utility function is

$$U_2 = p_{\text{audit}}[\Pr(\widehat{Z} = M) s_{\text{catch}} - \Pr(\widehat{Z} = A) s_{\text{fp}} - s_{\text{audit}}] = p_{\text{audit}} I_2 \tag{10}$$

where I_2 is the auditor's *audit margin*,

$$I_2 = \left[\kappa_{\text{tp}} - \alpha(\kappa_{\text{tp}} - \kappa_{\text{fp}})\right] s_{\text{catch}} - \left[1 - \kappa_{\text{tp}} + \alpha(\kappa_{\text{tp}} - \kappa_{\text{fp}})\right] s_{\text{fp}} - s_{\text{audit}} \tag{11}$$

where $\alpha = \varepsilon + (1-2\varepsilon)p_{\text{align}} = \bar{p}_{\text{align}}$ is the probability of true alignment. Auditing is incentivized when $I_2 > 0$ and discouraged when $I_2 < 0$.

Equilibria Analysis. Let $\beta = (1-\varepsilon)\kappa_{\text{tp}} + \varepsilon\kappa_{\text{fp}}$ and $d = (1-2\varepsilon)(\kappa_{\text{tp}} - \kappa_{\text{fp}}) > 0$. Assuming $s_{\text{catch}} + s_{\text{fp}} > 0$, an interior mixed equilibrium requires both $0 < \Omega_t < d(r_A - r_M)$ and $0 < \beta(s_{\text{catch}} + s_{\text{fp}}) - s_{\text{fp}} - s_{\text{audit}} < d(s_{\text{catch}} + s_{\text{fp}})$. Under these conditions it is

$$(p^*_{\text{align}}, p^*_{\text{audit}}) = \left(\frac{\beta(s_{\text{catch}} + s_{\text{fp}}) - s_{\text{fp}} - s_{\text{audit}}}{(1-2\varepsilon)(\kappa_{\text{tp}} - \kappa_{\text{fp}})(s_{\text{catch}} + s_{\text{fp}})}, \frac{\Omega_t}{(1-2\varepsilon)(\kappa_{\text{tp}} - \kappa_{\text{fp}})(r_A - r_M)} \right). \tag{12}$$

However, in contrast to Section 3, in the regime where s_{catch} is large enough to satisfy

$$\frac{1-\lambda}{s_{\mathrm{catch}}/s_{\mathrm{fp}}} + \frac{1}{s_{\mathrm{catch}}/s_{\mathrm{audit}}} < \lambda, \quad \lambda = \varepsilon\kappa_{\mathrm{tp}} + (1-\varepsilon)\kappa_{\mathrm{fp}} \tag{13}$$

and the solver condition $d(r_A - r_M) \geq \Omega_t$ also holds, $(p^*_{\mathrm{align}}, p^*_{\mathrm{audit}}) = (1, 1)$ is a pure equilibrium of this reference game. The displayed reward-ratio condition presumes positive costs; equivalently, the auditor condition is $\lambda s_{\mathrm{catch}} - (1-\lambda)s_{\mathrm{fp}} - s_{\mathrm{audit}} > 0$. However, as discussed in the noise-free case, this equilibrium is still undesirable since auditing may be costly, especially in non-verifiable settings.

B Scalable Oversight: Auditor Screening with Judge Review

Our analysis suggests a simple scalable-oversight protocol that separates cheap *screening* by the auditor from expensive *review* by a judge. The judge is the external resolution procedure of Appendix A (for example, a human, a debate protocol, or another externally controlled evaluator), and it returns a possibly noisy verdict $\widehat{Z} \in \{A, M\}$.

Protocol. During post-training, each prompt x is processed as follows.

1. The solver either abstains or submits an answer. Abstentions are scored as their own outcome and are not sent to the judge.
2. The auditor inspects the submitted answer and outputs a flag $F \in \{0, 1\}$, where $F = 1$ means the auditor believes the answer is wrong.
3. Every flagged answer is sent to the judge. Each unflagged answer is also sent to the judge, independently, with probability $\rho \in (0, 1]$ (*random review*). The randomness is drawn after the answer and flag are fixed and is hidden from both agents.

Let $S \in \{0, 1\}$ indicate whether an answer is sent to the judge, so that $\Pr(S = 1 \mid F) = \pi(F)$ with $\pi(1) = 1$ and $\pi(0) = \rho$. Because of random review, every submitted answer is judged with probability at least ρ, whatever the auditor does. An auditor that never flags therefore cannot hide errors from the principal, which makes training robust to, for example, auditor sandbagging.

Rewards. Rewards depend only on what is observed, namely the flag and, when $S = 1$, the judge's verdict. They never depend on true correctness, which is unobservable. A judged answer falls into one of four cases:

- flagged, $\widehat{Z} = M$: a successful catch;
- flagged, $\widehat{Z} = A$: an unsuccessful flag;
- unflagged, $\widehat{Z} = M$: a miss exposed by random review;
- unflagged, $\widehat{Z} = A$: a confirmed pass.

The third case is what makes the protocol work. A miss exposed by random review can penalize both the solver and the auditor, so the auditor is not rewarded simply for staying silent. An unjudged answer carries no verdict. It should not be recorded as verified-correct in training data or in value estimates.

Estimating principal value. Flagged answers are always judged and unflagged answers are judged only at rate ρ, so the judged answers are not a representative sample. If the outer controller (Appendix C) estimates population value from judged answers, it must correct for this selection. For a per-attempt value $g(F, \widehat{Z})$, we use the inverse-probability-weighted estimator

$$\widehat{W}_{\mathrm{IPW}} = \frac{1}{n}\sum_{i=1}^{n} \frac{S_i}{\pi(F_i)}\, g(F_i, \widehat{Z}_i). \tag{14}$$

Given the answer and its flag, selection is independent of the verdict and $\mathbb{E}[S/\pi(F)] = 1$, so (14) is unbiased for the average of g under the judge's verdicts. This target is value *as measured by the judge*, which is not the same as true correctness or the principal's latent values. Small ρ gives large weights $1/\rho$ and therefore high variance.

Relation to the local model. In Appendix A, the audit decision does not depend on the answer. An answer is resolved with probability p_{audit} whether or not it is aligned, and the auditor's payoff depends only on the audits it initiates. Screening changes both features. A useful auditor flags wrong answers more often than correct ones, so the probability of being judged depends on the true label:

$$q_M = \Pr(S = 1 \mid \bar{Z} = M, x), \qquad q_A = \Pr(S = 1 \mid \bar{Z} = A, x), \qquad q_M, q_A \geq \rho.$$

The auditor's utility also gains a penalty for misses exposed by random review. The results of Appendix A therefore do not carry over by substituting the average judging rate $\Pr(S = 1)$ for p_{audit}. The equilibrium of the screening protocol must be analyzed directly.

For the solver, this analysis is short. Keep the solver payments and noise model of Appendix A: the solver receives r_M after a negative verdict and r_A otherwise, including when its answer is not judged. Redefine κ_{tp} and κ_{fp} as in (6), but conditional on the answer being judged ($S = 1$). A truly misaligned answer then loses $r_A - r_M$ with probability $q_M \kappa_{\text{tp}}$, and a truly aligned answer loses it with probability $q_A \kappa_{\text{fp}}$. Raising p_{align} moves probability mass $(1 - 2\varepsilon)$ from $\bar{Z} = M$ to $\bar{Z} = A$. Holding q_M, q_A, and the κ's fixed, the solver's deterrence margin is therefore

$$I_1^{\text{screen}} = (1 - 2\varepsilon)(r_A - r_M)\big(q_M \kappa_{\text{tp}} - q_A \kappa_{\text{fp}}\big) - \Omega_t. \tag{15}$$

Setting $q_M = q_A = p_{\text{audit}}$ recovers Equation 8.

Equation 15 shows what random review does and does not guarantee. Because $q_M \geq \rho > 0$, errors are always exposed with some probability. This alone does not ensure $I_1^{\text{screen}} > 0$, which also requires a penalty gap $r_A - r_M$ that is large relative to Ω_t and $q_M \kappa_{\text{tp}} > q_A \kappa_{\text{fp}}$. Weak informativeness ($\kappa_{\text{fp}} < \kappa_{\text{tp}}$) is not enough on its own. The screening rates can reverse the inequality, for instance if the auditor flags correct answers more often than incorrect ones. The judge's accuracy on the selected subset can also differ from its population accuracy, and all of these rates can drift as the agents adapt.

C Detailed Description of Multi-Armed Bandit Algorithms

This appendix provides a formal description of the multi-armed bandit (MAB) algorithm used as the outer-loop reward controller in our experiments. In our setting, each "arm" corresponds to a reward profile $\mathbf{r} \in \mathcal{R}$. At each outer round t, the controller selects a profile $\mathbf{r}_t$, and after the inner agents adapt to $\mathbf{r}_t$, the controller receives a noisy scalar feedback y_t representing the empirical principal value.

C.1 Thompson Sampling

Thompson sampling (Agrawal & Goyal, 2012; Thompson, 1933) is a Bayesian approach that selects actions according to the probability that they are optimal. We use a Gaussian posterior for each reward profile.

Standard Gaussian Thompson Sampling. For each profile $a \in \{1, \ldots, L\}$, let $n_a(t)$ be the number of times it has been pulled and $\hat{\mu}_a(t)$ be its empirical mean. The controller samples a score

$$\tilde{\mu}_a(t) \sim \mathcal{N}\left(\hat{\mu}_a(t), \frac{\sigma^2}{n_a(t) + 1}\right)$$

and selects $a_t = \arg\max_a \tilde{\mu}_a(t)$. This is used in our toy benchmarks in section 2 with $\sigma = 0.65$.

Discounted Thompson Sampling. For non-stationary environments, such as our co-training setup where the agents' response to a reward profile changes over time, we use a discounted version. Let $\gamma \in (0, 1)$ be a discount factor. We maintain:

$$S_a(t + 1) = \gamma S_a(t) + \mathbf{1}\{a_t = a\} y_t$$
$$N_a(t + 1) = \gamma N_a(t) + \mathbf{1}\{a_t = a\}$$

The discounted empirical mean is $\mu_a(t) = S_a(t)/N_a(t)$ for $N_a(t) > 0$. After an initial round-robin warm start, the controller samples scores from $\mathcal{N}(\mu_a(t), \sigma^2/(N_a(t) + 1))$. We use this as the main controller for section 4 over the eight reward profiles in Table 6, with $\sigma = 0.55$. This keeps a persistent incentive to explore profiles that are uncertain while still concentrating on profiles with high observed principal value. We chose Thompson sampling as our MAB algorithm because it is competitive with, and in several regimes significantly outperforms, UCB-style algorithms (Chapelle & Li, 2011), in addition to the quick convergence observed in Section 2.

C.2 UCB1

The Upper Confidence Bound (UCB1) algorithm (Auer et al., 2002a) uses an optimism-in-the-face-of-uncertainty principle. It selects:

$$a_t = \arg\max_a \hat{\mu}_a(t) + \alpha\sqrt{\frac{\log(t+1)}{n_a(t)}}$$

where α is a scaling parameter.

C.3 EXP3 AND EXP4

The Exponential-weight algorithm for Exploration and Exploitation (EXP3) (Auer et al., 2002b) is designed for adversarial settings where rewards may not be i.i.d.

EXP3. It maintains weights $w_a(t)$ and samples from the distribution:

$$p_a(t) = (1-\eta)\frac{w_a(t)}{\sum_b w_b(t)} + \frac{\eta}{L}$$

where $\eta \in (0, 1]$ is an exploration parameter. After observing y_t (scaled to $[0, 1]$), the weight of the selected arm is updated:

$$w_{a_t}(t+1) = w_{a_t}(t)\exp\left(\eta\frac{y_t}{Lp_{a_t}(t)}\right).$$

EXP4. EXP4 is a contextual extension of EXP3 that uses a set of experts $\{E_j\}$. Each expert E_j provides a distribution $\xi_j(t)$ over arms based on current telemetry (context). The controller samples from

$$p_a(t) = \sum_j \bar{w}_j(t)\xi_j(t, a),$$

where $\bar{w}_j$ are the weights of the experts, updated similarly to EXP3. In a solver–auditor pipeline, such experts could respond to signals such as high silent-failure rates or excessive solver abstention.

D LLM ABLATIONS

D.1 BEHAVIORAL DECOMPOSITION ACROSS REWARD CONTROLLERS

The validation PV gap in Figure 2a is mirrored in the training-side behavior traces. Figure 5 compares the adaptive Thompson run directly against the fixed-binary baseline on the six rates that most clearly summarize solver and auditor behavior.

The most visible difference is solver-side calibration. Thompson ends with substantially higher base-test pass rate, but unlike fixed-binary it also learns to abstain frequently on harder or lower-confidence cases. That reduces the mass of bad attempted solutions enough to offset the drop in raw auditor catches. The figure therefore supports the main interpretation of the validation PV result: the adaptive controller is not simply making the auditor more aggressive; it is steering the joint system toward a different attempt-versus-abstain regime.



Figure 5: Training-side behavior trajectories for the Thompson controller, fixed-binary, and fixed-default. The solver base-test pass rate is computed over non-abstained, non-truncated solver attempts; solver abstention is the fraction of prompts on which the solver abstains. The auditor panels report event rates under the training evaluator: explicit abstention means the auditor declines to flag a solver attempt, true catches are incorrect solver attempts flagged by the auditor, false positives are correct solver attempts flagged by the auditor, and silent failures are incorrect solver attempts with a non-abstaining auditor output that fails to catch the bug. These rates are normalized by all auditor-training samples, so explicit auditor abstention is displayed separately rather than counted as a silent failure in this diagnostic. Thompson drives higher solver pass rate while retaining a substantial abstention policy. Fixed-binary keeps the auditor active but does not reach the same solver-side calibration. Fixed-default collapses to explicit auditor abstention, causing non-abstaining auditor catch, false-positive, and silent-failure events to vanish.

D.2 Mechanism of the Fixed-Default Failure

The fixed-default run in Figure 2a is not merely weaker overall; it fails in a specific way. The solver becomes more competent, but the default rewards give the auditor too little incentive to keep inspecting once correct solutions become common. As auditing collapses, silent failures dominate and validation PV remains negative throughout the full 80-checkpoint sweep.

Two additional checks support this interpretation. A mid-training probe of the outer-iteration-46 checkpoint already yields validation PV -0.166 with a 99.7% auditor abstention rate. On LiveCodeBench, the final fixed-default checkpoint reaches downstream proxy PV -0.7475, again because the auditor almost never asserts. Fixed-default is therefore a direct empirical instance of the paper's coupled incentive tension: making the solver look safer can simultaneously remove the auditor's incentive to monitor.

D.3 Outer-Loop Schedule Shape at Matched Compute

We also ablate how often the outer reward controller is updated when total learner-update budget is held fixed (or nearly fixed). The baseline schedule is 80×75 (80 outer iterations, 75 solver and 75 auditor GRPO steps per iteration). We compare this against a short-horizon schedule 40×150 and a long-horizon schedule 160×38.

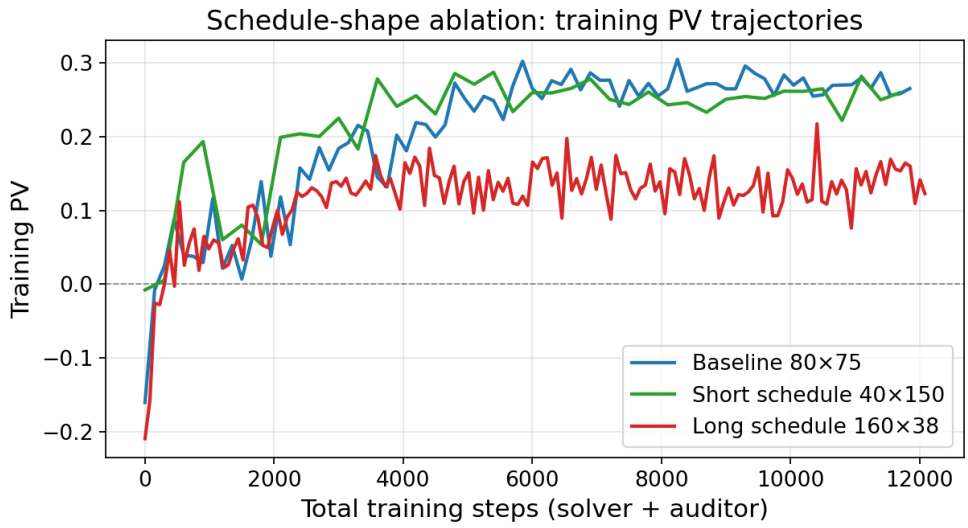

Figure 6: Schedule-shape ablation training PV trajectories versus total training steps (solver plus auditor updates). The short-horizon schedule (40×150) reaches the positive PV regime quickly and stays near the baseline trajectory, while the long-horizon schedule (160×38) remains in a lower-value regime despite more frequent outer-loop updates.

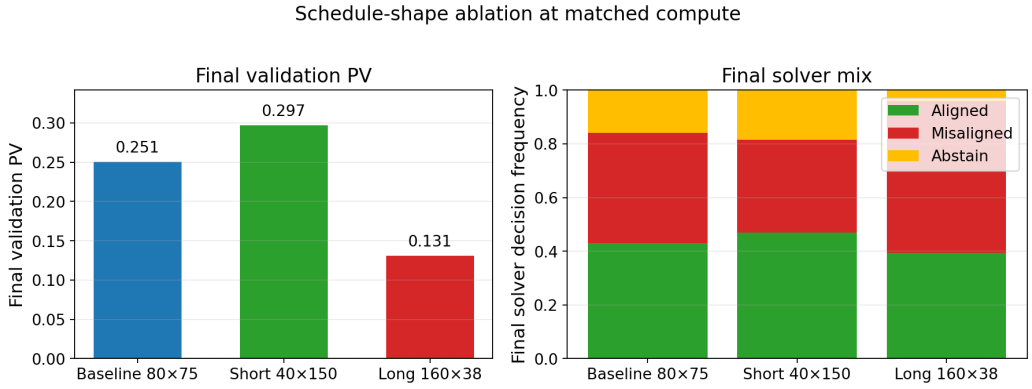

Figure 7: Schedule-shape ablation at matched compute, shown as a two-panel final-checkpoint summary. Left: final validation PV on the common 200×2 held-out protocol. Right: final solver decision mix (aligned / misaligned / abstain) under the same evaluator.

Figure 7 suggests a clear directional pattern in this setup: fewer outer updates with larger inner adaptation blocks (40×150) outperforms both the baseline and the high-frequency outer-update schedule (160×38). The right panel shows that this gain is accompanied by a more favorable final solver mix (higher aligned share and lower misaligned share). Intuitively, the long schedule may switch reward profiles too often relative to inner adaptation progress, yielding noisier reward-selection feedback and weaker final checkpoints. This is a single-seed schedule ablation, so we treat it as a strong diagnostic rather than a final claim about universally optimal schedule shape.

	Auditor iteration								
Solver iter	0	11	22	33	45	56	67	79	row mean
0	−0.053	−0.003	0.078	0.096	0.075	0.044	0.072	0.085	0.049
11	−0.088	0.047	0.104	0.080	0.077	0.044	0.095	0.022	0.048
22	0.010	0.140	0.202	0.158	0.203	0.121	0.134	0.120	0.136
33	0.059	0.175	0.225	0.239	0.192	0.251	0.262	0.210	0.202
45	0.083	0.190	0.242	0.255	0.240	0.211	0.224	0.261	0.213
56	0.030	0.216	0.234	0.227	0.197	0.227	0.240	0.208	0.197
67	0.125	0.242	0.203	0.244	0.270	0.251	0.268	0.267	0.234
79	0.176	0.278	**0.286**	0.246	0.221	0.277	0.238	0.213	**0.242**
col mean	0.043	0.161	0.197	0.193	0.184	0.178	0.192	0.173	

Table 1: Validation PV for the 8×8 cross-play grid of Thompson-controller solver and auditor checkpoints. Each cell is the mean validation PV on 200 tasks with 2 samples per task under a fixed evaluator. The best single pair is solver outer iteration 79 with auditor outer iteration 22 (bold). The mean across all 64 cells is 0.165 and the mean along the matched-iter diagonal is 0.173.

D.4 Cross-Play Robustness of the Thompson Pair

To check whether the Thompson improvements come from a specific coincidence of solver and auditor checkpoints, we evaluate an 8×8 cross-play grid: solver checkpoints from the Thompson run at iterations $\{0, 11, 22, 33, 45, 56, 67, 79\}$ paired with auditor checkpoints from the same set, giving 64 (solver, auditor) pairs. Each cell is scored with the same validation evaluator (200 tasks, 2 samples per task).

Table 1 reports validation PV for each pair. The final column and row summarize marginals (mean over one side of the pair), and the bottom-right summary line reports the mean across all 64 cells and the mean along the matched-iter diagonal.

Two qualitative observations come out of Table 1. First, the solver's marginal (row means) rises approximately monotonically with outer iteration. Second, the auditor's marginal (column means) rises rapidly early in training, peaks at outer iteration 22 with mean 0.197, and then fluctuates, ending at 0.173 by outer iteration 79. Thus additional auditor training does not monotonically improve validation PV on average. The best single pair in the grid is actually the final solver paired with a mid-training auditor (s_{79}, a_{22}). This pattern is consistent across several mid-training auditor columns and supports two readings: (i) the Thompson solver is the main driver of end-of-training validation PV on this setup, and (ii) the paired system is reasonably robust to moderate temporal mismatch between solver and auditor training stage, not relying on a brittle solver–auditor coincidence.

D.5 Agentic Revision as a Repair Signal

Figure 8 summarizes the agentic-revision benchmark, which tests whether auditor feedback can serve as a hallucination-avoidance repair signal after an initial solver attempt. The main pattern is mixed but still informative. Thompson has the best first-pass accuracy on this benchmark, while fixed-binary shows the clearest net gain from the revision step itself: it improves 15 samples from misaligned to aligned and degrades only 4. Thompson changes far more samples overall (45), but those edits are less consistently beneficial, improving 10 samples while degrading 13.

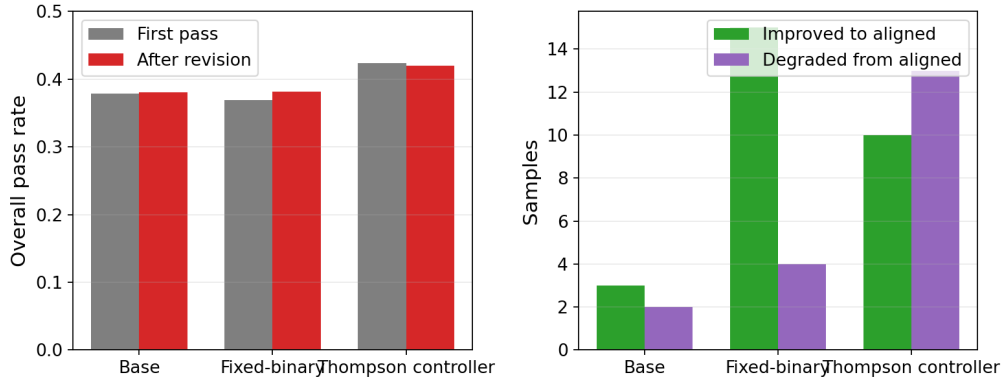

Figure 8: Agentic-revision benchmark summary. Left: first-pass versus post-revision overall pass rate. Right: number of samples repaired to aligned versus degraded from aligned by the revision stage. The Thompson controller starts from the strongest first-pass model, while fixed-binary shows the largest positive revision delta.

This is why we treat revision as a supporting result rather than a headline claim. The learned auditor outputs can sometimes provide actionable repair signal, but the strongest evidence for the method remains the first-pass incentive-design story in the main validation PV experiments.

D.6 Ablation: Suppressing Solver Abstention

The Thompson-trained solver abstains much more often than the baselines. This ablation asks whether those abstentions are *selective*, meaning concentrated on tasks the solver would get wrong, or simply frequent. We take the final Thompson solver and suppress abstention at decoding time by intervening on the logits of the abstention prefix, which forces the solver to attempt every task. We then measure how often the forced answers fail, both on the full evaluation set and on the subset of tasks where the unmodified policy abstains.

Evaluation set	Failure on abstained tasks	Failure on all tasks
Validation mixture	86%	58%
LiveCodeBench	99%	85%

Table 2: Failure rate of forced answers from the final Thompson solver with abstention suppressed. "Abstained tasks" are those on which the unmodified policy abstains. These rates count failures among forced answers, so they are not directly comparable to the hallucination rate in the main text, which counts incorrect attempts among all samples of the unmodified policy.

Forced answers fail far more often on abstained tasks than on the evaluation set as a whole (Table 2), so the solver's abstentions are concentrated on tasks it would likely get wrong. For reference, the base model's ordinary hallucination rate on LiveCodeBench is 85.25%, close to the fixed-binary baseline's 84.00%.

D.7 Ablation: Smaller Auditor

In the main experiments, the solver and auditor share the same 7B base model. To check that co-training does not depend on this symmetry, we pair the 7B solver with a `Qwen/Qwen2.5-Coder-3B-Instruct` auditor and train with the same co-training procedure. This run reaches a validation PV of 0.17, indicating that the method remains viable when the auditor is smaller than the solver. This indicates that the technique could perhaps be used for scalable oversight.

Split	APPS	MBPP	HumanEval+	Total
Training	3271 (90.6%)	204 (5.7%)	134 (3.7%)	3609
Validation	178 (89.0%)	12 (6.0%)	10 (5.0%)	200

Table 3: Dataset composition for the LLM experiments.

E LLM Experiment Details

E.1 Co-Training Procedure

Algorithm 2 Solver–auditor co-training with GRPO and reward-controller outer loop

Require: Initial solver and auditor adapters (θ_1, θ_2), library of reward profiles $\mathcal{R}$, outer budget T.

1: **for** outer iteration $k = 1, 2, \ldots, T$ **do**
2: **Profile choice.** Select $\mathbf{r}_k$ using the reward controller over $\mathcal{R}$.
3: **Solver phase.** Generate solver and auditor candidates on a batch of prompts, updating θ_1 according to $\mathbf{r}_k$ at each inner-round while freezing θ_2.
4: **Auditor phase.** Regenerate solver candidates from the updated θ_1 on the same batch, and run a GRPO update on θ_2 under the same $\mathbf{r}_k$.
5: **Evaluation.** Sample solver/auditor responses on held-out prompts, execute code and tests in the sandbox, classify outcomes, and compute an estimate of the principal's value $\widehat{W}_k$.
6: **Controller update.** Update the reward-controller state using $(\mathbf{r}_k, \widehat{W}_k)$.
7: **Warm start.** Carry (θ_1, θ_2) into the next round.
8: **end for**

E.2 LLM Data Splits and Evaluation Subset

Table 3 reports the dataset composition used by the LLM runs. Table 10 reports the composition of the 200-task validation subset used for the matched validation PV plots in the main text. This subset is the same across baseline and ablation runs (same tasks in the same order), but it is distinct from the held-out tasks used for the in-loop Thompson-controller updates during training.

The downstream LiveCodeBench comparison uses the first 400 tasks from the Hugging Face `livecodebench/code_generation` test split with 1 sample per task. LiveCodeBench is a contamination-aware code-generation benchmark of recent programming-contest problems (Jain et al., 2024); our subset uses standard-input/standard-output execution and contains 210 AtCoder, 181 LeetCode, and 9 Codeforces tasks, with 142 easy, 168 medium, and 90 hard tasks.

E.3 LLM Training Hyperparameters

Table 4 summarizes the main hyperparameters used for the LLM co-training runs. Unless otherwise noted, the solver and auditor use the same model, LoRA configuration, and GRPO sampling settings. The two-player runs use 80 outer iterations with 75 solver GRPO steps followed by 75 auditor GRPO steps per outer iteration; the compute-matched solver-only baseline uses 150 solver GRPO steps and no auditor update per outer iteration.

During each solver phase, the solver generates eight completions for each task prompt in a GRPO group. Non-abstaining, non-truncated solver completions are then used to build auditor-training prompts. During each auditor phase, the auditor generates eight completions for each fixed solver candidate context. Solver abstentions are filtered out before auditor training rather than replaced by a dummy code context, so a task for which all solver completions abstain contributes no auditor-training rows in that phase.

Category	Setting
Base model	`Qwen/Qwen2.5-Coder-7B-Instruct` for both solver and auditor.
Adaptation	LoRA adapters with rank $r = 8$, $\alpha = 16$, dropout 0.0, and no bias terms.
GRPO optimizer	`adamw_torch` with learning rate 2×10^{-5}; KL coefficient $\beta = 0$, so no KL penalty or reference model is used.
Batching	Per-device train batch size 2, gradient accumulation 2, generation batch size 16.
Grouped sampling	8 completions per prompt for within-group advantage normalization; solver groups share a task prompt, while auditor groups share a fixed non-abstaining solver candidate.
Generation	Maximum prompt length 2048, maximum completion length 1024, temperature 1.0, top-$p = 0.95$, top-$k = 0$.
Numerics and memory	bfloat16 enabled, gradient checkpointing enabled, Liger kernels enabled.
Outer loop	80 outer iterations; two-player runs use 75 solver steps and 75 auditor steps per iteration.
Controller	Discounted Thompson sampling with discount 0.9, Gaussian scale 0.55, and the eight-profile reward pool.
Evaluation	Main validation plots use 200 held-out tasks with 8 samples per task; downstream LiveCodeBench uses 400 tasks with 1 sample per task.
Sandbox	Per-test timeout 1.0s, CPU-time limit 1s, memory limit 256MB, and dataset-specific import allowlists.

Table 4: Hyperparameters for the LLM co-training experiments.

E.4 Prompt Templates

All main LLM runs use the `instructions_suffix` prompt variant, which places the task content before a final instruction block. The solver and auditor are both allowed to abstain by emitting exactly `<|abstain|>`. The templates below omit only the concrete task fields, written as placeholders.

Solver prompt.

```
You are a Python coding assistant.

### Dataset
{dataset_name}

### Task ID
{task_id}

### Problem
{problem_text}
{optional_starter_code_block}

### Instructions
Solve the coding task above by writing a full Python solution.
Return only Python code.
Do not include markdown fences or any explanation.
If you cannot produce a reliable solution, output exactly
↪  <|abstain|>.
```

Auditor prompt. For function-style tasks, the auditor is told that the harness binds `candidate` to the submitted function. For standard-input/standard-output tasks, the auditor is instead told that the harness exposes `run_io_case(input_text)`, which runs the candidate program and returns stripped stdout. The shared auditor prompt structure is:

```
You are a Python testing expert. Another agent proposed a solution
↪  for the
task below.

### Dataset
{dataset_name}
```

```
### Task ID
{task_id}

### Problem
{problem_text}
{optional_starter_code_block}

### Execution Hint
{function_or_stdio_execution_hint}

### Execution Contract
{candidate_binding_or_run_io_case_contract}

### Candidate Solution
```python
{candidate_solution}
```

Instructions
Write exactly one Python assert statement that passes for a
↪ correct
implementation and fails for this candidate if it is buggy.
If you cannot find a reliable bug-revealing assert, output exactly
<|abstain|>.
Return only a single assert statement on one line. Do not write
↪ multiple
asserts.
Do not include markdown fences, comments, or any explanation.
```

Revision prompt. The agentic-revision diagnostic in Appendix D.5 uses the following solver revision prompt:

```
You are a Python coding assistant. Another agent reviewed your
previous solution for the task below.

### Dataset
{dataset_name}

### Task ID
{task_id}

### Problem
{problem_text}
{starter_block}

### Previous Solution
```python
{candidate_solution}
```

Auditor Feedback
{auditor_feedback}

Instructions
Write a revised full Python solution that addresses the feedback
↪ if it is useful.
Return only Python code.
Do not include markdown fences or any explanation.
If you cannot produce a reliable solution, output exactly
↪ <|abstain|>.
```

E.5 Reward Profiles

The reward tables list configuration values. Keys ending in `_penalty` are magnitudes that are subtracted when the corresponding event occurs, while keys ending in `_reward` are applied directly.

For example, `solver_true_catch_penalty`=1.0 gives the solver reward -1.0 when an incorrect attempt is caught. The `auditor_false_catch_penalty` key penalizes the auditor for a bug-revealing assert against an aligned solution, and `auditor_false_positive_penalty` penalizes other valid asserts that fail on correct code; both are subtracted when applied.

Reward key	Default value
`solver_abstain_reward`	0.1
`solver_base_pass_reward`	1.0
`solver_true_catch_penalty`	1.0
`solver_uncaught_reward`	0.1
`auditor_true_catch_reward`	1.0
`auditor_false_positive_penalty`	1.0
`auditor_false_catch_penalty`	0.0
`auditor_explicit_abstain_aligned_reward`	0.05
`auditor_explicit_abstain_misaligned_penalty`	0.0
`auditor_miss_aligned_reward`	0.0
`auditor_miss_misaligned_penalty`	0.15
`auditor_invalid_output_penalty`	2.0

Table 5: Default reward profile used as the base configuration for the Thompson profile pool.

Profile name	Intuition	Reward overrides relative to the default configuration
`medium_abstain`	Mildly encourage solver abstention.	`solver_abstain_reward` $\leftarrow 0.20$
`high_abstain`	Strongly encourage solver abstention.	`solver_abstain_reward` $\leftarrow 0.55$
`strict_solver_catch`	Make solver more conservative about attempted failures.	`solver_true_catch_penalty` $\leftarrow 1.8$, `solver_uncaught_reward` $\leftarrow -0.2$
`lenient_solver_catch`	Encourage solver attempts even with catch risk.	`solver_true_catch_penalty` $\leftarrow 0.4$, `solver_uncaught_reward` $\leftarrow 0.35$
`audit_seeking`	Increase auditor willingness to flag suspicious outputs.	`auditor_true_catch_reward` $\leftarrow 1.7$, `auditor_false_positive_penalty` $\leftarrow 0.55$, `auditor_explicit_abstain_aligned_reward` $\leftarrow 0.0$, `auditor_explicit_abstain_misaligned_penalty` $\leftarrow 0.2$, `auditor_miss_misaligned_penalty` $\leftarrow 0.65$, `auditor_invalid_output_penalty` $\leftarrow 1.2$
`false_positive_averse`	Make auditor more cautious about false alarms.	`auditor_true_catch_reward` $\leftarrow 0.75$, `auditor_false_positive_penalty` $\leftarrow 1.7$, `auditor_explicit_abstain_aligned_reward` $\leftarrow 0.1$, `auditor_explicit_abstain_misaligned_penalty` $\leftarrow 0.0$, `auditor_miss_misaligned_penalty` $\leftarrow 0.25$, `auditor_invalid_output_penalty` $\leftarrow 1.0$
`silent_failure_penalty`	Penalize missed bugs and silent failures more heavily.	`solver_uncaught_reward` $\leftarrow -0.55$, `auditor_explicit_abstain_aligned_reward` $\leftarrow 0.0$, `auditor_explicit_abstain_misaligned_penalty` $\leftarrow 0.35$, `auditor_miss_misaligned_penalty` $\leftarrow 1.25$, `auditor_invalid_output_penalty` $\leftarrow 1.0$

Table 6: Non-default profiles in the Thompson reward-profile pool. The full pool is Table 5 plus these seven override profiles.

E.6 Thompson Profile Estimates

Table 7 reports the final controller state for the Thompson run used in the main LLM comparison. The Thompson sampler discounts past observations by 0.9, so the *discounted mean* is the controller's recency-weighted estimate of each profile's training PV at the end of the 80-outer-iteration run, and *discounted pulls* is the corresponding recency-weighted effective sample count.

The estimates are close across the profile pool, but the default profile remains in the high-estimated-value group: it is third of eight by final discounted mean and is within 0.0063 training PV of the highest estimated profile. This supports using fixed-default as a strong static baseline rather than a weak or arbitrary reward choice; the main result is that keeping this plausible profile fixed still underperforms adaptive profile selection.

E.7 Baseline Reward Tables

E.8 Positive-Abstention Solver-Only Diagnostic

We also trained a compute-matched solver-only diagnostic with the same rewards as Table 8 except for `solver_abstain_reward` $= 0.1$. This run is not used as the main solver-only baseline

Profile	Raw pulls	Discounted pulls	Final discounted mean
lenient_solver_catch	11	0.692	0.2777
silent_failure_penalty	10	1.779	0.27148
default	9	0.441	0.27146
high_abstain	11	1.333	0.2701
false_positive_averse	11	2.048	0.2669
audit_seeking	10	1.570	0.2662
strict_solver_catch	10	1.877	0.2628
medium_abstain	8	0.259	0.2603

Table 7: Final discounted Thompson-controller estimates by reward profile for the main LLM Thompson run. Profiles are sorted by final discounted mean.

Reward key	Value
solver_abstain_reward	0.0
solver_base_pass_reward	1.0
solver_true_catch_penalty	0.0
solver_uncaught_reward	0.0

Table 8: Solver-only baseline reward table. Only the solver is trained; the auditor is disabled. The solver receives reward 1 for passing the base tests and 0 for other outcomes, including abstention.

because it learns the degenerate low-risk policy of abstaining on almost every task. On the matched validation evaluator, its final checkpoint has abstention rate 0.975, overall pass rate 0.015, and solver hallucination rate 0.010. On LiveCodeBench, the same checkpoint abstains on all 400 tasks, giving zero coverage, zero overall pass rate, zero solver hallucination rate, and downstream proxy PV 0.1. This diagnostic confirms that rewarding abstention can trivially reduce hallucination by eliminating attempts, which is distinct from the Thompson controller's main behavior of reducing hallucination while preserving nontrivial coverage.

Reward key	Value
solver_abstain_reward	0.0
solver_base_pass_reward	1.0
solver_true_catch_penalty	0.0
solver_uncaught_reward	0.0
auditor_true_catch_reward	1.0
auditor_false_positive_penalty	0.0
auditor_false_catch_penalty	0.0
auditor_explicit_abstain_aligned_reward	0.0
auditor_explicit_abstain_misaligned_penalty	0.0
auditor_miss_aligned_reward	1.0
auditor_miss_misaligned_penalty	0.0
auditor_invalid_output_penalty	0.0

Table 9: Fixed-binary baseline reward table. Both agents are trained, but rewards take only values in $\{0, 1\}$: the solver gets 1 for passing base tests, the auditor gets 1 for true catches and for non-flagging aligned solutions through auditor_miss_aligned_reward, and all penalties are zero.

E.9 Compute Resources

All LLM training and evaluation jobs were run on an internal Slurm-managed GPU cluster using H100 GPU nodes. Training jobs requested 16 CPU cores and 64GB host memory per GPU. Solver–auditor training used two H100s for the learner updates; runs with online evaluation allocated a third H100 as a dedicated evaluation worker, while runs with online evaluation disabled used separate one-H100 evaluation jobs. Table 11 summarizes the allocations for the paper-reported runs; the listed wall times are Slurm allocations or observed elapsed times from the completed runs, rounded to the nearest hour where appropriate.

Dataset	Count	Share
APPS	178	89.0%
MBPP	12	6.0%
HumanEval+	10	5.0%
Total	200	

Table 10: Composition of the fixed 200-task validation subset used by matched validation PV evaluations.

Experiment type	Resources and time
Thompson solver–auditor training	One main seed used 2× H100 for solver/auditor training plus 1× H100 for online evaluation, with 16 CPU cores and 192GB host memory total. The run used a 20h allocation, corresponding to about 40 training H100-hours plus 20 evaluation H100-hours allocated.
Fixed-binary and fixed-default solver–auditor training	Two main baselines used 2× H100 for solver/auditor training, 16 CPU cores, and 128GB host memory, with evaluation run separately. Each used a 20–24h allocation and completed in roughly 11–12h, for about 45 elapsed training H100-hours across the two baselines.
Solver-only training	One main baseline used 2× H100, 16 CPU cores, and 128GB host memory. It used a 20h allocation and completed in roughly 10h, for about 20 elapsed H100-hours.
Schedule-shape and other LLM ablations	Each ablation used the same training allocation as the corresponding solver–auditor or solver-only run, with a separate or dedicated 1× H100 evaluation worker when needed. Each run used one 20–24h training allocation, plus 1× H100 evaluation allocations when needed.
Validation PV sweeps and downstream evaluations	Each reported final-checkpoint or checkpoint-sweep evaluation used 1× H100, 16 CPU cores, and 64GB host memory. Each job used an 8h allocation; final-checkpoint LiveCodeBench evaluations completed in minutes, while checkpoint sweeps used one such allocation per family/seed.
Toy MAB experiments and plotting	Local CPU jobs, negligible compared with LLM training.

Table 11: Compute resources for the experiments reported in the paper. H100-hour estimates count allocated GPUs times elapsed or allocated wall-clock time, depending on availability of completed-job accounting.

The full research project used more compute than the final runs summarized above. Preliminary experiments, failed or cancelled Slurm jobs, relaunches, and additional seed/ablation runs were used during development; these are not all reported as main results. The dominant additional cost came from extra LLM training launches under the same 2× H100 training pattern, sometimes with a third H100 reserved for online evaluation, plus 1× H100 downstream or validation-evaluation jobs.

F TOY EXAMPLE

F.1 INNER ROUND PSEUDOCODE

Algorithm 3 Inner Round (Toy Example)

Require: A finite reward vector $\mathbf{r} \in \mathbb{R}^d$.

Sample prior over task difficulties, $\tau \sim p(\{\textsf{easy}, \textsf{medium}, \textsf{hard}\})$.
Inject Gaussian noise, $\hat{p} \leftarrow p(\textsf{correct} \mid \tau) + \mathcal{N}(0, \sigma^2)$ and clip to $[0, 1]$.
Sample from solver's action distribution, $a_S \sim \textsf{SolverMLP}(\hat{p})$. $\triangleright\ a_S \in \{\textsf{attempt}, \textsf{abstain}\}$
if $a_S = \textsf{attempt}$ **then**
    Sample from auditor's action distribution, $a_A \sim \textsf{AuditorMLP}(\hat{p})$. $\triangleright\ a_A \in \{\textsf{audit}, \textsf{abstain}\}$
end if
if $a_S = \textsf{abstain}$ **then**
    Set $a_A = \textsf{abstain}$ (no audit).
end if
Sample the true solver correctness label $C \sim p(\textsf{correct} \mid \tau)$.
Evaluate rewards $\mathbf{r}(a_S, a_A, C)$ and update solver/auditor weights.

F.2 OUTER-ROUND PROCEDURE

Algorithm 4 Co-evolution Training Pipeline (Toy Example)

Require: MetaController, SolverMLP and AuditorMLP, initial reward profile $\mathbf{r}_0$, batch sizes $\mathcal{I}, \mathcal{I}'$.

for each outer round $o = 0, 1, \ldots, \mathcal{O} - 1$ **do**
    Train SolverMLP, AuditorMLP for $\mathcal{I}$ inner rounds using $\mathbf{r}_o$.
    Execute $\mathcal{I}'$ inference-only inner rounds (i.e., freeze SolverMLP and AuditorMLP weights).
    Estimate mechanism designer's value $\widehat{W}$ from the inference-only batch.
    Gather context $\mathcal{F}$ from the inference-only batch (e.g., statistics across inner round outcomes).
    Update reward profile, $\mathbf{r}_{o+1} \leftarrow \textsf{MetaController}(\widehat{W}, \mathcal{F})$.
end for

F.3 MAB CONTROLLER ABLATION

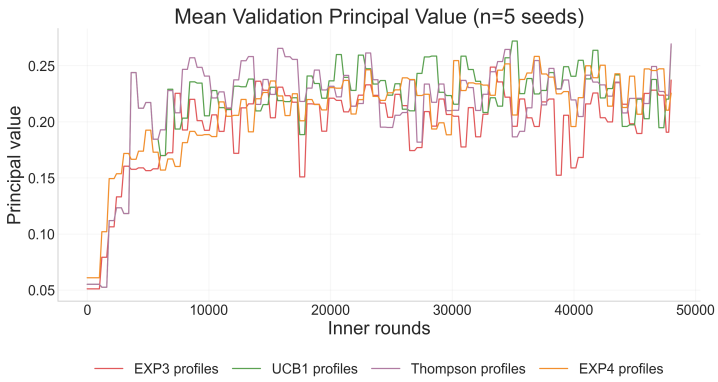

Figure 9: Many MAB algorithms see similar performance, but Thompson profiles improve earlier in these runs.

Action	Correct?	Auditor	Outcome	Explanation
Abstain	N/A	None	$V_{\text{abstain}} = 0.1$	Solver abstains.
Attempt	Yes	No Flag	$V_{\text{aligned}} = 1.0$	Solver correct; no auditor error.
Attempt	No	Flagged	$V_{\text{caught}} = 0.5$	Solver incorrect; auditor catch.
Attempt	Yes	Flagged	$V_{\text{fp}} = -0.3$	Solver correct; auditor false positive.
Attempt	No	No Flag	$V_{\text{silent}} = -1.0$	Solver incorrect; auditor miss.

Table 12: Expanded principal alignment values including solver correctness and auditor behavior.

$(p_{\text{align}}, p_{\text{audit}})$	Interpretation	Equilibrium Condition
$(0, 0)$	misaligned, no audit	$r_{\text{catch}} \leq r_{\text{audit}}$
$(0, 1)$	misaligned, audit	$\Omega_t \geq r_A - r_M$ and $r_{\text{catch}} \geq r_{\text{audit}}$
$(1, 0)$	aligned, no audit	$\Omega_t = 0$ and $r_{\text{audit}} + r_{\text{fp}} \geq 0$
$(1, 1)$	aligned, audit	$r_{\text{audit}} + r_{\text{fp}} \leq 0$ and $\Omega_t \leq r_A - r_M$

Table 13: Pure-strategy equilibria and their conditions.

F.4 Principal Value Table

G Pure-Strategy Equilibria in the Noise-Free Local Model

H More Than One Auditor

The same formulation extends to one solver and multiple critics. Each critic may flag the solver's response or abstain, and the external resolution procedure is triggered when at least one critic flags. From the solver's perspective, the relevant quantity is the probability that some critic initiates a successful correction.

Multiple critics can improve coverage, but they also create coordination problems: free-riding, redundant escalation, and false-positive cascades. The reward mechanism should therefore give credit to informative complaints that contribute to a correct verdict, while penalizing unnecessary escalation and false positives. The principal's outer problem remains the same: choose rewards that deter misalignment without destroying the incentive to monitor.

I Additional Related Work

Inspection games and incentives for oversight. Classical inspection games model costly inspection and strategic violations (Dresher, 1962; Avenhaus et al., 2002). The deterrence and enforcement literature also emphasizes the incentives of enforcers and the possibility of collusion (Becker, 1968; Stigler, 1970; Becker & Stigler, 1974; Mookherjee & Png, 1992; Tirole, 1986; Kofman & Lawarree, 1993; Polinsky & Shavell, 2000), and models auditing as a strategic interaction with a mixed equilibrium (Graetz et al., 1986). Our local solver–auditor game draws on this tradition; its mixed-equilibrium formula is not a novelty claim. In particular, the comparative static in Section 3.2—a larger solver penalty lowers the equilibrium audit probability, while the equilibrium alignment probability depends only on the auditor's payoffs—is the inspection-game "paradox of penalties" (Tsebelis, 1989; 1990). Subsequent work debated its robustness to repeated and multi-player settings (Bianco et al., 1990), showed that rewarding inspectors can shift violation rates in counterintuitive directions (Andreozzi, 2004), studied long-run inspectors facing boundedly rational offenders (Andreozzi, 2010), and analyzed a policymaker who reshapes the payoffs of an inspection game (Holler, 1993). Laboratory experiments find that higher punishment reduces inspection, while offenders respond to penalties more than the mixed equilibrium predicts (Rauhut, 2009; 2015; Nosenzo et al., 2013). These findings motivate treating the learned policies, rather than the equilibrium formula, as the object of evaluation. In AI auditing markets, Bova et al. (2024) identify declining incentives for vigilance. Overman & Bayati (2026) study cooperative oversight with a potential-game structure. Security- and audit-game formulations choose inspection intensity and punishment jointly (Blocki et al., 2013; Sinha et al., 2018), and recent work applies Stackelberg and principal–agent auditing

models to AI oversight and adversarial agents (Kim et al., 2026; Das et al., 2026). We instead choose separate learning rewards for co-trained model policies and evaluate their induced behavior against a distinct principal objective. This distinction does not imply a convergence guarantee for our learning dynamics.

Delegated monitoring and audit design. Costly-state-verification and optimal-auditing models show when random, report-contingent audits are optimal (Townsend, 1979; Border & Sobel, 1987; Mookherjee & Png, 1989). When the principal cannot commit to an inspection policy, delegating monitoring to a separately rewarded supervisor can restore commitment, although it creates scope for collusion (Melumad & Mookherjee, 1989; Strausz, 1997; Khalil, 1997). Rahman (2012) shows that a costly, privately informed monitor can be kept honest by occasionally testing it with deliberately induced violations. The hidden random review of unflagged outputs in Appendix B serves an analogous purpose: it keeps missed errors observable to the principal regardless of the auditor's flagging policy.

Proxy objectives and scalable oversight. Our work connects first to the literature on proxy objectives and scalable oversight. Reward hacking, reward tampering, and goal misgeneralization show that optimized objectives may diverge from the designer's true goal (Amodei et al., 2016; Everitt et al., 2019; Langosco et al., 2022; Skalse et al., 2022; Pan et al., 2022); in language-model settings, this concern is especially salient because RLHF and related post-training methods optimize learned or human-provided feedback rather than the latent objective directly (Christiano et al., 2017; Ouyang et al., 2022; Gao et al., 2023; Casper et al., 2023). RLHF can increase human approval of incorrect answers, including on programming tasks (Wen et al., 2025b). Specification gaming can generalize to reward tampering (Denison et al., 2024), and reward hacking learned in production coding environments can generalize to broader misalignment (MacDiarmid et al., 2025). Coding agents exploit or modify unit tests when doing so earns reward (Zhong et al., 2025; Gabor et al., 2026), and learned verifiers can themselves be exploited during reinforcement learning (Huang et al., 2026). Scalable-oversight methods such as reward modeling, iterated amplification, debate, weak-to-strong generalization, trained verifiers, process supervision, LLM critics, and multi-agent debate aim to obtain better supervision for tasks that are hard for humans to evaluate directly (Leike et al., 2018; Christiano et al., 2018; Irving et al., 2018; Burns et al., 2023; Cobbe et al., 2021; Lightman et al., 2023; McAleese et al., 2024; Du et al., 2023). Empirical and theoretical studies of debate and weak judges examine when a weaker overseer can supervise a stronger model (Khan et al., 2024; Kenton et al., 2024; Brown-Cohen et al., 2024; Engels et al., 2025), and correlated errors between similar models can undermine such oversight (Goel et al., 2025). Relative to these works, we do not primarily propose a new verifier or supervision protocol; we analyze how rewards induce the joint behavior of a solver and an auditor, both of which are adaptive learners.

Co-trained generators and checkers. Prover–verifier games train a prover and a verifier against each other and show that the induced equilibria depend on the game's design (Anil et al., 2021; Kirchner et al., 2024; Hammond & Adam-Day, 2025). In code generation, model-generated tests have been used to rerank solutions (Chen et al., 2023) and to provide debugging feedback (Prasad et al., 2025). Several recent methods co-train a coder and a unit-test generator with reinforcement learning, often rewarding tests that expose faulty code (Wang et al., 2025; 2026; Lee et al., 2026; Li et al., 2025; Lin et al., 2026). Related approaches jointly train generators with learned verifiers or adversarial critics (Zha et al., 2025; Wu et al., 2025; Chen et al., 2025a; Xie et al., 2025), train alignment auditors with reinforcement learning (Rosu & Wang, 2026), or use debate against a critic to limit judge exploitation during RL (Kenton et al., 2026). More broadly, multi-agent RL post-training co-trains several language models on shared tasks (Park et al., 2025; Liu et al., 2026). These methods typically fix the reward assigned to each role. Our focus is complementary: the rewards over joint correction outcomes are themselves the design variable, chosen to trade off solver deterrence against the auditor's incentive to inspect under a separate principal objective.

Adaptive rewards and multi-agent incentive learning. Our work also relates to bilevel reward design, adaptive reward shaping, and multi-agent incentive learning. The optimal reward problem distinguishes the reward given to a learning agent from the designer's objective and optimizes the former for the latter (Singh et al., 2010; Sorg et al., 2010); unlike potential-based shaping, which preserves optimal policies (Ng et al., 1999), such rewards deliberately change the induced behavior.

Prior methods learn intrinsic-reward or incentive functions so that one learner aligns an agent with a principal's objective (Yang et al., 2020; Du et al., 2019; Chakraborty et al., 2023; Zheng et al., 2018; Yang et al., 2022); recent LLM reward-function work similarly adapts reward models or reward code during training (Place, 2023; Ma et al., 2024). Bilevel formulations of reinforcement learning and RLHF treat the designer's problem as optimization over the learner's response (Colson et al., 2007; Shen et al., 2024; Thoma et al., 2024). The AI Economist uses a two-level multi-agent RL scheme in which a planner sets taxes for adapting agents to optimize a separate social-welfare objective (Zheng et al., 2022). Economic and learning-theoretic work studies principals who design contracts or payments for learning agents (Hadfield-Menell, 2021; Ben-Porat et al., 2024; Christoffersen et al., 2023; Scheid et al., 2024; Guruganesh et al., 2024; Ivanov et al., 2024), steer no-regret learners toward desired equilibria (Zhang et al., 2024a), or adapt incentives while agents play (Liu et al., 2022; Maheshwari et al., 2025). These works explore how to learn a reward for a target policy. Our focus is different: we study a solver–auditor correction game in which reward choices can change both the actor's incentive to misbehave and the monitor's incentive to inspect. We analyze and optimize the incentives that such monitors face once they are trained jointly with the systems they oversee.

Online selection of training signals and learning dynamics. Our outer loop selects among reward profiles whose values drift as the agents train. Discounted and sliding-window bandit algorithms address abruptly or gradually changing reward distributions (Garivier & Moulines, 2011; Raj & Kalyani, 2017; Besbes et al., 2014), and rising bandits model arms whose value increases with use (Metelli et al., 2022). Bandit controllers have selected training tasks or curricula (Graves et al., 2017; Chen et al., 2025b) and which reward to optimize during language-model RL fine-tuning (Pasunuru et al., 2020). Our separation of a slow reward controller from fast policy updates follows two-timescale stochastic approximation (Borkar, 1997; Heusel et al., 2017). Learning dynamics in games need not converge to mixed equilibria: regularized learning can cycle, and mixed Nash equilibria that are not strict cannot be stable and attracting under follow-the-regularized-leader dynamics (Mertikopoulos et al., 2018; Flokas et al., 2020). This is why we evaluate learned trajectories rather than rely on the local equilibrium.

AI control, auditing, and mechanism design. Our closest deployment-protocol relatives are AI control and untrusted monitoring, which study how to remain safe when an untrusted model may intentionally subvert a protocol, including through collusion between monitored and monitoring model instances (Greenblatt et al., 2024b; Griffin et al., 2024; Shlegeris, 2024; Gardner-Challis et al., 2026). Related protocols resample suspicious actions, adapt their strictness over time, or quantify the safety cost of monitoring under an audit budget (Bhatt et al., 2025; Wen et al., 2024; Terekhov et al., 2026). Under miscalibrated and correlated confidence, random audit allocation can outperform confidence-based triage (Zavattari et al., 2026). Language models can also collude through steganographic channels (Motwani et al., 2024; Mathew et al., 2025), scheme in context (Meinke et al., 2025; Schoen et al., 2025), behave differently when they believe they are being trained (Greenblatt et al., 2024a), strategically underperform on evaluations (van der Weij et al., 2025), and potentially resist RL training through exploration hacking (Jang et al., 2026); these behaviors motivate the limitations in our conclusion. Related LessWrong/Alignment Forum discussions of "incentives design" emphasize that safety may depend not only on an AI system's motivations or available actions, but also on the incentives that make agents willing to preserve human control, report failures, or monitor one another (Clymer, 2025). Our setting is complementary: rather than treating the monitor as a fixed component of a deployment protocol, we treat the auditor's monitoring policy as induced by the reward mechanism. This also connects to automating-auditing and oversight-robustness proposals, which use auditors, attackers, or adversarial incentives to expose failures of oversight (Hubinger, 2021; Tzfati et al., 2024; Beigi et al., 2026), and to mechanism-design approaches to alignment and feedback elicitation (Hadfield-Menell & Hadfield, 2019; Manyika et al., 2025; Gao et al., 2019). Relative to these lines, our focus is the coupled incentive phenomenon: rewards that deter solver misbehavior can simultaneously make auditing less valuable, undermining the correction signal that made deterrence credible.

Hallucination, abstention, and correction signals. Rewarding correctness without adequately distinguishing uncertainty can encourage guessing (Kalai et al., 2026), and reinforcement fine-tuning can sharply reduce refusal on unanswerable problems (Song et al., 2025). Abstention has a long history as a reject option in classification and selective prediction (Chow, 1970; Geifman & El-Yaniv,

2017); learning to defer similarly routes uncertain cases to an external expert (Madras et al., 2018; Mozannar & Sontag, 2020), analogous to our auditor's escalation to resolution. Language models can partially predict their own correctness (Kadavath et al., 2022), and supervised or preference-based methods train them to refuse or express uncertainty (Zhang et al., 2024b; Yang et al., 2024); see Wen et al. (2025a) for a survey. Multiple language models can also probe one another's knowledge gaps to decide when to abstain (Feng et al., 2024). Recent RL methods reward abstention or calibrated confidence directly (Wei et al., 2026; Damani et al., 2026; Wu et al., 2026). Several report that fixed abstention rewards can collapse to near-universal refusal and propose capability-aware or adaptive corrections (Zhang et al., 2026; Gao et al., 2026; Che et al., 2026), consistent with our positive-abstention solver-only diagnostic (Appendix E.8). In code generation, code language models are often miscalibrated (Spiess et al., 2025), and test-based selective generation and abstention methods decide when to withhold code (Jeong et al., 2026; Zhou et al., 2026). Selective predictors calibrated in-distribution can become overconfident under domain shift (Kamath et al., 2020), consistent with the recalibration gap we observe on LiveCodeBench. Monitoring and critic-based methods use intermediate feedback to detect errors and other undesirable behavior (Wen et al., 2026; Baker et al., 2025; McAleese et al., 2024). Our outcome-based rewards distinguish correct attempts, caught errors, silent failures, false positives, and abstentions. This requires an external resolution signal; it does not solve the problem of obtaining reliable supervision. Learned multi-agent pipelines can also exhibit strategic accommodation (Dang et al., 2026). The abstention and smaller-auditor ablations probe specific aspects of our coding setting, rather than establishing robustness to collusion or a general solution to scalable oversight.