

Beyond Division of Labor: How Roles Shape Feedback and Recovery in Multi-Agent Systems

Preprint

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Abstract

Multi-agent large language model (LLM) systems commonly assign distinct roles such as Planner, Executor, and Reviewer. In this paper, we investigate what these roles actually change beyond their intended division of labor. We evaluate three open-weight models on the Omni-MATH benchmark and two of them on the SciBench benchmark. Throughout these experiments, we hold the agents, agent topology, call budget, and ground-truth evaluator constant. We find that deleting all role instructions largely eliminates functional specialization, yet the team’s first-pass accuracy does not drop. Roles also allocate decision rights. A role-structured Reviewer often withholds approval and forces internal deliberation. Consequently, its team submits answers to the external evaluator less frequently and receives less corrective feedback. In an exploratory comparison where feedback is accessible to both teams, the role-free team recovers from a failed first answer more often across all five model-benchmark settings and achieves higher final accuracy. A post hoc analysis reveals that this role-free advantage disappears when comparing the two teams at an equal number of evaluator verdicts. In a second exploratory intervention, run for the gemma-4 model on 400 Omni-MATH problems, we preserve the role prompts but force a submission in every cycle (one pass of the three agents), and the role-structured team’s recovery rate then matches that of the role-free team. We conclude that roles govern decision rights and information flow as well as work allocation. Evaluating multi-agent organizations therefore requires looking beyond role fidelity to analyze decision policies, feedback access, and failure recovery.

Keywords

Multi-agent systems, large language models, role specialization, division of labor, organizational structure, ablation

1 Introduction

Researchers usually present a multi-agent large language model system as a detailed diagram. It features components like a Planner that decomposes without solving, an Executor that carries out the plan, a Reviewer that critiques rather than re-derives (PER), and

arrows for who hands what to whom [21, 37, 43]. This diagram is read as a mechanism that explains where the system’s competence comes from and justifies the cost of multiple agents. In practice, however, what runs is simply a set of prompts handed to a language model. The model may honor the intended division of labor, ignore it or satisfy its surface constraints while solving the entire problem in every box. A simple accuracy score cannot distinguish between these scenarios. A pipeline whose agents have quietly collapsed into identical solvers returns the same final score as an organization that works exactly as drawn.

Beyond merely dividing work, roles also dictate critical decisions. In a PER team, the Reviewer does not just check the answer. It also decides when the candidate is ready for submission to an external evaluator. This decision is crucial because the evaluator provides corrective information, namely a verdict and a repair hint, which internal deliberation cannot generate. A conservative Reviewer saves evaluations. This behavior is practical when evaluations are expensive. However, every withheld submission is a missed opportunity to discover that an answer is incorrect. Therefore, we investigate what roles alter in a multi-agent system beyond the intended division of labor. We specifically examine how roles govern the team’s access to external corrective information and how this access impacts recovery from failure.

To isolate the effect of roles, we fix the agent topology, the actor model (the language model that plays all three agents), the call budget, and the ground-truth evaluator. We then vary the organization in two directions. First, we completely delete every role instruction. Second, we strictly enforce the division of labor at the interface using typed message formats. Because feedback typically reaches each agent through its prompt, a role ablation also rewrites the feedback channel. We therefore carefully record the exact information each condition receives (§4). In our main comparison, both teams have equal access to the evaluator’s verdict and hint. While access is matched, the realized number and content of verdicts differ. This difference is what roles control.

Deleting roles largely dismantles the intended division of labor (aided by the fact that the role-free prompt asks each agent for its current answer). However, consistent with prior role ablations [10, 35], the team’s first-pass accuracy does not decline. The crucial difference emerges only after a rejected first answer.

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In an exploratory comparison where feedback is available to both teams, the role-free team successfully recovers more of its failed trajectories (runs on a problem whose first answer was rejected) across all five actor and domain settings. This leads to a higher final accuracy. The evidence points to the Reviewer’s decision rights. A role-structured Reviewer frequently withholds approval, often even before any feedback exists. Consequently, the role-structured team submits less often and receives fewer verdicts. Under a capped budget, a withheld submission is a lost chance to recover. A post hoc comparison at equal numbers of evaluator verdicts removes the role-free advantage. Furthermore, an exploratory intervention for the gemma-4 model on 400 problems keeps the role prompts intact but forces the team to submit an answer in every cycle (one pass of the three agents). This intervention raises the role-structured team’s recovery to the role-free team’s level on the same problems. Enforcing the division of labor at the interface also loosens this Reviewer gate and raises recovery, though resolvably only for the Llama-3.3 model. However, 8% to 17% of enforced trajectories never finish, because malformed or boundary-violating outputs use up the allowed regenerations, so end-to-end accuracy does not improve. Ultimately, roles shape two distinct factors simultaneously. They determine who does which work, and they dictate when the team asks for correction. These two factors affect system performance in different ways.

We summarize our contributions as follows:

- (1) We introduce a budget-matched design that varies organization in two directions and strictly documents the information each condition receives. This includes a feedback-withheld diagnostic that shows what a role ablation measures when it also removes the feedback channel.
- (2) We provide empirical evidence across three actors that roles successfully induce functional specialization, and across two domains that they do not improve first-pass accuracy.
- (3) We provide evidence that the Reviewer’s approval governs the team’s feedback opportunities. We support this claim using submission logs, a post hoc verdict-matched comparison, and an exploratory intervention that removes the submission gate while keeping role prompts intact.
- (4) We outline implications for evaluating multi-agent organizations. We recommend reporting recovery rates after a failed first attempt and treating decision rights and information pathways as core components of the organizational structure (§6).

2 Related Work

Roles in organizational models. Organization-centered multi-agent systems give roles a formal existence independent of their occupants. Moise⁺ separates structural, functional and *deontic* specifications [24]. Opera makes role adoption an explicit social contract [7], and electronic institutions specify norms outside the agents [13], to be regimented (violation made impossible) or sanctioned [19]. An LLM role prompt is the degenerate case: functional and deontic clauses in prose, with nothing to make them bind. Our role-free condition keeps the positions, the communication graph and the turn order, but deletes these clauses. Conversely, our enforced condition regiments the functional part at the interface. In

this paper, adaptability means revising an answer across attempts on the same problem, not reorganizing the team [22]. Teamwork models make repairing a failed joint plan a team obligation [41], which a prompt can state but not secure.

Role specialization and role drift. Prompted roles are known to be unreliable. Agents disobey the role specification across annotated traces [4], and *role drift* has a training-time account [3]. Role-label conditioning fails to induce the intended specialization [35], and agents collapse into highly similar rationales [36]. Differentiation can even arise *without* assigned roles [12]. Recent enforcement-side work makes role scope part of the system structure rather than advice in the prompt [15, 40]. While role drift and the decoupling of role quality from task performance are known [3, 44], we measure them under a controlled ablation.

Redundancy and parity. Dylan et al. [10] hold total LLM calls fixed and find an evolved Planner, Executor and Critic team no better than an evolved single agent. Zhang et al. [47] find that many reported multi-agent efficiency gains arise where one agent suffices. This objection is why we report a single-agent reference (§4). On the same premise, Chen et al. [6] prune redundant agents.

Feedback, revision and answer lock-in. Iterative refinement with self-generated or tool feedback [17, 30, 38] and multi-agent debate [9, 29] are standard patterns. Models often repeat earlier outputs under exact verifier feedback [25]. Under self-reflection, this failure is called Degeneration-of-Thought [29], and belief entrenchment is studied directly [33]. Huang et al. [23] report that self-correction degrades accuracy only in the *intrinsic* setting, where the model judges its own answer, and improves it when the correct label serves as a stopping rule [see also 27, 39]. Our loop is gated by such an oracle (an evaluator that knows the correct answer), where more attempts buy more coverage, the share of problems solved by at least one attempt [1]. This is the regime in which the role-structured team’s restraint costs it. Closest to our lever, Chen et al. [5] vary only the *role label* on a byte-identical erroneous claim and move explicit-correction rates by 23 to 93 percentage points. However, they do so in a single agent without budget matching.

Coordination versus capability. Canedo [2] frames structured specification as a *capability equalizer* whose value falls with model strength. Theoretical [46] and complementary [8] accounts agree. Our enforced condition reproduces that direction only under a scoring rule that drops the trajectories that never finish. Under our pre-registered rule (fixed in writing before the data existed), it reverses for the gemma-4 model and vanishes for the other actors (§5.4).

Prior studies of one trace corpus, and critique uptake. Three studies report on one multi-agent trace corpus on the Omni-MATH benchmark [26, 44, 45]. The closest study separates reviewer *precision* from critique *uptake* [44]. It finds that a pipeline reviewer flags real errors more precisely than broadcast-style peer discussion does. Yet its correct critique far less often changes the next candidate, and uptake improves when the critique is placed in the solver’s working context. We ask an organizational question these studies leave open: what the role structure changes beyond the division

of labor, including who decides when the team asks for correction. None of them manipulates the role structure, checks that the specified division of labor is realized, or matches feedback access across conditions. Elsewhere, the failure to revise is almost always measured in a single agent, leaving no organization to ablate.

3 Definitions and Measurement

3.1 What “load-bearing” means here

An organization is specified by a tuple: agents, an interaction order, a message-passing graph, a context-visibility policy, and a *role contract* per agent. A contract asserts that an agent performs some function (MUST decompose the problem) or that it does not (MUST NOT state a final answer). Clauses of the second kind make the specification a *division* of labor. Because a clause is prose handed to a language model, the *realized* organization is not guaranteed to match it. Nothing records which clauses were honored.

Definition. A component of an organization is **load-bearing** if removing or altering it reproducibly degrades task performance *under matched call and information budgets*.

The budget clause ensures we test organization rather than scale. Removing roles alongside agents, calls or context confounds the evaluation. We apply this definition *per phase*, because a team must first produce an answer and then repair it if it is wrong. The information clause covers every channel through which a signal reaches an agent. In a prompted system, feedback arrives through the same templates that carry the roles. Consequently, a role ablation can change both the roles and the feedback channel. A contrast is matched only when it changes one of them (§4). Matched access does not mean matched use: how many verdicts a team draws depends on how often it submits.

3.2 Measuring the realized organization

Functional collapse (primary). The first deliberation cycle (one pass of Planner, Executor and Reviewer) *collapses* if *all three* agents independently assert their own final answers. The pipeline then runs as three solvers in sequence rather than a division of labor. This metric targets the property the diagram asserts, not textual overlap. We measure it on the first cycle so it is not confounded with trajectory length. It is not applicable to the structurally enforced conditions because their schema forbids an answer field.

Exact output duplication (secondary, a lower bound). This occurs when two or more role messages are byte-identical, that is, the same raw completion character for character. It bounds collapse from below (supplement) and is never used to compare actors.

Answer authorship, and computation. We measure whether a Planner authors a final answer. Structural enforcement leaves the Planner no field in which to write one, so this measure serves as the manipulation check. Separately, we track whether a Planner abstains from *computing*, scored as containing no evaluated numeric equality (**P1a**, the measure of pre-registered endpoint P1; §4.1). Because P1a scores the absence of a pattern, it rewards short messages. Therefore, it cannot reliably measure a division of labor, and we use it only as a fidelity check (§4.1).

First pass and recovery. *First-pass* accuracy grades the first answer submitted to the evaluator. It need not come from the first cycle: for the role-structured team, it may follow internal deliberation without external feedback. If a trajectory never submits an answer before the budget runs out, it is graded once on its final answer, and that grade is its first pass. *Recovery* is the fraction of *failed* trajectories, those whose first graded answer was wrong, that nonetheless finish correct. Because recovery conditions on a post-treatment variable (whether the first answer failed, which the intervention can change), recovery contrasts are descriptive [32]. We also report the main contrast on unconditional final accuracy (§5.2). The verdict-matched analysis (§5.3) is conditional by construction.

Validation. P1a was hand-adjudicated against a definition frozen *before* the adjudication sheet was opened. It clears a pre-set agreement threshold of 0.80 (supplement).

4 Intervention

We vary organization in both directions from one baseline: we remove it, and we enforce it. All conditions share three agents, one interaction order (Planner → Executor → Reviewer), the message-passing graph, context visibility (except under enforcement, below) and the total call budget. They also use the same verifier and prompt, retry budget, decoding parameters (temperature 0, that is, greedy decoding, and identical max_tokens), stopping rule (a PASS ends the trajectory) and grading. Every trajectory in the frozen snapshot (the pre-registered dataset, §4.2) carries the identical budget record, so the design tests organization, not scale. Besides the roles, three things differ. After a FAIL, the evaluator returns a repair hint, written with the gold answer (the reference solution) in view and instructed not to reveal it. The role-structured and enforced templates show the hint to all three agents in the next cycle, each through its role’s advice slot, the template field reserved for the evaluator’s feedback (Figure 1). The role-free template has no such slot, so the role-free team receives the feedback as an entry in its shared history, and a variant that withholds it serves as a diagnostic. The role-free footer (the closing instruction of the prompt) also asks every agent for an answer, and the role-structured Reviewer carries two clauses the role-free prompt lacks.

Role-structured PER (baseline). Role prompts are as ordinarily written. The Reviewer is told to approve only a complete answer of the right shape. In a cycle that receives a FAIL, it is told to route the work back to the Planner or Executor rather than approve.

Role-free with feedback (main comparison). All three agents receive one byte-identical prompt, “You are one of three agents collaboratively solving this problem. Continue the work.”, followed by the problem, the shared history and the agent’s slot number (its position, first to third). A footer asks for the agent’s current best answer in `\boxed{ }` and a closing [Agree] (ready to submit) or [Continue], the conventions the role-structured Executor and Reviewer use. Every clause saying who does what is removed, above all the Planner’s MUST NOTs on computing and answering. In every prose condition, the third agent’s [Agree] is approval (a message with neither tag counts as [Continue]), and the latest boxed answer is submitted. After a FAIL, all three agents see the evaluator’s verdict and repair hint, written by the same evaluator and prompt

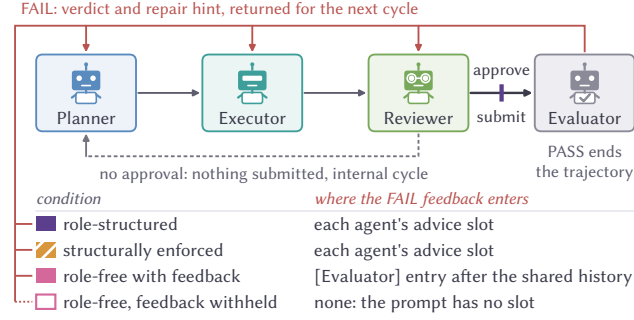


Figure 1: One deliberation cycle and where the feedback enters. A trajectory (one problem’s run, at most six cycles) advances one cycle per pass of Planner, Executor and Reviewer. The third agent’s approval (the bar marks this gate) is the only route to the evaluator, which knows the reference answer and returns PASS, ending the trajectory, or FAIL with a repair hint; without approval nothing is submitted and the team deliberates internally in the next cycle. The key gives where the feedback enters per condition: the advice slot of each role prompt for the role-structured team (Planner, Executor and Reviewer prompts) and the structurally enforced one (the same roles imposed by typed message formats); for the role-free team (one generic prompt for all three agents, with no slot), an [Evaluator] entry in the shared history, or nowhere in the feedback-withheld diagnostic. A trajectory that never submits is graded once when the budget runs out.

as for the role-structured team, as an [Evaluator] entry after the shared history; like the advice slot, it stays visible for one cycle. The two teams thus have the same access to feedback; how many verdicts each draws, and their content, depend on what it submits.

Role-free, feedback withheld (diagnostic). The same prompt without the [Evaluator] entry: nothing in this template carries the evaluator’s verdict or hint. It differs from the main role-free condition only in the feedback and shows what a role ablation measures when it also removes the feedback channel.

Structurally enforced. The interface, not the wording, enforces specialization. The Planner emits JSON (a typed output format) against a schema with *no answer field*, so a final answer is unrepresentable. The Executor receives the parsed plan object and the problem, never the Planner’s raw text, and emits a derivation and an answer. The Reviewer emits a verdict, a target span and a critique. Its accept promotes the Executor’s answer verbatim *by reference*, because it has no field in which to *author* one. Each function stays with one agent under the same budget, but the Executor sees less context, and a rejected output can exhaust the regeneration budget (the number of times a malformed output may be regenerated). A *context-restored control* repeats this condition with the shared history restored.

Single-agent reference. A budget-matched single-agent protocol on the same problems guards against multi-agent gains that merely mimic a single capable agent [47]. It comes from a separate, earlier

corpus (§4.2). The comparison cuts both ways, and the arms come from separate runs (supplement).

4.1 Instrumentation and pre-registered analyses

An intervention that never engages cannot explain an outcome, so we measure engagement. We log every rejected-and-regenerated output with its reason, count only role-boundary violations in the enforcement rate, and count answer-bearing calls per condition. One logging gap is disclosed in §5.1.

All five endpoints (the pre-specified analyses, numbered P1 to P5) were fixed in writing and committed before any intervention data existed. The full table, with the outcome for each, is in the supplement. Units are (problem, actor) pairs, and all conditions run the same problems, so every test is paired, with bootstrap intervals that resample whole problems [11]. The pre-registered primary endpoint, P4, asks whether enforcement’s effect on accuracy differs by actor. It is null: no pairwise difference-in-differences (enforcement’s effect in one actor minus its effect in another) survives Holm’s correction for multiple comparisons [20] (smallest adjusted p -value, $p = 0.17$). Before correction, the 95% bootstrap interval for the Llama-3.3 model against gemma-4, $+0.040 [+0.001, +0.080]$, only just excludes zero, and a separate resample gives $p = 0.055$. Only the gpt-oss against gemma-4 contrast had adequate statistical power for an interaction of plausible size.

The P1a manipulation objective also failed. It required enforcement to drive Planner computation-abstention to ≈ 1.0 , but the schema forbids an *answer*, not a computation. It was re-designated to answer authorship. The role-structured versus feedback-withheld contrast was pre-registered as a secondary, exploratory replication of a published null, and splitting it into phases is post hoc. The role-free condition with feedback, and with it the main comparison, was added after the freeze (when the dataset and analysis plan were fixed). Its design and primary analysis were committed in writing before any of its data existed, but its results are exploratory, as are those of the forced-submission run (§5.3). Exclusions were fixed in advance, including the rule that decides §5.4: *unparseable trajectories count as incorrect rather than dropped*.

4.2 Campaign

Task and problems. We use the Omni-MATH benchmark of competition mathematics [14], sampled by a deterministic seeded manifest (a fixed, reproducible list of problem identifiers) so that every prefix of the run is a valid random sample. All conditions run the same problems, so every comparison is paired. A multiple-choice benchmark was ruled out before launch: on a four-option item, producing a different candidate succeeds one time in three by chance, which would inflate recovery.

Actors and scale. Three actor models are served from one pinned vLLM server [28]: gpt-oss-120b [34], gemma-4-31B [16] and Llama-3.3-70B [18]. The pre-registered analysis reads one frozen snapshot of 11,600 trajectories, 83,320 role messages and 17,486 gradings on 1,600 problems sampled uniformly at random from the 4,181 available. The three frozen conditions run 1,600 problems per cell (one actor in one condition on one domain) for gemma-4 and gpt-oss and 400 for Llama-3.3. The context-restored control covers gemma-4 and gpt-oss at 400 each. The evaluator is gpt-oss-120b in every cell,

Table 1: Functional collapse on the Omni-MATH benchmark. Share of first cycles (one pass of Planner, Executor and Reviewer) in which all three agents assert their own final answer, per actor (the model playing all three agents). RS: role-structured (Planner, Executor and Reviewer prompts); RF⁻ and RF: role-free (one generic prompt for all three agents) with the evaluator’s feedback withheld or shown. *Adjudicated no division*: the share of a judged sample of first cycles in which a model judge with a fixed rubric finds no division of labor; the judge did not see the grade but could infer the condition. The byte-duplication lower bound, the enforced condition and a sensitivity analysis over answer-marker definitions are in the supplement.

actor	functional collapse			adjudicated no division	
	RS	RF ⁻	RF	RS	RF ⁻
gemma-4-31B	0.000	0.976	0.974	0.000	0.857
gpt-oss-120b	0.120	0.846	0.883	0.000	1.000
Llama-3.3-70B	0.000	0.872	0.900	0.000	1.000

itself one of the actors under study (§7). The single-agent comparator is *not* part of this campaign; it comes from an earlier, separately collected corpus on the same benchmark, joined by problem identifier. Power simulated from that corpus is high for the paired per-actor main effect (§4.1 for the interaction). An early campaign, stopped when outputs cut off at the token limit were counted as role-boundary rejections, was never pooled (supplement).

The role-free condition with feedback. It was run after the freeze (4,748 trajectories) on the same server, problems, budget and evaluator, for all three actors on Omni-MATH and for gemma-4 and gpt-oss on the SciBench benchmark [42]. Its data are manifested separately and never pooled into a frozen cell. The two gemma-4 cells were run a second time, independently. A re-run (the first of two; supplement) of gemma-4’s role-structured condition on 400 frozen problems shows no detectable drift in final accuracy (0.818 against 0.825 frozen, paired $p = 0.75$; recovery 0.198 against 0.146). Decoding at temperature 0 is not deterministic on a shared server. The two role-free conditions are byte-identical until the first verdict, yet they agree on gemma-4’s first verdict for 1,496 of 1,598 problems, and their first-cycle collapse differs by up to 0.04 (Table 1). We therefore treat a cell as one sample and give the second run, where we have one, in the supplement. Prompts, runner, analysis scripts and data manifests are in the supplementary material.

5 Results

All statistical contrasts are paired at the problem level, with bootstrap intervals clustered by problem. Trajectories that never produce a graded answer count as incorrect, except in the first-pass contrast of §5.1. Significance is the exact McNemar test [31] on discordant pairs (problems that exactly one condition gets right).



Figure 2: Roles organize the team; the Reviewer’s gate limits feedback and recovery. Conditions: role-structured (Planner, Executor and Reviewer prompts); role-free (one generic prompt for all three agents) with the evaluator’s feedback shown, or withheld as a diagnostic (hollow); structurally enforced (roles imposed by typed message formats; Omni-MATH only). Actors are the models playing all three agents. (A) First-pass accuracy: whether a team’s first submitted answer is correct. (B) Recovery: the share of first-pass-wrong trajectories (runs whose first graded answer was wrong) that finish correct, each condition over its own set; brackets give the exact McNemar p for role-structured against role-free with feedback, paired on problems both first answer wrongly. (C) How gemma-4’s first-pass-wrong Omni-MATH trajectories spend the six-cycle budget: cycles ending in a submission (the third agent approved) or internal cycles without submission (it declined). The bottom row is the exploratory forced-submission run on its own 400-problem subset: role prompts kept, the answer submitted in every cycle. (A) is over trajectories with a first verdict; in (B) trajectories that never finish count as unrecovered; error bars are bootstrap 95% intervals.

5.1 R1. Roles create specialization, but not a better first answer

We delete the role clauses and keep the agents, interaction order, budget and evaluator; the role-free footer also asks every agent for its current best answer. Functional collapse, the share of first cycles

in which every agent asserts its own final answer, rises from at most 0.120 with roles to at least 0.846 without them for all three actors on Omni-MATH, the gemma-4, gpt-oss and Llama-3.3 models (Table 1). Most of the change comes from the Planner (per-role rates in the supplement). The Executor answers in both conditions; the Planner seldom answers with roles (at most 0.183) and nearly always does without them, so the division of labor rests on one prohibition, its *MUST NOT* clause. Because the role-free footer asks for an answer, the absolute level partly reflects that request; the claim concerns the *change*. The ordering holds for every actor under all three answer-marker definitions (the cues that count as asserting an answer; supplement).

Content duplication and a second opinion. For the gemma-4 and Llama-3.3 models, the role-free Planners write full solutions before boxing an answer, and their messages share far more numbers with the Executor’s than with roles, so the Executor redoes the Planner’s work (supplement). A frontier model judging a sample of first cycles against a frozen rubric agrees with the collapse measure (Table 1); the judge was outcome-blind but not condition-blind (supplement), and the collapse claim does not rest on it.

First-pass accuracy does not follow. The collapse costs no accuracy: on problems graded both with roles and without them (the two role-free conditions are identical until the first verdict), first-pass accuracy is *identical* for gemma-4 (+0.0000, [−0.0156, +0.0150]), flat for Llama-3.3 and higher without roles for gpt-oss (+0.0503, [+0.0339, +0.0666]; Figure 2A). Because one model plays every agent, a Planner that solves the problem brings no competence the Executor lacks. The gpt-oss model may gain because its answers vary between derivations (§5.3), so extra independent solutions add chances to be right; we did not test this.

A logging defect. Our runner recorded a response’s hidden reasoning trace only when its visible content was empty, so most of gpt-oss-120b’s role-free derivations (75.7% of messages) are missing from the record (supplement). Agents pass content only, in every condition, so the defect is in the record alone and leaves no point estimate of gpt-oss’s duplication. It does not manufacture the collapse: in first cycles with no affected message (106 without roles), role-free collapse is 0.821 and role-structured 0.118.

5.2 R2. With feedback available to both teams, the role-free team recovers more

What a cycle is. The pipeline runs a fixed grid of six deliberation cycles, three outer rounds of two inner rounds, each one pass of Planner, Executor and Reviewer; Figure 1 shows how a cycle ends in a *submission* or as an *internal cycle* without submission.

Recovery and final accuracy. Both teams can obtain the evaluator’s feedback, so this is the main, exploratory comparison (Table 2; §4.1). The role-free team recovers more in all five cells. For gemma-4 on Omni-MATH it recovers 0.609 of its failed trajectories against 0.222 (each team over its own; Figure 2B), and of the 236 problems both first answer wrongly, 89 are recovered only without roles and 4 only with them ($p = 6.2 \times 10^{-22}$). Both teams reach the same first-pass rate and never lose a correct first answer, so the extra recoveries carry straight into final accuracy (0.921 against

0.846). They are genuine revisions: nearly every passing answer differs from the one first rejected, and identical evaluator prompts almost never disagree (supplement). The other two actors repeat the ordering, and the feedback leaves their first-pass accuracy essentially unchanged, as expected, since the two role-free conditions are identical until the first verdict.

Leakage and reproducibility. A hint written with the gold answer in view could hand it over. Hints contain it verbatim in at most 2.3% of failing verdicts in any cell, and dropping the gemma-4 trajectories on Omni-MATH that saw one barely moves either recovery rate (supplement); we did not audit partial leakage. A second, independent run of the two gemma-4 cells with feedback is close to the first, though the paired test on the SciBench benchmark weakens from $p = 0.013$ to 0.052; we report the first run throughout (supplement).

5.3 R3. The Reviewer’s approval governs feedback opportunities

Each submission returns a verdict and, on a FAIL, a hint written with the gold answer in view, so every submission is another chance to revise. The two teams differ in how often they seize this opportunity.

A gate on submission. The role-structured Reviewer dictates when the team submits. For the gemma-4 model, this team submits 1.27 times per failed trajectory, compared to 3.20 for the role-free team (Figure 2C), and it submits less often in every cell (Table 2). Much of this withholding occurs before any feedback exists: 53% of the Reviewer’s declines happen before the first submission, and 84 of the 316 failed trajectories never submit, so they are graded once at the end and never see external feedback. The prompt clause forbidding approval right after a FAIL covers only 19% of the declines. In later cycles where approval is allowed, the Reviewer still declines 75% of the time, against 4% for the role-free team’s third agent. On Omni-MATH the clause covers at most 37% of the declines for any actor, so there the gate reflects the Reviewer’s disposition rather than the letter of its prompt, plausibly because an agent instructed to critique will search for flaws; on SciBench the clause covers more (supplement).

Removing the gate. If this gate causes the gap, bypassing it with the role prompts unchanged should eliminate the gap. In a direct, exploratory test, we kept the role-structured prompts byte for byte but submitted the team’s answer in every cycle, overriding the Reviewer. We ran it once, for gemma-4 on the 400 Omni-MATH problems of the post-freeze re-run (supplement). Every cycle the team uses then becomes a submission (Figure 2C, bottom row), and recovery surges from 0.198 to 0.613 (paired, 26 against 2, $p = 3.0 \times 10^{-6}$), matching the role-free team’s 0.549 on the same problems (7 against 7). First-pass accuracy does not move, so the Reviewer’s early declines did not buy a better first answer either. With the role prompts unchanged, overriding the Reviewer’s approval closed the gap in this run.

Matched on verdicts (post hoc). A descriptive analysis examines whether the gap persists at an equal number of verdicts. On problems both teams first answer wrongly, we truncate each role-free

Table 2: Role-structured against role-free with feedback, in every cell (one actor, the model playing all three agents, on one benchmark; n problems). RS: role-structured (Planner, Executor and Reviewer prompts); RF: role-free (one generic prompt for all three agents) with the evaluator’s feedback shown; RF⁻: role-free with it withheld. A trajectory (one problem’s run) is first-pass wrong when its first submitted answer is graded wrong. *Recovery*: share of first-pass-wrong trajectories that finish correct, each condition over its own set. *Recovered only by*: on the n problems both RS and RF first answer wrongly, those recovered by one condition and not the other, with the exact McNemar p for the two counts. *Verdict-matched* (post hoc): the same paired recovery with each RF trajectory cut off after as many evaluator verdicts (PASS or FAIL decisions) as RS received on that problem, so both get the same number of verdicts and hints. *Final accuracy*: correct at the end, over all problems, paired; never-graded trajectories count as incorrect. *Submissions*: answers the team submits (third-agent approvals) per first-pass-wrong trajectory, excluding the single end-of-budget grading. *RS never submits*: RS first-pass-wrong trajectories with no submission in six cycles, graded once at the end.

actor, domain (n)	recovery			recovered only by (paired)				verdict-matched			final accuracy			submissions		RS never
	RS	RF	RF ⁻	n	RS	RF	p	RS	RF	p	RS	RF	p	RS	RF	submits
gemma-4, Omni-MATH (1,600)	0.222	0.609	0.051	236	4	89	6.2×10^{-22}	0.165	0.153	0.69	0.846	0.921	1.7×10^{-24}	1.27	3.20	84/316
gpt-oss, Omni-MATH (1,600)	0.324	0.552	0.247	319	17	101	1.0×10^{-15}	0.229	0.216	0.72	0.803	0.894	2.7×10^{-28}	2.10	3.53	51/463
Llama-3.3, Omni-MATH (400)	0.014	0.147	0.015	250	1	26	4.2×10^{-7}	0.012	0.008	1.0	0.315	0.415	1.6×10^{-7}	0.32	2.71	205/276
gemma-4, SciBench (574)	0.254	0.419	0.051	100	7	21	0.013	0.230	0.190	0.48	0.847	0.880	0.0054	1.95	3.62	3/118
gpt-oss, SciBench (574)	0.315	0.483	0.231	93	7	18	0.043	0.215	0.247	0.68	0.852	0.892	1.9×10^{-4}	2.68	3.95	1/124

trajectory to the number of verdicts the role-structured team received. No cell then shows a significant difference (Table 2; Llama-3.3’s cell has only five recoveries in total); pooled and stratified by cell, the difference is +0.009 in favor of roles (95% interval -0.014 to $+0.033$). The matching is coarse: a trajectory cut off after a single FAIL cannot recover, so wherever the role-structured team received one verdict the pair ties at zero by construction, as in most of gemma-4’s pairs. Restricted to problems with at least two verdicts, the difference remains not significant; recoding the scoring defect of \$7 gives +0.029 (+0.004 to +0.054), a small significant margin for roles (further variants in the supplement). No variant puts the role-free team significantly ahead. A truncation cannot show that the two teams use each verdict equally well, but combined with the forced-submission run it indicates that the gap stems mainly from how often the team requests feedback, rather than from how effectively it uses that feedback.

The feedback-withheld diagnostic. This diagnostic removes external feedback entirely and reveals what a role ablation measures when it also removes the feedback channel. The role-free team still submits in almost every cycle (Figure 2C), but its agents receive no signal that the answer failed, so they repeat it: for gemma-4 the team keeps a single distinct answer in 84.5% of failed trajectories. Adding the verdict and hint to the identical prompts changes this behavior. The team explores new answers, and recovery rises from 0.051 to 0.609 (paired, 139 against 2, $p = 7.2 \times 10^{-39}$). Against this diagnostic alone, roles would appear load-bearing for repair: strongly for gemma-4 (0.222 against 0.051), directionally for gpt-oss (0.324 against 0.247; $p = 0.21$), whose answer moves even without feedback (a single distinct answer in 20.8% of failed trajectories), and not for Llama-3.3 (0.014 against 0.015), whose role-structured team rarely submits.

5.4 R4. Generality and organizational constraints

A second domain. The SciBench benchmark’s complete text-only slice of 574 free-response college science problems, frozen separately, tests whether the pattern belongs to the organization or to competition mathematics. The role-free prompt is byte-identical across domains; the role-structured and evaluator prompts adapt only their domain wording (supplement). For both actors run there, the gemma-4 and gpt-oss models, the ordering returns (Table 2): first-pass accuracy is flat, recovery is highest with feedback and no roles, and so is final accuracy. The recovery contrast is weaker, consistent with a looser gate: the role-structured team almost always submits at least once (all but 3 and 1 of its failed trajectories), so more of the feedback reaches it, and, as on Omni-MATH, the contrast vanishes when both teams are credited with the same number of verdicts (answer-lock rates in the supplement).

Structural enforcement. Enforced and role-structured prompts carry the same advice slot, so this contrast is matched on feedback access. Enforcement replaces the behavioral request with a constraint: the Planner’s final answer becomes unrepresentable. The manipulation check passes: the Planner asserts a final answer in 0.000 of enforced first cycles for all three actors, against at least 0.889 without roles. Enforcement does not change what the Planner computes, so the adjudication still finds no functional division in 0.071 to 0.188 of enforced first cycles: an agent can be structurally stopped from answering without being stopped from doing the work. It does loosen the gate: the enforced team submits more often for every actor and, with more hints arriving, unlocks the answer for gemma-4 and Llama-3.3 (supplement).

Recovery rises most where the gate was tightest. Llama-3.3’s role-structured team almost never submits, and under enforcement its recovery rises about tenfold, from 0.014 to 0.139 ($p = 3.8 \times 10^{-6}$), close to the role-free team’s 0.147. For gemma-4 and gpt-oss the direction is the same ($p = 0.30$ and 0.11) but not resolvable against

attrition: enforced trajectories that died after an incorrect first verdict count as unrecovered, as the pre-registration requires.

Improved revisability does not become end-to-end accuracy, because enforcement is brittle. A malformed or boundary-violating output must be regenerated, and when the regeneration budget runs out the trajectory dies: 8.0%, 9.9% and 17.0% of enforced trajectories never finish, against at most 2.5% in any other condition, and the weakest actor breaks the schema most often (supplement). Under the pre-registered rule, which counts these trajectories as incorrect, enforcement changes accuracy by -0.0225 , -0.0050 and $+0.0175$ for gemma-4, gpt-oss and Llama-3.3: one significant negative and two nulls. Dropping them instead gives gains for all three actors, all significant and ordered by actor weakness, matching the direction reported for the capability equalizer [2]. That reading is survivorship bias, since it scores only the trajectories that finished. Restoring the shared history at fixed enforcement helps gemma-4, mostly through fewer trajectories dying, and not gpt-oss (supplement).

6 Discussion

Roles allocate decisions as well as work. Generating a first answer is a generation problem. Three agents reaching the same conclusion by different routes cost only tokens, so first-pass accuracy remains largely unaffected by the division of labor. After a failure, however, the team must produce something distinct from the rejected answer, and except for the gpt-oss model, teams rarely change their answer unless told it failed. Once this failure signal is provided, the role contract adds little capacity for revision: matched on the number of verdicts, the difference between the two teams nearly vanishes. The role contract primarily dictates when the team asks for external evaluation. A role-structured Reviewer withholds approval until it is satisfied, so its team draws fewer verdicts, sometimes none. In one exploratory run, overriding this Reviewer closed the gap for the gemma-4 model.

A design trade-off, not a defect. A Reviewer that withholds approval conserves external evaluations, which is valuable when evaluations are costly, noisy or unavailable. In our setting, with a ground-truth evaluator and a fixed cycle budget, a strict Reviewer instead limits the team’s opportunities to obtain corrective feedback, and fewer verdicts mean fewer recoveries. We measure this side of the trade-off, not the full cost and performance balance. Two design implications follow for similar settings. First, if a cheap and reliable verifier is available, let the team submit frequently and gate on the verifier rather than on an internal critic. Second, whatever the role structure, route the verifier’s feedback to every agent that can act on it, as the feedback-withheld diagnostic shows (§5.3).

Information audits. In a prompted multi-agent system, feedback, tool outputs and verifier verdicts reach the agents through slots in the same templates that carry the role instructions. Rewriting these templates during an ablation can therefore silently remove a channel, and no budget record, call count or accuracy metric reports the absence. A role ablation thus requires an information audit alongside the budget audit: the runtime inputs each template consumes must be documented, and a contrast is matched only when these input sets are identical. Even with matched access, the

realized use can differ, because the organization decides how often the channels are used.

Decision rights and information pathways. Prior work shows that precise reviewing does not guarantee that a critique is adopted [44]. Similarly, a clean division of labor does not guarantee that a team recovers from errors. The critical factor is whether corrective information reaches the team and changes its next actions. A task-outcome evaluation alone cannot reveal this, because an aggregate score averages the stages where the organization sits idle with those where it does its work. We recommend reporting performance conditioned on a failed first attempt, measuring the realized organization instead of assuming the specified one, and treating who may submit and which agents see the feedback as core parts of the organization’s specification.

7 Limitations

Evaluation paradigm and oracle-gated recovery. The recovery loop in our experiments is gated by an oracle. The evaluator triggering retries sees the ground truth, so every reported recovery is oracle-assisted, resting on a ground-truth-informed failure signal rather than autonomous self-correction. Stopping at the correct answer also hides revisions that would have turned a correct answer into a wrong one, and an evaluator that can be queried repeatedly favors the team that queries it most (§5.3).

Scope, provenance, and experimental disclosures. We test one PER organization, with an evaluator that is itself one of the models under study. While the ordering returns across a second domain of college science problems, the findings are limited to text-based reasoning tasks. The role-free condition with feedback, on which the main comparison rests, and the forced-submission run are exploratory follow-ups, added after the freeze and outside the pre-registration. The forced-submission run covers only one actor, one domain, and a single run, and it removes the entire gate rather than only the clause forbidding approval after a failure. Matched access to feedback does not match the realized number or content of verdicts, and we do not show that any submission policy is cost-optimal. The condition with feedback also differs in feedback presentation: role-structured agents receive feedback inside role-specific instructions, whereas role-free agents view it as a bare [Evaluator] entry. Finally, the context-restored control is only a *partial* control due to one-sided attrition (§5.4), and our functional measure only detects whether every agent asserted an answer, not whether their reasoning was identical.

A scoring defect. The automated runner graded the last *submitted* answer but recorded the last *held* answer. For the gemma-4 model on the Omni-MATH benchmark, the two disagree on 17% of role-structured first-pass-wrong trajectories, and on more in some other cells (per-cell rates in the supplement). Recoding the affected trajectories narrows the unadjusted recovery gap and shifts the pooled verdict-matched difference to $+0.029$ [$+0.004$, $+0.054$], which significantly favors the role-structured team (§5.3). The primary analysis keeps the scoring rule that was fixed before the data were collected.

8 Conclusion

Deleting multi-agent roles largely eliminated the intended division of labor in every tested model, yet first-pass accuracy did not fall. Roles do more than allocate work: they also allocate decisions. A role-structured Reviewer controls when the team submits to the external evaluator, and it frequently withholds approval. In exploratory comparisons with feedback available to both teams, the role-free team recovered more often in every tested setting. In a post hoc comparison at equal numbers of verdicts the advantage disappeared, and for gemma-4, forcing a submission in every cycle with the role prompts kept raised the role-structured team’s recovery to the same level. Roles do not just assign responsibilities; they also govern decisions and information flow, which shape how a team recovers from failure. Building an effective multi-agent organization therefore goes beyond assigning roles correctly. It requires designing decision rights and information pathways that support the desired system behavior.

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Supplementary Material

What this document is

The main paper states every claim it makes and is complete without this file. What it cannot carry in eight pages is the full numeric backing behind some of its sentences, and that backing is here, in the order of the main paper’s results: the realized organization (R1), recovery (R2), the submission gate (R3), and the second domain and structural enforcement (R4), followed by the pre-registered end-points and the single-agent comparator. Every table is generated by a script from the manifested data (the frozen snapshot, or the separately manifested post-freeze runs) with the same scripts and outputs as the main paper’s tables, except the single-agent comparator, which comes from an earlier, separately collected corpus. The four figures are generated from the same outputs.

Terms used throughout. A *trajectory* is one problem’s run through the pipeline, at most six *cycles* of Planner, Executor and Reviewer. A cycle ends in a *submission* to the evaluator when the third agent approves the current answer, and in a *decline* otherwise; each submission returns a *verdict*, PASS or FAIL, and on a FAIL a repair hint. *First pass* is whether the first submitted answer is correct; a trajectory that never submits is graded once when its budget runs out, and that grade is its first pass. *Recovery* is the share of *first-pass-wrong* trajectories that finish correct. Recovery is reported on two populations that must not be read across: the *marginal* population is every first-pass-wrong trajectory of a condition, on that condition’s own denominator; the *paired* population is only the problems first-pass wrong under both conditions being compared, on one shared denominator, and it is the population every test of recovery uses. Tests are exact McNemar tests on the problems that exactly one condition gets right; intervals are 95% bootstrap intervals that resample problems; where several tests form a family, *p* values are corrected with Holm’s step-down procedure. Two *scorings* recur: the *pre-registered* scoring counts a trajectory that never produced a graded answer as incorrect, and the *as-graded* scoring drops it. The conditions are abbreviated RS (role-structured: Planner, Executor and Reviewer prompts), RF (role-free: one generic prompt for all three agents, with the evaluator’s feedback shown), RF[−] (role-free with the feedback withheld) and SE (structurally enforced: the same roles imposed by typed message formats). The actors are the gemma-4-31B-it, gpt-oss-120b and Llama-3.3-70B-Instruct models, each playing all three agents.

A The realized organization (R1)

The main paper reports that deleting the role clauses makes every agent assert its own final answer. Table S1 is the full form of its Table 1, with two further measures and the enforced condition, and Figure S1 shows which agent changes.

The Planner is the agent that changes. By role, the share of first cycles in which an agent asserts its own final answer moves from RS to RF[−] as follows, for gemma-4-31B-it, gpt-oss-120b and Llama-3.3-70B-Instruct in turn: Planner 0.000, 0.183 and 0.000 to 0.979, 0.898 and 0.889; Executor 0.973, 0.907 and 1.000 to 0.993, 0.951 and 0.982; Reviewer 0.781, 0.823 and 0.492 to 0.998, 0.961 and 0.965. The Executor’s job is to produce the answer, so it answers under both conditions. The Reviewer often states an answer even with

Table S1: Functional collapse on the Omni-MATH benchmark under three measures, with the structurally enforced condition and a sensitivity analysis over answer markers.

actor	functional collapse	exact byte duplication	adjudicated no division
<i>role-structured (RS)</i>			
gemma-4-31B-it	0.000	0.000	0.000
gpt-oss-120b	0.120	0.000	0.000
Llama-3.3-70B-Instruct	0.000	0.000	0.000
<i>role-free, feedback withheld (RF[−])</i>			
gemma-4-31B-it	0.976	0.000	0.857
gpt-oss-120b	0.846	0.680 [†]	1.000
Llama-3.3-70B-Instruct	0.872	0.008	1.000
<i>role-free, with feedback (RF)</i>			
gemma-4-31B-it	0.974	–	–
gpt-oss-120b	0.883	–	–
Llama-3.3-70B-Instruct	0.900	–	–
<i>structurally enforced (SE)</i>			
all actors	n/a	≤ 0.001	0.071–0.188

RS: role-structured prompts; RF[−] and RF: role-free prompts with the evaluator’s feedback withheld or shown (RF was run after the freeze); SE: structural enforcement. Functional collapse: share of first cycles in which all three agents assert their own final answer. Exact byte duplication: share of first cycles in which two or more of the three messages are identical character for character, a lower bound on collapse that is never used to compare actors. Adjudicated no division: share of a judged sample of first cycles in which a model judge with a fixed rubric finds no division of labor; the judge did not see the grade but could infer the condition. Collapse is not applicable under SE, whose Planner schema has no answer field; for RF only functional collapse was measured. *Sensitivity.* Under three answer-marker definitions the feedback-withheld role-free rate is 0.846–0.999; the role-structured rate is 0.000–0.238 under the two stricter markers and 0.328–0.892 under the loosest, which also counts any “therefore”, “hence” or “= number” (as when a Planner restates givens), so read the role-structured level as “low,” not a point estimate. The ordering holds in all 9 of 9 actor×marker cells. [†]Not interpretable: 80% of this cell’s planner messages are under 500 visible characters (the main paper’s logging defect).

roles, plausibly because approving an answer tends to restate it. The division of labor therefore rests on the Planner’s single prohibition, and the role-free footer, which asks every agent for its current answer, lifts it.

Without roles, Planner and Executor compute the same quantities. The numbers in a Planner’s first-cycle message and in the Executor’s overlap more than Planner and Executor messages from different problems in the same condition do, by +0.449, +0.680 and +0.432 without roles against +0.159, +0.167 and +0.145 with them (Jaccard similarity of the two number sets). Under roles the Planner writes a plan and the Executor computes; without them both compute the same quantities. The gpt-oss-120b value without roles is not interpretable, because most of those Planner messages are only a few characters long (the logging defect below).

A second opinion from a model judge. A frontier model rated the first cycles of 171 sampled trajectories (42 RS, 56 RF[−] and the rest SE) against a rubric frozen before the first judgment, asking whether a division of labor was realized. Its verdicts agree with the functional-collapse measure (Table S1), and across the three rubric questions checked for stability its repeated judgments agree on 0.947 to 0.959 of the items. The judge did not see the grade, but it

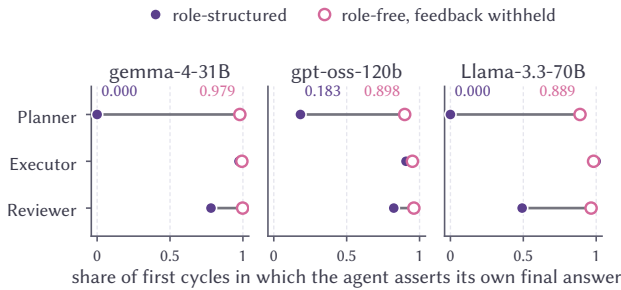


Figure S1: Which agent changes when the role clauses are deleted. For each actor, the share of first cycles in which the Planner, Executor and Reviewer assert their own final answer under role-structured prompts (filled) and under role-free prompts with the evaluator’s feedback withheld (hollow), on the Omni-MATH benchmark. The Planner moves from almost never answering to almost always answering; the Executor answers under both; the Reviewer moves less.

could infer the condition, because role words were renamed rather than equalized across conditions; the collapse claim therefore does not rest on it.

The logging defect, in detail. A message whose hidden reasoning was discarded is short but slow, so we mark messages with fewer than 10 visible characters per second of latency. They are 75.7% of gpt-oss-120b’s role-free messages and 7.7% of its role-structured ones, and 93.4% and 22.7% of its first cycles contain at least one. The runner’s own fallback flag marks only 5.8% and 2.7% of the same messages, so the flag undercounts the defect. For the other two actors at most 0.5% of messages are marked. The defect cannot have produced the collapse: on first cycles with no marked message, gpt-oss-120b’s functional collapse is 0.821 ($n = 106$) without roles and 0.118 ($n = 1,236$) with roles.

Validating the computation-abstention measure. P1a, the pre-registered measure of whether a Planner abstains from computing, was checked by hand on 72 Planner messages against a definition frozen before the adjudication sheet was opened, each judgment with a recorded reason. It agrees with the hand labels on 0.903 of messages (Cohen’s $\kappa = 0.806$; precision 0.943, recall 0.868), clearing the pre-set 0.80 threshold. Agreement fell from 0.967 at $n = 30$ as adjudication continued, and we report the final figure. Its residual errors point in opposite directions (symbolic results with no digits are missed, restated givens are flagged), so they do not push the measure one way.

B Recovery (R2)

The main paper reports that, with the same access to feedback, the role-free team recovers more, and that against the feedback-withheld diagnostic the roles would look load-bearing (the main paper’s R3). Table S2 gives that frozen feedback-withheld comparison on both populations, so the marginal rates and the paired test can be checked against each other; the comparison with feedback is the main paper’s Table 2.

Where the diagnostic gap comes from. For gemma-4-31B-it the accounting closes exactly. The two conditions reach a correct first answer on the same number of problems (1,284 of 1,600), and a correct first answer is never lost because the loop halts on a PASS, so the gap in final accuracy, 54 problems in favor of roles ($p = 1.6 \times 10^{-5}$), is exactly the gap in recoveries, 70 against 16. Most of it arises on the 237 problems both teams first miss, where 38 are recovered with roles against 4 without; the problems that only one team first misses contribute a further 20 in the same direction. This accounting needs equal first-pass counts and does not transfer to the other two actors.

The result reproduces. The two gemma-4-31B-it cells of the RF condition were run twice, independently and about thirteen hours apart, with byte-identical runner and prompt code; every other RF cell was run once. Table S3 gives both runs against the frozen RS cell. The ordering and the final-accuracy contrasts reproduce in both domains; the SciBench paired recovery test, on 100 problems, is $p = 0.013$ in the first run and $p = 0.052$ in the second, so on that benchmark the recovery contrast is at the edge of resolution. Each run also repeated the drift check of the main text on the same 400 frozen problems: the RS re-run reaches final accuracy 0.818 in the first and 0.845 in the second, against 0.825 frozen (paired $p = 0.75$ and $p = 0.20$). The main text reports the first run throughout.

Recoveries are revisions, and the hints do not leak the answer. Of the 193 recoveries of the gemma-4-31B-it RF team on Omni-MATH, 192 pass with an answer different from the one first rejected (541 of 547 across the five RF cells), so re-grading the same answer explains almost none of them; identical evaluator prompts at temperature 0 return different verdicts in only 20 of 2,790 prompt groups (0.0072). The evaluator writes each hint with the gold answer in view, so a hint could hand the answer over. Hints contain it verbatim in at most 2.3% of failing verdicts in any cell, and dropping every gemma-4-31B-it Omni-MATH trajectory that saw such a hint leaves recovery at 0.599 for RF and 0.212 for RS, against 0.609 and 0.222 on all trajectories. We did not audit partial leakage.

C The submission gate (R3)

The main paper’s account of what roles do after feedback is that the role-structured Reviewer gates submission: the RS team asks for fewer verdicts, and matched on verdicts no significant difference remains. This section gives the evidence in three steps: where the declines happen (Table S4), how recovery tracks exposure to the evaluator (Figure S2), and how far the verdict-matched comparison can be pushed (Table S5, Figure S3a). The forced-submission run (Table S6) then removes the gate directly.

Where the Reviewer declines. On Omni-MATH the clause that forbids approval after a FAIL covers only a minority of the RS Reviewer’s declines, between 4% and 37% depending on the actor, and in the free cycles, where no clause applies, it approves far less often than the RF team’s third agent (Table S4). The gate is therefore mostly the Reviewer’s disposition rather than the letter of its prompt; gpt-oss-120b’s Reviewer is the most permissive of the three. On SciBench the clause covers more of the declines, and the RS team almost always submits at least once, which is why the recovery contrast is weaker there (Section D).

Table S2: First-pass accuracy and recovery with roles and without them (feedback withheld) on the Omni-MATH benchmark, on both populations.

<i>Marginal population: first pass paired; recovery on each condition's own denominator</i>						
actor	first pass			recovery first verdict wrong		
	RS	RF ⁻	difference	RS (<i>n</i>)	RF ⁻ (<i>n</i>)	difference
gemma-4-31B-it	0.802	0.802	+0.0000 [-0.0156, +0.0150]	0.222 (316)	0.051 (316)	-0.1709 [-0.2247, -0.1171]
gpt-oss-120b	0.713	0.763	+0.0503 [+0.0339, +0.0666]	0.324 (463)	0.247 (380)	-0.0766 [-0.1361, -0.0153]
Llama-3.3-70B-Instruct	0.308	0.316	+0.0077 [-0.0283, +0.0437]	0.014 (276)	0.015 (268)	+0.0004 [-0.0214, +0.0192]
<i>Paired population: problems first-pass wrong under both conditions; one shared denominator</i>						
actor	<i>n</i> _{both}	RS	RF ⁻	RS only	RF ⁻ only	exact <i>p</i>
gemma-4-31B-it	237	0.160	0.017	37	3	1.95×10^{-8}
gpt-oss-120b	327	0.242	0.202	52	39	0.208
Llama-3.3-70B-Instruct	245	0.004	0.016	1	4	0.375

RS: role-structured; RF⁻: role-free with the evaluator's feedback withheld. First pass: whether a trajectory's first submitted answer is correct. Recovery: the share of first-pass-wrong trajectories that finish correct. Upper block: first pass over the problems graded under both conditions; recovery over each condition's own first-pass-wrong trajectories (*n* in parentheses, not a shared denominator); differences are RF⁻ minus RS, with 95% bootstrap intervals over problems. Lower block: only the problems first-pass wrong under both conditions (*n*_{both}), which is the actual test; "RS only" and "RF⁻ only" count problems recovered under one condition and not the other, with the exact McNemar *p* for the two counts. The two blocks have different denominators and must not be read across. Recovery conditions on a post-treatment variable and is descriptive rather than causal; no cell recovers before its first evaluator verdict.

Table S3: The two gemma-4-31B-it cells of the role-free condition with feedback, run twice, against the frozen role-structured cell.

	first pass	recovery	paired recovery	final	paired final
<i>Omni-MATH</i>					
role-structured (frozen)	0.802	70/316 = 0.222	–	0.846	–
role-free, with feedback, run 1	0.802	193/317 = 0.609	89 vs 4, <i>p</i> = 6.2×10^{-22}	0.921	137 vs 17, <i>p</i> = 1.7×10^{-24}
role-free, with feedback, run 2	0.811	177/302 = 0.586	95 vs 7, <i>p</i> = 7.8×10^{-21}	0.922	139 vs 18, <i>p</i> = 2.4×10^{-24}
<i>SciBench</i>					
role-structured (frozen)	0.794	30/118 = 0.254	–	0.847	–
role-free, with feedback, run 1	0.796	49/117 = 0.419	21 vs 7, <i>p</i> = 0.013	0.880	31 vs 12, <i>p</i> = 0.0054
role-free, with feedback, run 2	0.784	54/124 = 0.435	19 vs 8, <i>p</i> = 0.052	0.878	29 vs 11, <i>p</i> = 0.0064

Both runs use byte-identical runner and prompt code; the main text reports run 1. First pass: whether a trajectory's first submitted answer is correct. Recovery: first-pass-wrong trajectories that finish correct, over each condition's own first-pass-wrong set (count / denominator). Final: accuracy at the end over all problems, never-graded trajectories counted as incorrect. The paired columns compare each role-free run with the role-structured run on the problems first-pass wrong under both (recovery) or on all problems (final): problems recovered, or finally correct, only without roles versus only with them, with the exact McNemar *p* for the two counts.

Recovery tracks exposure. Figure S2 shows the mechanism directly. In every cell the RS team's share of trajectories still receiving verdicts falls steeply after the first one, while the RF team's falls slowly, and the cumulative recovery curves follow the exposure curves. Per verdict received, neither team is consistently ahead: on the paired problems, pooled over the five cells, the share whose second verdict is a PASS is 0.267 for RS and 0.224 for RF, and at the third and fourth verdicts 0.083 against 0.127 and 0.105 against 0.089. Recoveries per verdict after the first are higher with roles in every cell (Table S4), but that ratio compares different trajectories, since most of the RF team's later verdicts are further attempts on problems it has already failed more than once; the like-for-like comparison is the verdict-matched one.

How far the verdict-matched comparison can be pushed. Matching on verdicts is coarse. When the RS team received a single verdict, because it never submitted and was graded at the end or because it submitted once and stopped, the RF trajectory cut to one verdict

cannot recover either, and the pair ties at zero by construction. These forced ties are most of the gemma-4-31B-it pairs and nearly all of the Llama-3.3-70B-Instruct pairs (Table S5), so the pooled interval of the main paper is narrower than the information behind it. The cut is also one-sided, since the RS count stops at that team's own passing verdict, which can only favor it. Table S5 and Figure S3a therefore give the contrast under six definitions: the problems with at least two RS verdicts, both trajectories cut at the smaller count, and the scoring defect recoded, each with and without Llama-3.3-70B-Instruct. No variant puts the RF team significantly ahead: the two symmetric cuts favor it by 0.005 and 0.010 with intervals that include zero, the informative problems allow up to +0.065 in favor of roles (+0.073 with the recoding), and with the recoding on all problems the RS team leads by +0.029 with an interval that excludes zero. The defensible reading is the main paper's: matched on verdicts the RF advantage disappears, and any

Table S4: Where the role-structured Reviewer declines, per cell, and what each team’s allocation of the budget buys.

actor, domain	RS declining cycles			clause	RS never	free-cycle approval		by 2nd verdict		per later verdict		approvals	
	before	FAIL	free	share	submits	RS	RF	RS	RF	RS	RF	RS	RF
gemma-4-31B-it, Omni-MATH	2.24	0.81	1.20	0.19	84/316	0.25	0.96	0.206	0.438	0.41	0.28	1.27	3.20
gpt-oss-120b, Omni-MATH	1.33	1.17	0.71	0.37	51/463	0.53	0.68	0.246	0.296	0.27	0.22	2.10	3.53
Llama-3.3-70B-Instruct, Omni-MATH	4.49	0.25	0.88	0.04	205/276	0.01	0.82	0.014	0.092	0.25	0.08	0.32	2.71
gemma-4-31B-it, SciBench	0.55	1.38	1.48	0.40	3/118	0.35	0.99	0.237	0.239	0.26	0.16	1.95	3.62
gpt-oss-120b, SciBench	0.41	1.53	0.60	0.60	1/124	0.67	0.78	0.258	0.317	0.19	0.16	2.68	3.95

RS: role-structured; RF: role-free with the evaluator’s feedback shown. Every column is over first-pass-wrong trajectories. A cycle is one pass of the three agents; it ends in a submission to the evaluator when the third agent approves and in a decline otherwise. RS declining cycles: per trajectory, split into those before the first submission (no feedback yet), those in a cycle that received a FAIL verdict (where the RS clause forbids approval) and the remaining “free” cycles; clause share is the FAIL column as a fraction of all three. RS never submits: trajectories with no approval in six cycles, graded once at the end. Free-cycle approval: the share of free cycles ending in approval. By 2nd verdict: the share recovered with at most one round of feedback. Per later verdict: recoveries per verdict after the first, a ratio over different trajectories in the two conditions (the verdict-matched comparison of the main paper’s Table 2 is the like-for-like one). Approvals: submissions per trajectory.

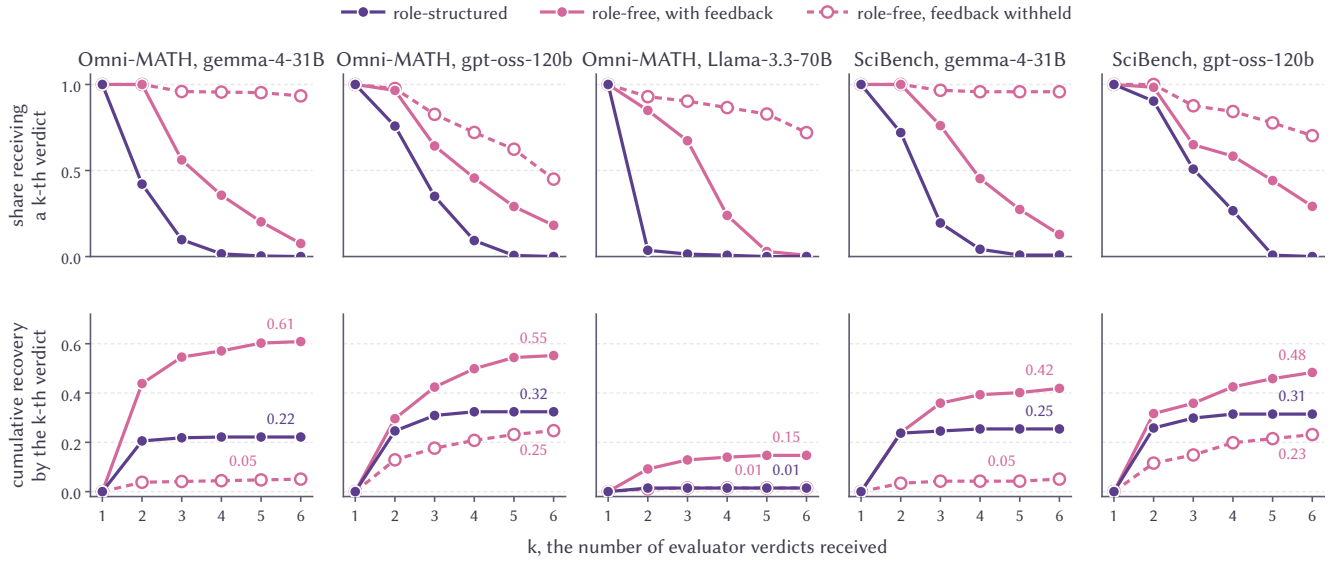


Figure S2: Recovery follows exposure to the evaluator. Top: the share of first-pass-wrong trajectories that receive a k -th evaluator verdict. Bottom: the cumulative share recovered by the k -th verdict, with the final value labeled. One panel per cell (actor on benchmark): role-structured (purple), role-free with feedback (rose) and role-free with the feedback withheld (hollow, dashed). The role-structured curve flattens where its trajectories stop receiving verdicts, not where they stop using them.

per-verdict advantage of roles is small. Matching equalizes exposure to the evaluator, not calls; the RS team spends more cycles per verdict.

Across the freeze. The frozen RS cell and the RF team were run on opposite sides of the freeze, so we repeat the comparison against the post-freeze RS re-run of gemma-4-31B-it on 400 Omni-MATH problems. On the 65 problems both first answer wrongly, the RF team leads 22 to 3 unmatched ($p = 1.6 \times 10^{-4}$), but the RS team leads 4 to 1 verdict-matched ($p = 0.38$): the same pattern as in the frozen comparison.

Removing the gate alone. The forced-submission run tests the gate directly. It uses the RS prompts and runner unchanged except for one step: after the Reviewer’s turn, the pipeline submits the team’s current answer to the evaluator whatever route the Reviewer

chose, and the verdict and hint reach the agents through the same role-specific slots as in the RS condition. An offline test in the supplementary material (no model calls) checks that the prompts are byte-identical to the RS ones up to the first verdict and that the evaluator is called in every cycle. It was run once, after the freeze, for gemma-4-31B-it on the 400 Omni-MATH problems of the RS re-run, with the same server, budget and evaluator. Table S6 compares it with that re-run, the frozen RS cell and the RF team, restricted to the same problems. Recovery rises to the RF team’s level while first-pass accuracy does not move: once the Reviewer’s approvals no longer decide anything, the role prompts recover as much as the generic ones.

Table S5: Robustness of the verdict-matched recovery contrast: forced ties, six definitions of the contrast, and per-verdict pass rates.

cell	paired n	forced ties	RS never submitted	problems left	held-answer recodes (RS / RF)
gemma-4-31B-it, Omni-MATH	236	141	66	95	12 / 3
gpt-oss-120b, Omni-MATH	319	84	36	235	4 / 0
Llama-3.3-70B-Instruct, Omni-MATH	250	242	187	8	2 / 1
gemma-4-31B-it, SciBench	100	29	2	71	2 / 0
gpt-oss-120b, SciBench	93	11	1	82	0 / 0

definition	all five cells			without Llama-3.3	
	n	difference [95% interval]	equivalent	n	difference [95% interval]
all paired problems (as in the main paper’s Table 2)	998	+0.009 [−0.014, +0.033]	yes	748	+0.011 [−0.020, +0.043]
problems with two or more role-structured verdicts	491	+0.018 [−0.029, +0.065]	no	483	+0.017 [−0.031, +0.064]
both cut at the smaller verdict count	998	−0.005 [−0.029, +0.019]	yes	748	−0.008 [−0.040, +0.024]
both cut, two or more verdicts	491	−0.010 [−0.059, +0.039]	no	483	−0.012 [−0.062, +0.037]
scoring defect recoded	998	+0.029 [+0.004, +0.054]	no	748	+0.035 [+0.003, +0.067]
recoded, two or more verdicts	491	+0.026 [−0.020, +0.073]	no	483	+0.025 [−0.023, +0.072]

per-verdict pass rate	role-structured	role-free with feedback
verdict 2	131/491 = 0.267	212/946 = 0.224
verdict 3	19/228 = 0.083	89/700 = 0.127
verdict 4	8/76 = 0.105	39/439 = 0.089

RS: role-structured; RF: role-free with the evaluator’s feedback shown; a cell is one actor on one benchmark. The contrast takes the problems both teams first answer wrongly and cuts the RF trajectory after as many evaluator verdicts as the RS trajectory received on that problem. Where RS received a single verdict, the cut RF trajectory cannot recover either, so the pair ties at zero by construction. Upper block: such forced ties per cell, how many of them never submitted, the problems left, and the trajectories whose held answer matched the key but were graded wrong (the scoring defect of the main paper’s Limitations). Middle block: the pooled difference, RS minus RF, stratified by cell with 95% bootstrap intervals over problems, under six definitions, with and without Llama-3.3-70B-Instruct; equivalent means the 90% interval lies inside ± 0.05 . Recoding counts an RS trajectory with a correct held answer as recovered, and an RF one only if it was not cut. Lower block: the share of trajectories whose k -th verdict is PASS among those whose earlier verdicts all failed, pooled over the five cells.

Table S6: Removing the submission gate: gemma-4-31B-it on the same 400 Omni-MATH problems in every row.

condition	first pass	recovery	final accuracy	verdicts	Reviewer approvals
role-structured, every cycle submitted	0.768	57/93 = 0.613	0.910	4.20	0.98
role-structured, re-run after the freeze	0.773	18/91 = 0.198	0.818	1.44	1.13
role-structured, frozen snapshot	0.795	12/82 = 0.146	0.825	1.41	1.15
role-free with feedback	0.795	45/82 = 0.549	0.908	3.34	3.34

pair	recovery (both first wrong)			final accuracy	
	n	first / second only	p	first / second only	p
every cycle submitted vs. re-run	72	26 / 2	3.0×10^{-6}	42 / 5	2.5×10^{-8}
every cycle submitted vs. role-free with feedback	70	7 / 7	1	8 / 7	1
re-run vs. role-free with feedback	65	3 / 22	1.6×10^{-4}	6 / 42	1.0×10^{-7}

Role-structured: Planner, Executor and Reviewer prompts, in the frozen snapshot and in a re-run after the freeze; role-free with feedback: one generic prompt for all three agents, with the evaluator’s feedback shown. The forced-submission run (“every cycle submitted”) uses the role-structured prompts byte by byte, but the pipeline submits the team’s answer in every cycle whatever the Reviewer decides; its approvals are logged but do not decide submission. First pass: the share of trajectories whose first submitted answer is correct. Recovery: first-pass-wrong trajectories that finish correct, over each row’s own first-pass-wrong set (count / denominator). Final accuracy: over all problems, never-graded trajectories counted as incorrect. Verdicts and Reviewer approvals: per first-pass-wrong trajectory, including the first. The lower block pairs two conditions on the problems both first answer wrongly (recovery) and on all problems (final accuracy): problems solved only by the first / only by the second, with the exact McNemar p for the two counts.

D The second domain (R4)

Table S7 gives the SciBench feedback-withheld comparison in full: the frozen RS and RF[−] cells with their denominators, for the two actors run there. The main comparison, with feedback, is in the main paper’s Figure 2 and Table 2.

What was and was not changed. The prompts are not byte-identical to Omni-MATH’s, and the prompt files in the supplementary material make the difference checkable. Of the 16 prompts the two domains share, 11 are identical and 5 differ: four of the six RS prompts (the Planner, Executor and Reviewer preambles and the Reviewer’s routing rules) and the evaluator’s preamble, which must ask for scientific rather than mathematical equivalence.

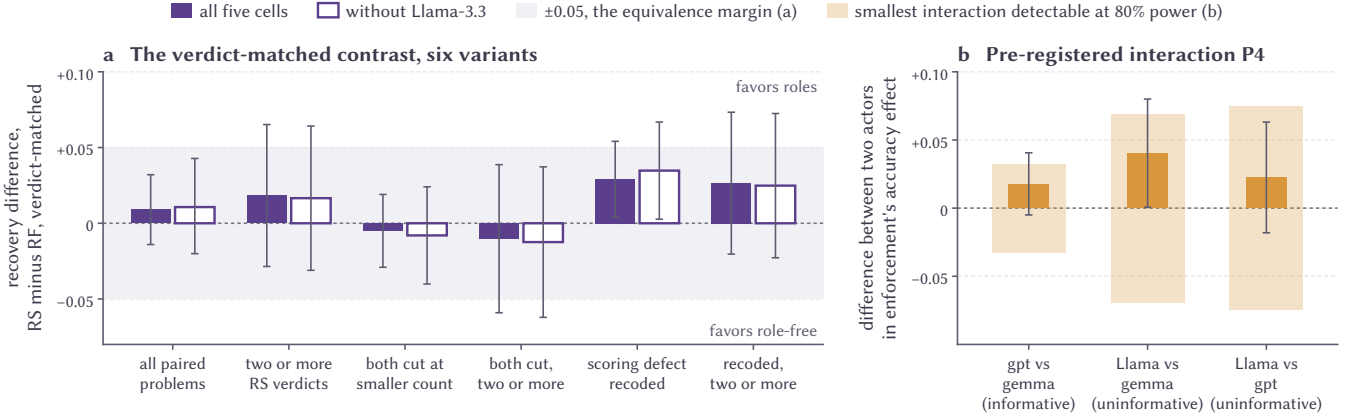


Figure S3: Two robustness checks. (a) The verdict-matched recovery difference, role-structured minus role-free with feedback, pooled over the five cells (filled) and without Llama-3.3-70B-Instruct (hollow), under the six definitions of Table S5, with 95% intervals; the band marks the ± 0.05 equivalence margin. (b) The pre-registered interaction P4 (Section F): each difference between two actors in enforcement's effect on final accuracy, with its 95% interval; the shaded bar behind it spans the smallest interaction detectable at 80% power. Where that bar is wider than any plausible effect, the null is uninformative.

Table S7: The SciBench benchmark: first pass and recovery with roles and without them (feedback withheld), two actors, frozen separately.

actor	first pass (paired, all problems)					recovery (paired on first-pass wrong under both)						
	n	RS	RF [−]	diff.	exact p	n_{both}	RS	RF [−]	diff.	RS only	RF [−] only	exact p
gemma-4-31B-it	574	0.794	0.794	+0.0000	1	99	0.212	0.020	−0.1919	21	2	6.60×10^{-5}
gpt-oss-120b	574	0.784	0.789	+0.0052	0.784	96	0.219	0.135	−0.0833	17	9	0.169

The complete 574-problem text-only slice of free-response college science, 2,296 trajectories. RS: role-structured; RF⁻: role-free with the evaluator's feedback withheld; the contrast with feedback shown is in the main paper's Table 2. First pass is paired over all 574 problems, with the exact McNemar *p* for the problems right under only one condition. Recovery is paired over only the problems first-pass wrong under both conditions (*n*_{both}); "RS only" and "RF⁻ only" count problems recovered under one condition and not the other. Differences are RF⁻ minus RS under the pre-registered scoring (never-graded trajectories count as incorrect). gpt-oss-120b's recovery cell is underpowered at its *n*_{both}; its estimate points the same way as on Omni-MATH. Llama-3.3-70B-Instruct was not run in this domain.

Both RF prompts are identical across domains, as are the six SE prompts (enforcement was not run on SciBench). The asymmetry deserves stating plainly: the RS prompts, whose team recovers less than the RF team, are the ones that were rewritten, while the RF prompts ran identical text in both domains. The rewrite is domain wording ("algebraic manipulation or numerical calculation" where Omni-MATH said "symbolic manipulation or arithmetic"), and the structural clauses are unchanged; a reader who suspects the adaptation is doing the work can check the files. Topology, budget, decoding, retry policy and the evaluator protocol are unchanged, so this is a replication of the mechanism under a matched protocol, not a rerun of identical prompts.

Why the answer does not lock here. On Omni-MATH the gemma-4-31B-it RS team often keeps the same answer across cycles (a single distinct answer in 28.8% of its failed trajectories); on SciBench it does not (7.6%, against 8.5% for the RF team). The RS team on SciBench also almost always submits at least once (Table S4), so it sees more feedback, and by the second verdict the two teams

have recovered equally often. A looser gate is the simplest common explanation of both observations, though we did not test it.

E Structural enforcement (R4)

The main paper reports that enforcement loosens the gate and raises recovery, but that attrition keeps the gain from reaching end-to-end accuracy, and that the apparent accuracy benefit under the as-graded scoring is survivorship. Table S8 and Figure S4 are the evidence for both halves: the manipulation check, the one-sided attrition, and the same accuracy difference under both scorings.

Why the columns do not compose. The enforced recovery and first-pass rates do not combine into the accuracy column, and cannot: trajectories that never receive a first verdict have no first pass, and those that die after one stay in the recovery denominator, so the three quantities do not share a denominator. The pre-registration fixed in advance that unparseable trajectories count as incorrect rather than being dropped, and that rule is what makes the as-graded reading survivorship rather than a finding.

Table S8: Structural enforcement against the role-structured baseline on the Omni-MATH benchmark: the manipulation check, attrition, and final accuracy under both scorings.

	Planner authors	locked answer	recovery		final accuracy, SE – RS	
actor	answer (RF [−])	(RF [−] / RS)	(RS)	attrition	pre-registered	as graded
gemma-4-31B-it	0.000 (0.986)	0.119 (0.845/0.288)	0.314 (0.222)	8.0%	−0.0225 [−0.0387,−0.0063]	+0.0190 [+0.0054,+0.0333]
gpt-oss-120b	0.000 (0.916)	0.092 (0.208/0.052)	0.395 (0.324)	9.9%	−0.0050 [−0.0219,+0.0112]	+0.0361 [+0.0215,+0.0507]
Llama-3.3-70B-Instruct	0.000 (0.958)	0.072 (0.806/0.750)	0.139 (0.014)	17.0%	+0.0175 [−0.0200,+0.0550]	+0.0453 [+0.0030,+0.0876]

SE: structurally enforced; RS: role-structured; RF[−]: role-free with the evaluator’s feedback withheld. Planner authors answer: the share of Planner messages that state a final answer, under SE and (in parentheses) under RF[−]; SE drives it to zero in all three actors, so the intervention engaged (the manipulation check). Locked answer: the share of first-pass-wrong trajectories whose answer string never changed, under SE and (in parentheses) RF[−] / RS. Recovery: the share of first-pass-wrong trajectories that finish correct, under SE and (in parentheses) RS, on the denominator of Table S2. Attrition: the share of SE trajectories that never finish because a malformed output exhausted its regeneration budget. Final accuracy: the difference over all problems with 95% bootstrap intervals, under the pre-registered scoring (never-graded trajectories count as incorrect) and as graded (they are dropped); the two columns are different populations and the attrition column is why they disagree. Attrition elsewhere is negligible: 0.0–0.8% with roles, 0.0–2.5% without. The as-graded column turns every sign positive; the pre-registration fixed in advance that unparseable trajectories count incorrect, and **the benefit appears only after conditioning on successful parsing**.

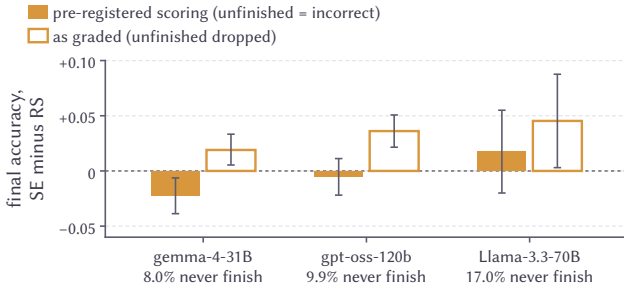


Figure S4: Enforcement’s effect on final accuracy under the two scorings. Enforced minus role-structured final accuracy per actor with 95% intervals, under the pre-registered scoring (unfinished trajectories count as incorrect; filled) and as graded (they are dropped; hollow). The tick labels give the share of enforced trajectories that never finish, which is what separates the two bars. The sign flips between the scorings for two of the three actors, and the as-graded gains are ordered like the attrition.

The gate loosens, and brittleness follows the same order. Per failed trajectory, the SE Reviewer approves 2.02, 2.33 and 1.84 times for gemma-4-31B-it, gpt-oss-120b and Llama-3.3-70B-Instruct, against 1.27, 2.10 and 0.32 under RS prompts. Enforcement logs 838, 1,183 and 395 role-boundary rejections, that is 0.52, 0.74 and 0.99 per trajectory, in the same order as the attrition (8.0%, 9.9% and 17.0%): the actor that breaks the schema most often is the one that loses the most trajectories. For Llama-3.3-70B-Instruct, whose RS team rarely submits, enforced recovery comes close to the RF team’s (0.139 against 0.147); on the 219 problems both first answer wrongly, 9 are recovered only under enforcement and 17 only by the RF team ($p = 0.17$).

The context-restored control. Enforcement also hides the shared history from the Executor, so part of its brittleness may be lost context rather than the typed interface. Restoring the shared history at fixed enforcement (gemma-4-31B-it and gpt-oss-120b, 400

problems each; attrition 4.5% and 9.8%) changes final accuracy relative to enforcement by +0.0450 [+0.0125, +0.0775] ($p = 0.0079$) for gemma-4-31B-it and by −0.0125 [−0.0475, +0.0200] ($p = 0.56$) for gpt-oss-120b under the pre-registered scoring, and by +0.0196 [−0.0028, +0.0419] ($p = 0.14$) and −0.0089 [−0.0357, +0.0149] ($p = 0.65$) as graded. For gemma-4-31B-it the gain is significant only under the scoring that penalizes attrition, so it comes mostly from fewer trajectories dying; the control is partial, because the attrition it removes is one-sided.

An early campaign stopped for a truncation confound. The main run was gated on a smoke test requiring non-zero boundary rejections with reasons logged, answer-bearing calls equal across conditions, and a pinned backend. An early campaign was stopped after 121 trajectories when 74% of enforcement rejections proved to be the model cut off at the token limit mid-JSON rather than violating a role boundary. Truncation was not condition-specific but its cost was: a prose condition tolerates a truncated message where the enforced one rejects unparseable JSON, so left alone it would have produced a convincing “enforcement lowers accuracy” artifact. The main run uses twice that token limit, and the early rows were never pooled.

Accumulated context as a competing explanation. The RS team’s recovery advantage over the RF[−] team could in principle come from the longer history it accumulates over its internal cycles rather than from the feedback it sees. Table S10 stratifies recovery by accumulated context. The comparison is directional only: quartile cut points are computed over both conditions pooled, and accumulated context is nearly a function of condition, because the RF[−] team stops changing its answer, so the strata are not balanced and context cannot be separated from condition. It is reported because it does not behave uniformly. The gap survives in the lower quartiles, but in gpt-oss-120b’s top quartile the ordering reverses, as it does on a handful of events in Llama-3.3-70B-Instruct’s third, so more context is not simply more recovery.

F The pre-registered endpoints in full

The main paper reports that the pre-registered primary endpoint P4, whether enforcement’s effect on accuracy differs by actor, is null, and that the P1a manipulation objective failed. Table S9 is

Table S9: The five pre-registered endpoints and what each returned. P4, the primary, is null before and after Holm correction.

endpoint	disposition					
P1 intervention → fidelity (also the manipulation check)	failed as written; re-designated. The interface forbids an <i>answer</i> field, not computation. Re-scoped to answer authorship, which it drives to 0.000 in all three actors (Table S8). Reported as a second dissociation, not as a pass.					
P2 actor × interv. on fidelity	not estimable across the panel: the fidelity clauses are condition-specific, so no common scale exists. Reported as unresolved rather than substituted.					
P3 intervention → accuracy (enforced – role-structured), Holm across three:						
	raw <i>p</i>	Holm <i>p</i>	verdict			
gemma-4-31B-it	0.0078	0.023	reject			
gpt-oss-120b	0.60	0.85	retain			
Llama-3.3-70B-Instruct	0.43	0.85	retain			
P4 actor × intervention on accuracy (the pre-registered primary), Holm across three:						
	difference-in-diff. [CI]	raw <i>p</i>	Holm <i>p</i>	MDE _{80%}	null is	
gpt-oss-120b – gemma-4-31B-it	+0.0175 [-0.0050,+0.0406]	0.145	0.29	0.0325	informative	
Llama-3.3-70B-Instruct – gemma-4-31B-it	+0.0400 [+0.0006,+0.0800]	0.0553	0.166	0.0693	uninformative	
Llama-3.3-70B-Instruct – gpt-oss-120b	+0.0225 [-0.0181,+0.0631]	0.286	0.29	0.0748	uninformative	
P5 Δfidelity vs Δoutcome	association only, pre-declared as non-causal: both sides are outcomes of the same intervention. No <i>p</i> -value is reported and none is corrected.					

The endpoints were fixed in writing before any intervention data existed. The intervention is structural enforcement against the role-structured baseline, per actor. Fidelity: whether the Planner abstains from computing (P1a). Accuracy: final accuracy over all problems, never-graded trajectories counted as incorrect. P3: exact McNemar test per actor, with the raw *p*, the *p* after Holm’s step-down correction across the three actors, and the verdict at $\alpha = 0.05$. P4: each difference-in-differences is enforcement’s effect in one actor minus its effect in another, with its 95% bootstrap interval over problems, the raw and Holm-corrected *p*, and MDE_{80%}, the smallest interaction detectable with 80% power at the realized sample; a null is informative only where the MDE is below a plausible effect. P2 and P5 carry no *p* value, so the testable contrasts are P3’s three and P4’s three; Holm across all six changes no verdict. P4’s intervals and *p* values come from two separately seeded bootstrap runs, so an interval can exclude zero while *p* is just above 0.05; the MDE is computed on problems shared by both actors while each difference-in-differences uses each actor’s own problems, so neither can be derived from the other. **Only one of the three P4 nulls is informative.** The largest per-actor main effect is small enough that a true interaction is plausibly ≤ 0.040 ; only the gpt-oss/gemma-4 contrast can see an effect that size. The two Llama-3.3 contrasts cannot detect an interaction of any plausible magnitude and are uninformative rather than evidence of absence.

the complete endpoint table: all five endpoints as written before any intervention data existed, what each one returned, and the Holm-corrected verdict.

Two things in it should be read rather than skimmed. First, only one of the three P4 contrasts is an informative null (Figure S3b): the largest per-actor main effect is small enough that a true interaction is plausibly at most 0.040, only the gpt-oss-120b against gemma-4-31B-it contrast can detect an effect of that size, and the two contrasts involving Llama-3.3-70B-Instruct cannot detect an interaction of any plausible magnitude at their sample size. They are uninformative rather than evidence of absence. Second, the contrast pre-registered as secondary and exploratory was RS against RF[−]; the contrast this paper is organized around, RS against RF, and the forced-submission run are outside the pre-registration; the RF condition’s design and primary analysis were committed in writing before its data existed, but its results are exploratory, and the main paper labels them so.

G The single-agent comparator

Table S11 is the single-agent reference under the same call cap. The main paper uses it as a guard against reading “collapse costs nothing” as “the system was one agent all along”, and reports both directions. The result cuts both ways: a collapsed gpt-oss-120b team beats its own single agent, so three agents emitting one answer are not functionally one agent, while gemma-4-31B-it’s team is significantly worse than its own single agent in both conditions. Neither “multi-agent systems are just single agents” nor “more

agents help” survives it. The comparator comes from an earlier, separately collected corpus whose two actor arms were produced in separate runs, so each direction within an actor is a sound guard and the gap between actors is not.

H AI use statement

In this work, we used generative AI tools for three purposes: to edit the manuscript for readability and polish its wording; to search for and identify relevant literature and to format references; and to assist with data processing and analysis, including drafting and refining the code for these tasks and for the figures. All AI-generated text and code were reviewed by the authors, and LLM-generated code was verified and tested for correctness by the authors. We take responsibility for the final content of this work, including text, claims or artifacts produced with the aid of generative AI.

Table S10: Recovery by accumulated-context quartile, with roles and without them (feedback withheld), on the Omni-MATH benchmark; directional only.

context quartile	RS		RF [−]		difference
	recovery	<i>n</i>	recovery	<i>n</i>	
<i>gemma-4-31B-it</i>					
Q1 (least context)	0.662	65	0.151	93	+0.511
Q2	0.280	50	0.009	108	+0.271
Q3	0.047	85	0.014	73	+0.033
Q4 (most)	0.078	116	0.000	42	+0.078
<i>gpt-oss-120b</i>					
Q1 (least context)	0.923	13	0.258	198	+0.665
Q2	0.656	128	0.293	82	+0.363
Q3	0.262	164	0.191	47	+0.071
Q4 (most)	0.070	158	0.189	53	−0.119
<i>Llama-3.3-70B-Instruct</i>					
Q1 (least context)	0.100	40	0.031	96	+0.069
Q2	0.000	86	0.000	50	+0.000
Q3	0.000	103	0.030	33	−0.030
Q4 (most)	0.000	47	0.000	89	+0.000

RS: role-structured; RF⁻: role-free with the evaluator’s feedback withheld. Recovery: the share of first-pass-wrong trajectories that finish correct, with *n* the number of such trajectories in the quartile; the difference is RS minus RF⁻. Context is the total character count of all agent messages in the trajectory; quartile cut points are computed once per actor over both conditions pooled. Because accumulated context is nearly a function of condition (the RF⁻ team writes less because it stops changing its answer), the strata are not balanced and the comparison cannot separate context from condition.

Table S11: Each three-agent team against a single agent of the same actor under the same call cap, on the Omni-MATH benchmark.

organization	team	single agent	difference
<i>gemma-4-31B-it</i>			
role-structured	0.8462	0.8688	−0.0225
	[−0.0400, −0.0056], $p = 0.0122$, $n = 1600$		
role-free	0.8125	0.8688	−0.0563
	[−0.0737, −0.0394], $p = 1.22 \times 10^{-10}$, $n = 1600$		
<i>gpt-oss-120b</i>			
role-structured	0.8031	0.7900	+0.0131
	[−0.0044, +0.0300], $p = 0.152$, $n = 1600$		
role-free	0.8194	0.7900	+0.0294
	[+0.0125, +0.0456], $p = 0.000678$, $n = 1600$		
<i>Realized call budget</i>			
actor	protocol	evaluator	actor
<i>gemma-4-31B-it</i>	single agent	1.66	3.16
<i>gemma-4-31B-it</i>	role-structured team	1.11	6.76
<i>gemma-4-31B-it</i>	role-free team	1.95	5.87
<i>gpt-oss-120b</i>	single agent	1.69	3.40
<i>gpt-oss-120b</i>	role-structured team	1.35	7.54
<i>gpt-oss-120b</i>	role-free team	1.85	6.25

Final accuracy over all problems (*n*) of the role-structured (RS) and role-free, feedback-withheld (RF⁻) teams against a single agent of the same actor solving the same problems under the same cap on model calls; the difference is team minus single agent, with its 95% bootstrap interval over problems and the exact McNemar *p*. The single-agent trajectories come from an earlier, separately collected trace corpus, joined by problem, and its two actor arms were produced in separate runs: each direction within an actor is a sound guard, the gap between actors is not. Realized call budget: calls per problem to the evaluator and to the actor.