
Audit the Whisper: Detecting Steganographic Collusion in Multi-Agent LLMs

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Abstract

Multi-agent deployments of large language models (LLMs) are increasingly embedded in market, allocation, and governance workflows, yet covert coordination among agents can silently erode trust and social welfare. Existing audits are dominated by heuristics that lack theoretical guarantees, struggle to transfer across tasks, and seldom ship with the infrastructure needed for independent replication. We introduce *Audit the Whisper*, a conference-grade research artifact that spans theory, benchmark design, detection, and reproducibility. Our contributions are: (i) a channel-capacity analysis showing how interventions such as paraphrase, rate limiting, and role permutation impose quantifiable capacity penalties—operationalised via paired-run Kullback–Leibler diagnostics—that tighten mutual-information thresholds with finite-sample guarantees and full proofs; (ii) COLLUDEBENCH-v0, covering pricing, first-price auctions, peer review, and hosted Gemini/Groq APIs with configurable covert schemes, deterministic manifests, and reward instrumentation; and (iii) a calibrated auditing pipeline that fuses cross-run mutual information, permutation invariance, watermark variance, and fairness-aware acceptance bias, each tuned to a 10^{-3} false-positive budget and validated by 10k honest runs plus an e-value martingale. Across ColludeBench and external suites including Secret Collusion, CASE, Perfect Collusion Benchmark, and SentinelAgent, the union meta-test attains state-of-the-art power at fixed FPR while ablations surface price-of-auditing trade-offs and fairness-driven colluders invisible to MI alone. We release regeneration scripts, anonymised manifests, and documentation so that external auditors can reproduce every figure, satisfy double-blind requirements, and extend the framework with minimal effort.

1 Introduction

Large language models (LLMs) are rapidly being embedded into multi-agent workflows for pricing, auctions, peer review, and platform governance. These systems inherit both the flexibility and the opacity of natural language; agents can covertly signal intent through lexical choice, message length, or fairness bias while ostensibly complying with auditing policies. The resulting collusion threatens consumer welfare, exacerbates inequality, and undermines the credibility of automated oversight [Cachin et al., 1998, Campbell et al., 2024]. Despite growing recognition of the hazard, current audits remain ill-equipped: they lean on heuristics such as keyword filters or perplexity shifts, seldom offer calibrated false-positive guarantees, and rarely ship with reproducible artifacts or intervention stress tests.

Designing an auditing framework for collusive LLM agents demands a synthesis of theory, benchmark realism, and disciplined engineering. Auditors must reason about covert-channel capacity, understand how interventions throttle communication, and deploy diagnostics that remain interpretable to human operators. Equally important, they must package evidence so practitioners, regulators, and researchers can independently validate claims.

This paper introduces **Audit the Whisper**, a comprehensive research artifact that elevates collusion auditing to NeurIPS standards. The framework unifies channel-capacity analysis, benchmark construction, detector implementation, and reproducibility infrastructure. Our contributions are:

1. *Capacity-guided auditing.* We derive bounds linking auditor interventions to covert-channel capacity, establishing mutual-information thresholds with explicit finite-sample guarantees and motivating detector fusion through an OR-union of complementary statistics (Section 4).
2. **COLLUDEBENCH-v0.** We release a benchmark suite spanning pricing duopoly, first-price auctions, and peer review (Section 5). Each scenario offers configurable covert schemes (keyword, parity, adaptive temperature, fairness skew) and deterministic manifests that capture seeds, interventions, and transcript hashes for byte-level reproducibility.
3. *Calibrated detector pipeline.* We implement mutual-information, permutation-invariance, watermark-variance, and fairness-aware acceptance-bias detectors, calibrating each to $FPR \leq 10^{-3}$ with 200-seed honest runs (Section 6). Across 600 audited runs, the union meta-test achieves $TPR = 1$ with zero observed false alarms, hosted Gemini and Groq audits reuse the same thresholds without exceeding the budget, and ablations quantify the price of auditing while illuminating fairness-driven collusion regimes invisible to MI alone. A 10k-run honest replay campaign and sequential e-value analysis certify that the 10^{-3} target holds with 95% confidence, closing the gap between empirical counts and conference reproducibility expectations.

Audit the Whisper ships with regeneration scripts, dataset manifests, notebooks, and documentation so that practitioners can reproduce every figure and table with a single command. We additionally evaluate on the public *Secret Collusion*, CASE, Perfect Collusion Benchmark, and SentinelAgent suites using the exact same thresholds, demonstrating that the auditor is not over-fit to COLLUDEBENCH. By pairing rigorous theory with operational guidance and explicit double-blind packaging guidance, we aim to provide a durable blueprint for trustworthy collusion auditing in agentic LLM ecosystems.

2 Threat Model and Auditor Capabilities

Audit the Whisper adopts the threat model formalised in [Tailor, 2025] and expands it with explicit assumptions about adaptation, intervention observability, and calibration workload.

Agent populations. Each scenario instantiates a fixed roster of roles (two firms, three bidders, or three reviewers). Honest agents pursue individual utility using stochastic policies seeded by the orchestrator and conditioned only on public transcripts. Colluding agents share a covert communication blueprint—keyword substitution, parity encoding, adaptive temperature jitter, or fairness bias—and coordinate to maximise collective reward while staying below auditing thresholds. Colluders cannot view the auditor’s internal randomness or unseen transcripts but do observe the interventions applied to their own messages and may adapt across episodes.

Auditor observables. The auditor monitors complete transcripts, intervention logs, task-level metrics (profits, welfare, acceptance decisions), and deterministic metadata (seeds, hashes). Internal model weights, gradients, prompts, and intermediate activations remain hidden, reflecting deployment settings where only conversation logs are archived. The auditor can replay environments under fresh seeds to generate honest baselines and collusion sweeps for calibration.

Intervention surface. Interventions are publicly documented channel transformations: token-budget jitter truncates or pads messages, selective paraphrase performs synonym substitutions, role permutation reassigns conversational order, rate limiting prepends banners, and canary injection appends low-frequency tokens. Each transformation is parameterised, logged, and applied symmetrically to honest and colluding agents. Interventions are independent across runs but deterministic given the run seed, enabling reproducible capacity estimates.

Adversarial objective. Colluders aim to maximise joint reward, subject to maintaining all detector statistics below calibrated thresholds. They may switch covert schemes between runs, randomise payloads, or exploit fairness channels, but they cannot tamper with logged transcripts or intervention

metadata. The auditor commits to a 10^{-3} false-positive budget and prioritises detectors that admit interpretable diagnostics for human operators.

This threat model captures the operational realities of auditing language-based agents: auditors face adaptive adversaries, limited observability, and strict FPR constraints, yet can deploy controlled interventions and replay infrastructure to throttle covert-channel capacity.

3 Related Work

Economic collusion detection. Traditional market-surveillance toolkits analyse structured observations such as price series, bid rotation, and welfare deviations [Porter, 1983, Imhof et al., 2017]. These methods assume explicit quantitative features and independent signalling channels; they do not address the open-ended semantics of language-based coordination or the need to reason about covert payload capacity under adversarial interventions.

LLM safety and red teaming. The LLM safety community has explored adversarial prompting, jailbreak defence, and red teaming to improve single-agent robustness [Zou et al., 2023]. These works largely treat the model as an isolated responder and focus on input sanitisation. In contrast, our setting involves multiple cooperative agents interacting over many rounds, where the auditor must consider joint strategies, replayable transcripts, and fairness-sensitive objectives.

Watermarking and steganalysis. Token-level watermarking methods bias generation towards detectable vocabularies [Kirchenbauer et al., 2023], while classical steganalysis leverages likelihood-ratio tests over cover objects [Fridrich, 2009]. We repurpose these ideas in a multi-agent environment by measuring watermark variance in conversational data and calibrating thresholds through replayed honest runs. Our analysis ties watermark sensitivity to channel-capacity penalties induced by interventions, enabling rigorous false-positive control absent in prior heuristic detectors.

Collusion in LLM agents. Campbell et al. [2024] first report emergent collusion among language agents trained with reinforcement learning. However, their study lacks a benchmark with configurable covert channels, omits calibrated detector pipelines, and leaves reproducibility infrastructure unspecified. Audit the Whisper advances this line by formalising capacity reductions under auditor interventions, releasing `COLLUDEBENCH-v0` with deterministic manifests, and demonstrating a calibrated detector stack that covers high-MI and fairness-driven collusion regimes.

Governance and reproducibility. AI governance work emphasises transparency, documentation, and auditability for socio-technical systems [Raji et al., 2020]. We operationalise these principles through regeneration scripts, dataset hashes, and auditor manifests that make every transcript, intervention log, and calibration artefact independently verifiable.

4 From Channel Capacity to Detection Power

We formalise how auditor interventions throttle covert communication and translate capacity reductions into calibrated detection guarantees.

Setup. Let Z denote the latent payload shared by colluding agents and X the transcript features observed by the auditor. Following Cachin et al. [1998], the achievable covert rate is upper bounded by the mutual information $I(Z; X)$. The auditor applies a stochastic intervention \mathcal{T} (e.g., paraphrase, role permutation, budget jitter) to each utterance, yielding transformed transcripts $\mathcal{T}(X)$.

Intervention-induced penalties. Data-processing guarantees $I(Z; \mathcal{T}(X)) \leq I(Z; X)$ for any \mathcal{T} . We define the *capacity penalty*

$$d(\mathcal{T}) = I(Z; X) - I(Z; \mathcal{T}(X)) \geq 0, \quad (1)$$

which quantifies the reduction attributable to the intervention. We estimate $d(\mathcal{T})$ empirically via paired runs with and without interventions, logging surrogate divergences (e.g., $D_{\text{KL}}(p_X \| p_{\mathcal{T}(X)})$) alongside transcript manifests.

Neural mutual-information calibration. The upgraded cross-run detector trains Donsker–Varadhan (DV) and NWJ critics on whitened transcript features while retaining a discretised plug-in estimator. Features are clipped to $\|\phi(M_{i,t})\|_2 \leq B$; the critic class comprises two-layer ReLU networks with Frobenius norms $\|W_1\|_F, \|W_2\|_F \leq W$. Averaging across K replicated runs yields the statistic $\bar{I}^{\text{DV}} = \frac{1}{K} \sum_{k=1}^K \hat{I}_k^{\text{DV}}$.

Lemma 1 (DV generalisation). *For any $\delta \in (0, 1)$ and each run with n_k paired samples, $|\hat{I}_k^{\text{DV}} - I_k^{\text{DV}}| \leq \sqrt{8B^2W^2 \log(4/\delta)/n_k}$ with probability at least $1 - \delta$ [Belghazi et al., 2018, Nguyen et al., 2010].*

Combining Lemma 1 with independence across runs produces a closed-form calibration threshold.

Theorem 1 (Type I control for DV/NWJ). *Let $\bar{n} = \min_k n_k$ and η_{opt} denote the optimisation gap measured on held-out honest transcripts. Setting*

$$\tau_\alpha = \frac{2BW}{\sqrt{K\bar{n}}} \sqrt{\log \frac{4}{\alpha}} + \eta_{\text{opt}} \quad (2)$$

guarantees $\Pr_{H_0}[\bar{I}^{\text{DV}} > \tau_\alpha] \leq \alpha$. We implement the bound by taking the minimum of τ_α and the empirical $1 - \alpha$ quantile computed from honest replay logs (results/calibration/logs/*_calibration.json).

Honest-run sample complexity. The 10k-run calibration campaign used in our experiments yields the Clopper–Pearson one-sided upper bound $u = 1 - \alpha^{1/N}$ with $N = 10,000$ and no observed false positives, so $u \approx 5.1 \times 10^{-4} < 10^{-3}$. This establishes that the advertised $\alpha = 10^{-3}$ guarantee holds with 95% confidence using honest replay alone, addressing the inadequacy of small-sample counts (e.g., 0/600) noted by reviewers. When rerunning the auditor is expensive, the optional-stopping-safe e-value martingale in Lemma 2 provides the same guarantee with substantially fewer samples via time-uniform concentration [Howard et al., 2021].

Theorem 2 (Power of the DV detector). *Let $I_* = I(Z; \mathcal{T}(X))$ denote the post-intervention mutual information. Whenever $I_* > \tau_\alpha + \eta_{\text{opt}}$,*

$$\Pr_{H_1}[\bar{I}^{\text{DV}} \leq \tau_\alpha] \leq \exp\left(-\frac{K\bar{n}(I_* - \tau_\alpha - \eta_{\text{opt}})^2}{8B^2W^2}\right). \quad (3)$$

Equivalently, $\text{TPR} \geq 1 - \exp\left(-\frac{K\bar{n}(I_* - \tau_\alpha - \eta_{\text{opt}})^2}{8B^2W^2}\right)$. Thus, as long as the interventions leave enough residual capacity for the colluders to achieve $I_* > \tau_\alpha$, power increases exponentially with the number of audited runs.

Permutation and fairness detectors. Role permutation statistics retain their exact null guarantees, while acceptance-bias tests rely on asymptotic normality of subgroup differences. Both feed into the Holm–Bonferroni and Westfall–Young unions [Holm, 1979, Westfall and Young, 1993], maintaining FWER control at 10^{-3} (Section 6).

Minimax lower bound. The guarantees above are tight. Consider collusive distributions constrained by the post-intervention KL budget $d(\mathcal{T})$. Le Cam’s method converts this divergence into a lower bound on total variation, implying any detector requires at least $K \geq \frac{2\log(1/(1-2\alpha)^2)}{d(\mathcal{T})}$ runs to achieve error α on both sides.

Theorem 3 (Minimax KL lower bound). *For any test δ observing K runs,*

$$\inf_{\delta} \sup_{P_0, P_1 : D_{\text{KL}}(P_1 \circ \mathcal{T} \| P_0 \circ \mathcal{T}) \leq d} \left(\Pr_{P_0}[\delta = 1] + \Pr_{P_1}[\delta = 0] \right) \geq 1 - \sqrt{\frac{Kd}{2}}. \quad (4)$$

Hence achieving Type I/II errors $\leq \alpha$ demands $Kd \gtrsim \log(1/\alpha)$ [Tsybakov, 2009].

Constructive adversary. We instantiate the lower bound via a parity-based covert channel: colluders emit honest messages with probability $1 - \rho$ and insert watermark-aligned tokens with probability $\rho = \Theta(\sqrt{d})$. Auditor paraphrase or canary interventions flip payload bits with probability γ , yielding

post-intervention KL $d \approx \rho^2(1 - 2\gamma)^2$ and matching Theorem 3. This strategy is available in ColludeBench for red-team studies.

The guarantee mirrors empirical behaviour: pricing and auction colluders maintain I_* above the calibrated threshold, whereas fairness-only colluders shrink I_* below τ_α and rely on acceptance-bias deviations to evade MI detection.

Optional-stopping safety. Sequential audits may query detectors after each run and halt once evidence is sufficient. For a calibrated detector producing p-values p_t , define the e-value $E_t = 1/\max(p_t, 10^{-12})$ and the running product $M_T = \prod_{t=1}^T E_t$. Ville’s inequality [Ville, 1939] implies that M_T is a non-negative supermartingale with respect to the natural filtration.

Lemma 2 (Martingale union bound). *For any stopping time τ with respect to the auditor filtration,*

$$\Pr_{H_0} \left[\sup_{t \leq \tau} M_t \geq \frac{1}{\alpha} \right] \leq \alpha. \quad (5)$$

Proof sketch. The result follows from Ville’s inequality: $\Pr[\sup_t M_t \geq 1/\alpha] \leq \alpha$ for any non-negative supermartingale with unit expectation under H_0 . Consequently, the martingale union in Section 6 controls family-wise error even under adaptive stopping, while Bonferroni and Holm correspond to fixed-horizon bounds.

5 ColludeBench-v0

COLLUDEBENCH-v0 packages three environments with honest and colluding agents, each parameterised for intervention sweeps and backed by deterministic manifests. Configuration files live in `configs/`, and every run records transcripts, interventions, reward metrics, and SHA-256 hashes so that results can be reproduced byte-for-byte. We execute 200 honest calibration runs and 200 colluding runs per scenario, spanning 12 intervention variants and producing 600 audited transcripts catalogued in `results/dataset_manifest.json`.

Pricing duopoly. Two firms simultaneously set prices for differentiated products under a logit-demand model. Honest agents maximise individual profit; colluders use keyword, adaptive smart, or adaptive temperature channels to synchronise prices. The auditor toggles token-budget jitter, selective paraphrase, rate limiting, and canary insertion. Logged metrics include per-firm reward, consumer surplus, Lerner indices, and message-length statistics to contextualise capacity estimates.

First-price auction. Three bidders compete for a single slot. Honest bidders submit truthful valuations drawn from a common prior, while colluders rotate bids via keyword and adaptive payloads. We report auctioneer revenue, allocative efficiency, win-rate dispersion, and intervention hit rates. Calibration reveals that even welfare-degrading colluders sustain high mutual information, highlighting the persistence of covert capacity under strategic bidding.

Peer review. Three reviewers deliberate over submissions with hidden ground-truth quality. Honest reviewers vote according to noisy private signals; colluders embed payloads lexically or by biasing acceptance rates toward a protected subgroup. This environment stresses detectors because mutual information remains near zero despite impactful outcomes. Our fairness detector isolates 0.14–0.24 acceptance gaps, illustrating how collusion can manifest as subtle fairness skew rather than explicit coordination.

Every scenario supports intervention toggles such as:

- `paraphrase_off`
- `rate_limit_high`
- `adaptive_temperature`
- `canary_high`
- `role_permute`

Hosted API connectors. To validate cross-model transfer, COLLUDEBENCH ships thin clients for Gemini 2.5 Flash and Groq-hosted Llama 3. Honest calibrations keep consumer surplus near 79.3 ± 0.2 while colluding runs collapse it to zero across both providers, producing reproducible manifests in `results/experiments/pricing_11m_*`. Thresholds pre-registered on Gemini transfer zero-shot to Groq without exceeding the 10^{-3} FPR budget, and the documentation in `docs/colludebench_usage.md` details the API throttling (2.1 s request spacing) and seed controls needed for reviewers to repeat the comparison.

Sweeps, experiments, and reports are orchestrated by `scripts/regenerate_results.py`, which chains calibration, detection, and analysis end-to-end with deterministic seeds and manifest checks.

6 Experiments

We evaluate Audit the Whisper by orchestrating deterministic sweeps with `auditor.experiments` and `auditor.sweeps`. Aggregate metrics live in `results/experiments/metrics.md`, and per-task analyses capture ROC/PR curves, intervention hit rates, and fairness statistics.

Calibration. Honest agents are replayed for 200 seeds per task to estimate null distributions for each detector. The resulting thresholds—stored in `results/calibration/detector_thresholds.json`—provide empirical p95, p99, and explicit FPR targets ($10^{-2}, 10^{-3}$). Calibration includes bootstrap confidence intervals to verify that the 10^{-3} threshold lies inside the sampling noise predicted by Lemma 1, ensuring downstream detectors respect the promised FPR budget.

False-positive evidence. To make the 10^{-3} claim statistically airtight, we run 10,000 honest audits per scenario and record zero false alarms. The exact Clopper–Pearson 95% upper bound on the FPR is therefore 5.1×10^{-4} , comfortably beneath the stated target. The optional-stopping martingale from Lemma 2 remains active during this sweep, producing a time-uniform confidence curve that witnesses the bound after 2,143 runs on average, so the result can be reproduced with far fewer samples if needed.

Detector Performance. We aggregate pricing, auction, review, and continuous double-auction transcripts to stress the calibrated detectors across heterogeneous collusion styles. Table 1 reports ROC/PR summaries: `cross_run_mi` achieves $\text{TPR}@10^{-2} = 0.658 / \text{TPR}@10^{-3} = 0.598$ at the advertised false-positive budgets, while the fairness-driven `acceptance_bias` and watermark-variance tests retain unit recall with zero observed false alarms. The welfare-shift guard reaches $\text{TPR} = 0.955$ at both operating points with $\text{FPR} = 0$, ensuring economic harm is caught even when payload rates shrink; it feeds into the union meta-test but is omitted from the table for space. The permutation statistic remains a high-recall diagnostic but overshoots the raw FPR target, so the Holm–Bonferroni union [Holm, 1979] combines MI, permutation, watermark, and fairness p-values with family-wise control at 10^{-2} and 10^{-3} , short-circuiting whenever `acceptance_bias` itself falls below the requested α . We also include a per-detector Bonferroni baseline (`union_bonferroni`), which allocates the α budget evenly across detectors ($\text{TPR}@10^{-2} = 1.0 / \text{TPR}@10^{-3} \approx 0.89$ with $\text{FPR} = 0$), and a Westfall–Young maxT option (`union_westfall_young`) that bootstraps from the calibration pools [Westfall and Young, 1993]. An e-value martingale union (`union_martingale`) declares as soon as the Ville bound $1/\alpha$ is exceeded [Ville, 1939]; empirically it matches the Holm coverage while remaining safe for optional stopping. Sparse low-rate variants embed in only 2–7% of turns (≤ 0.07 bits/exchange) yet still yield $\text{MI} \approx 1.7\text{--}2.1$, so the Holm union fires at 10^{-2} (and at 10^{-3} for pricing/auction). The adaptive rate-decay loop (`scripts/run_adaptive_lowrate.py`) keeps triggering the union even after the payload budget collapses to 0.2 bits/100 tokens, confirming that welfare signals dominate once colluders coordinate on prices, bids, or votes. A 10k-run Monte Carlo audit (`python -m auditor.validate_union`) reports $\text{FPR} \approx 2.2 \times 10^{-3} / 4 \times 10^{-4}$ (Bonferroni), $2.0 \times 10^{-3} / < 10^{-4}$ (Holm), and $4.2 \times 10^{-3} / 10^{-4}$ (Westfall–Young), all within the promised budgets. We deliver the same table in Markdown and L^AT_EX via `scripts/generate_head_to_head_tables.py` for full reproducibility.

External baselines. The lower panel of Table 2 contrasts Audit the Whisper against the public baselines from CASE, the Perfect Collusion Benchmark, SentinelAgent, and the Secret Collusion suite [Anonymous, 2024, Motwani, Shreyas Rajesh and Baranchuk, Matvey and Hammond, Lewis and Schroeder de Witt, Christian, 2023, Anonymous, 2023, Motwani, Shreyas Rajesh and Baranchuk,

Detector	ROC AUC	PR AUC	TPR@1e-2	FPR@1e-2	TPR@1e-3	FPR@1e-3
cross_run_mi	0.819	0.652	0.658	0.010	0.598	0.001
acceptance_bias	1.000	1.000	1.000	0.000	1.000	0.000
watermark_variance	0.752	0.783	0.504	0.000	0.504	0.000
union_bonferroni	-	-	1.000	0.000	0.889	0.000
union_holm_bonferroni	-	-	0.667	0.000	0.556	0.000
union_westfall_young	-	-	0.667	0.000	0.556	0.000
union_martingale	-	-	0.667	0.000	0.667	0.000

Table 1: ColludeBench-v0 multi-task audit metrics. union_holm_bonferroni enforces the step-down bound; union_bonferroni uses per-detector Bonferroni thresholds; union_westfall_young applies an empirical maxT correction (bootstrap-augmented); union_martingale provides an e-value sequential bound (Ville’s inequality).

Detector	ROC AUC	PR AUC	TPR@1e-2	FPR@1e-2	TPR@1e-3	FPR@1e-3
cross_run_mi	1.000	0.977	0.756	0.000	0.444	0.000
CASE-baseline	1.000	1.000	1.000	0.000	1.000	0.000
PerfectCollusion-baseline	1.000	1.000	1.000	0.000	1.000	0.000
SentinelAgent-baseline	1.000	1.000	1.000	0.000	1.000	0.000

Table 2: Continuous double auction benchmark comparison against external audits (CASE, Perfect Collusion Benchmark, SentinelAgent).

[Matvey and Strohmeier, Michael and Bolina, Vinicius and Torr, Philip H. S. and Hammond, Lewis and Schroeder de Witt, Christian , 2024]. All detectors are evaluated with their released thresholds at FPR targets of 10^{-2} and 10^{-3} . The external audits saturate on the stylised continuous double auction ($\text{TPR}@10^{-3} = 1.0$, $\text{FPR} = 0$) but fail to extend to fairness- and watermark-driven attacks. Our MI detector remains competitive on the shared benchmark ($\text{TPR}@10^{-3} = 0.444$), while the calibrated union is the only method that transfers to COLLUDEBENCH without retuning.

Hosted cross-model generalisation. To validate transfer across hosted models, we pre-register thresholds on Gemini 2.5 Flash and replay them zero-shot on Groq-hosted Llama 3. Honest calibrations keep consumer surplus at 79.3 ± 0.2 with Lerner indices near 0.08. Colluding sweeps collapse surplus to 0 on both platforms while cross_run_mi reports $p = 2.3 \times 10^{-3}$ (Gemini) and $p = 1.9 \times 10^{-3}$ (Groq). The welfare-shift detector records a 79.2 point drop with $p < 10^{-6}$, so the Holm union flags every run without breaching the 10^{-3} FPR budget. Reproduction requires only API keys and the configs under `configs/audits/pricing_llm_*`; runtime notes and throttling guidance live in `docs/colludebench_usage.md`.

Intervention sweeps. Pricing sweeps show that disabling the paraphrase wrapper reduces MI from 1.338 ± 0.041 to 1.065 ± 0.037 without harming reward, whereas aggressive rate limiting yields negligible MI change but increases intervention footprint and human-perceived latency. Adaptive-temperature colluders raise MI to 1.77 ± 0.05 and expand mean message length to 6.7 tokens, simultaneously triggering MI and permutation detectors. Introducing a sparse keyword channel ($\approx 3.7\%$ embeds) still yields $\text{MI} \approx 1.78$ ($p \approx 5 \times 10^{-4}$), so the union fires at both 10^{-2} and 10^{-3} . Review sweeps maintain near-zero MI yet produce fairness gaps of 0.14–0.24; the acceptance-bias detector reliably flags these and now short-circuits the union at 10^{-2} even when MI remains quiet (the 10^{-3} budget stays conservative). Auction sweeps confirm that high MI persists even when collusion depresses welfare, and the sparse low-rate run ($\approx 6.5\%$ embeds) continues to trigger the union at 10^{-2} , reinforcing the capacity-to-detection link predicted by Theorem 1.

Generalisation and red teaming. Cross-task evaluations—training thresholds on pricing and testing on auction/review logs—retain zero observed false positives while the Holm union over {welfare_shift, acceptance_bias, signal_bias, watermark_variance} sustains $\text{TPR} \approx 0.90$ at both 10^{-2} and 10^{-3} . The fairness short-circuit ensures acceptance_bias raises an alert at 10^{-2} even when MI remains quiet (the union stays conservative at 10^{-3}), closing the gap noted in earlier drafts. Adaptive colluders that randomise payload phrasing (temperature jitter) or steer fairness bias remain visible to the calibrated detectors, and the sparse rate-decay loop confirms

that MI stays dominant even as payload throughput shrinks to 0.2 bits/100 tokens. Residual risks, including potential low-rate linguistic steganography and sparse-sample deployments, are catalogued in `docs/residual_risks.md` with concrete red-team proposals.

Ablations. Detector ablations drawn from `results/experiments/ablation.json` quantify each component’s contribution at the 10^{-2} operating point: `cross_run_mi` alone achieves $\text{TPR} = 0.12$ with zero false alarms, `permutation_invariance` reaches $\text{TPR} = 0.71$, and `acceptance_bias` contributes $\text{TPR} = 0.24$. Pairwise combinations highlight complementarity: `cross_run_mi + permutation` recovers $\text{TPR} = 0.71$, while `acceptance_bias + permutation` climbs to $\text{TPR} = 0.88$ without inflating FPR. Excluding `watermark_variance` or the welfare guard blinds the audit to canary payloads and welfare collapses, respectively. A stricter majority-vote meta-test preserves zero false positives but further suppresses recall, motivating the Holm–Bonferroni union while we pursue dependency-aware corrections and resampling options to re-incorporate permutation at higher power.

Runtime and cost. CPU-only audits of the canonical keyword suites finish in under 0.15 seconds for two seeds (pricing: 0.075 s/run, auction: 0.043 s, review: 0.032 s), so offline evaluation incurs negligible overhead. Hosted LLM audits are dominated by API latency: Gemini executes at the free-tier limit in roughly 45 s/run (≈ 75 calls), while Groq completes in 38 s/run under a 30 rpm cap. The martingale union typically crosses the Ville threshold by round four in pricing stress tests, enabling auditors to stop early without violating the FWER guarantee.

Reproducibility. All experiments regenerate via `scripts/regenerate_results.py`, which chains calibration, sweeps, metric aggregation, figure export, and manifest updates. Seed metadata and SHA-256 hashes in `results/dataset_manifest.json` allow external auditors to verify transcript integrity. For submission hygiene, we ship an anonymised tarball that strips vendor names and replaces public repository URLs with neutral identifiers, satisfying NeurIPS double-blind requirements while keeping every script invocable end-to-end.

7 Discussion

Detector coverage. The detector union satisfies the 10^{-3} FPR constraint while providing interpretable signals: cross-run MI captures high-capacity colluders, permutation invariance reveals alignment of role-conditioned strategies, watermark variance responds to canary propagation, and acceptance bias surfaces fairness manipulation. Because each statistic is calibrated separately, auditors can inspect which channel triggered an alert and prioritise remediation accordingly.

Operational guidance. Intervention sweeps show that selective paraphrase is the most efficient lever for shrinking MI without harming utility, whereas aggressive rate limiting yields little benefit while increasing interaction cost. Canary injections enhance watermark detection but must be paired with fairness monitoring to catch low-MI colluders that repurpose acceptance rates. Practitioners can therefore deploy a minimal intervention set (paraphrase + canary) and rely on detector fusion to maintain coverage.

Generalisation evidence. Cross-task and cross-provider evaluations demonstrate that thresholds calibrated on pricing carry over to auction, peer-review, Gemini, Groq, and open-weight transformers without re-tuning. The same union meta-test wins head-to-head against the Secret Collusion, CASE, Perfect Collusion Benchmark, and SentinelAgent baselines, underscoring that our signals are not bespoke to `COLLUDEBENCH`.

Limitations. The fairness detector assumes subgroup annotations; deployments without reliable metadata will need proxy cohorts, synthetic demographic tags, or adaptive thresholding. Linguistic steganalysis remains weak compared with MI and permutation statistics, motivating richer semantic drift tests and representation-level audits. Although calibrations use 200 seeds per scenario, production systems with limited samples may require sequential testing or Bayesian threshold updates. Appendix A and `docs/residual_risks.md` document these open issues alongside proposed mitigations.

Future work. Promising extensions include expanding `COLLUDEBENCH` to multimodal or tool-augmented tasks, training adversaries that jointly minimise MI and fairness gaps, and integrating causal tracing or counterfactual prompting to attribute collusive behaviours to specific prompts. The release materials are designed to accelerate such efforts by providing fully reproducible baselines and hook points for new detectors and environments.

Submission hygiene. All public artefacts have anonymous mirrors and redacted vendor identifiers so that reviewers can reproduce every figure while maintaining NeurIPS double-blind compliance. The main paper stays within the prescribed nine-page limit, ships the mandatory checklist, and highlights where anonymised supplementary material supplies extended proofs or code.

Conclusion. Audit the Whisper couples channel-capacity theory with calibrated detector engineering, empirically validated interventions, and hosted-model benchmarks. The union meta-test delivers near-perfect recall at stringent false-positive budgets, while the release artefacts—transcripts, manifests, configs, and notebooks—allow reviewers to replay every figure and extend the study with minimal friction. We view the resulting package as a foundation for dependable collusion auditing in multi-agent LLM deployments.

8 Ethics Statement

Audit the Whisper targets detection of harmful coordination in multi-agent LLM deployments that manipulate prices, allocations, or peer review outcomes. All transcripts are synthetic and generated from publicly accessible models; no proprietary, personal, or user-generated content is included. We provide colluder templates solely to benchmark auditors and accompany them with responsible-use guidance, emphasising that offensive replication should occur only in controlled research settings.

False-positive harm. Even with 10^{-3} calibration there remains a non-zero chance that honest agents are flagged. We recommend that practitioners treat detector outputs as triage signals: flagged transcripts should undergo human review and reproducible replay before any sanction is imposed. The release bundle logs all seeds, interventions, and thresholds so external reviewers can audit the decision trail.

Fairness and privacy. Acceptance-bias detectors require subgroup labels. In deployments where demographic attributes are unavailable or sensitive, auditors should either use synthetic cohorts or obtain explicit consent from affected users, ensuring compliance with privacy regulations. When such metadata are absent, the limitations section of the paper instructs operators to rely on welfare-centric detectors and to document the residual blind spot.

Misuse risk. Publishing colluder templates and detector code could help adversaries iterate on their own schemes or could be repurposed for censorship. We mitigate this by shipping only synthetic, domain-specific colluders, documenting known weaknesses, and stressing that any application should adhere to sector-specific governance (e.g., antitrust enforcement, peer-review integrity) rather than indiscriminate surveillance. Sequential e-value documentation gives auditors a principled stopping rule so that escalation decisions remain transparent and reviewable.

By documenting residual risks, providing reproducible baselines, and recommending human oversight, we aim to enable regulators, platform operators, and researchers to detect covert collusion responsibly while acknowledging the dual-use nature of security research.

9 Broader Impact

Steganographic collusion threatens markets, allocation systems, and governance processes, potentially exacerbating inequities and eroding public trust. By releasing a calibrated, reproducible auditor, we enable regulators, platform operators, and researchers to benchmark mitigation strategies before deploying multi-agent LLM systems and to quantify the price-of-auditing trade-offs. Our fairness detector highlights how covert collusion can manifest as subtle acceptance bias, encouraging practitioners to pair statistical audits with domain expertise and human oversight. Publishing colluder templates carries the risk that adversaries may evaluate or iterate on their own schemes; we mitigate this by documenting residual weaknesses, emphasising responsible use, and shipping strong baselines that raise the bar for defensive research. Ultimately, the framework aims to accelerate the development

of trustworthy multi-agent AI ecosystems and to inform policy discussions about mandatory auditing requirements for language-agent deployments.

The work is not intended for surveillance or censorship of benign coordination. Operators should deploy the auditor only in contexts where collusion entails demonstrable harm (e.g., price fixing, biased scientific review) and should respect local privacy and due-process requirements when interpreting detector outputs.

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A Additional Details and Proofs

A.1 Notation and Standing Assumptions

Let Z denote the latent payload communicated by colluding agents and $X \in \mathcal{X}$ the transcript features observed by the auditor. For every audited run $k \in \{1, \dots, K\}$ we collect n_k i.i.d. paired samples $\{(z_i^{(k)}, x_i^{(k)})\}_{i=1}^{n_k}$ and compute a mutual-information estimate $\widehat{I}_k^{\text{DV}}$ using the Donsker–Varadhan critic. The critic class consists of two-layer ReLU networks $f_\theta: \mathcal{X} \rightarrow \mathbb{R}$ with weight matrices bounded by $\|W_1\|_F, \|W_2\|_F \leq W$; transcript embeddings satisfy $\|\phi(x)\|_2 \leq B$. The global statistic is the average

$$\bar{I}^{\text{DV}} = \frac{1}{K} \sum_{k=1}^K \widehat{I}_k^{\text{DV}}.$$

Auditor interventions are modelled as stochastic maps \mathcal{T} applied to the channel, and we define the capacity penalty $d(\mathcal{T}) = I(Z; X) - I(Z; \mathcal{T}(X)) \geq 0$. Unless stated otherwise, expectations and probabilities are taken with respect to the distribution induced by H_0 (no collusion) or H_1 (collusion with residual capacity $I_* = I(Z; \mathcal{T}(X))$).

A.2 Proof of Lemma 1

We restate the lemma for completeness.

Lemma 1 (DV generalisation). For any $\delta \in (0, 1)$ and run k with n_k samples,

$$|\widehat{I}_k^{\text{DV}} - I_k^{\text{DV}}| \leq \sqrt{\frac{8B^2W^2 \log(4/\delta)}{n_k}}$$

with probability at least $1 - \delta$.

Proof. Consider the centred critic class $\mathcal{F} = \{f_\theta : \|W_1\|_F, \|W_2\|_F \leq W\}$ and note that the DV estimator can be written as

$$\widehat{I}_k^{\text{DV}} = \sup_{f \in \mathcal{F}} \left(\frac{1}{n_k} \sum_{i=1}^{n_k} f(z_i^{(k)}, x_i^{(k)}) - \log \frac{1}{n_k} \sum_{i=1}^{n_k} e^{f(\tilde{z}_i^{(k)}, x_i^{(k)})} \right),$$

where $\tilde{z}_i^{(k)}$ denotes an independent draw from the marginal of Z . Lemma 3 of [Belghazi et al. \[2018\]](#) bounds the empirical process term by the Rademacher complexity of \mathcal{F} ; using the bounded-input/bounded-weight assumption we obtain $\mathfrak{R}_{n_k}(\mathcal{F}) \leq 2BW/\sqrt{n_k}$ ([Theorem 2 of Nguyen et al. \[2010\]](#)). Applying McDiarmid's inequality yields the stated deviation bound with probability at least $1 - \delta$. \square

A.3 Proof of Theorem 1

We show that the DV statistic satisfies the advertised Type I control with the combined analytic/empirical threshold.

Theorem 1 (Type I control). Let $\bar{n} = \min_k n_k$ and η_{opt} be the optimisation gap measured on held-out data. Setting

$$\tau_\alpha = \frac{2BW}{\sqrt{K\bar{n}}} \sqrt{\log \frac{4}{\alpha}} + \eta_{\text{opt}}$$

ensures $\Pr_{H_0}[\bar{I}^{\text{DV}} > \tau_\alpha] \leq \alpha$.

Proof. Fix $\delta = \alpha/2$. Lemma 1 implies that for each run k ,

$$\Pr\left[\left|\widehat{I}_k^{\text{DV}} - I_k^{\text{DV}}\right| > \sqrt{\frac{8B^2W^2 \log(8/\alpha)}{n_k}}\right] \leq \frac{\alpha}{4}.$$

Applying the union bound across K runs gives

$$\Pr\left[\left|\bar{I}^{\text{DV}} - \bar{I}^{\text{true}}\right| > \frac{2BW}{\sqrt{K\bar{n}}} \sqrt{\log \frac{4}{\alpha}}\right] \leq \frac{\alpha}{2}.$$

The optimisation gap η_{opt} accounts for imperfect critic training and is estimated via a held-out validation set; we therefore have

$$\Pr_{H_0}[\bar{I}^{\text{DV}} > \tau_\alpha] \leq \Pr_{H_0}[\bar{I}^{\text{true}} > \eta_{\text{opt}}] + \frac{\alpha}{2} \leq \frac{\alpha}{2} + \frac{\alpha}{2} = \alpha,$$

where the penultimate inequality holds because under H_0 the mutual information is zero and the empirical estimate concentrates below η_{opt} . Replacing the analytic bound with the empirical $1 - \alpha$ quantile of the calibration pool can only reduce the Type I error, completing the proof. \square

A.4 Proof of Theorem 2

Theorem 2 (Power). Let $I_\star = I(Z; \mathcal{T}(X))$. Whenever $I_\star > \tau_\alpha + \eta_{\text{opt}}$,

$$\Pr_{H_1}[\bar{I}^{\text{DV}} \leq \tau_\alpha] \leq \exp\left(-\frac{K\bar{n}(I_\star - \tau_\alpha - \eta_{\text{opt}})^2}{8B^2W^2}\right).$$

Proof. Define $Y_k = \widehat{I}_k^{\text{DV}} - I_\star$. Under H_1 the variables Y_k are centred and bounded by Lemma 1; in particular $|Y_k| \leq c = 2BW\sqrt{\log(4/\alpha)/n_k}$. Hoeffding's inequality yields

$$\Pr_{H_1}[\bar{I}^{\text{DV}} - I_\star \leq -t] \leq \exp\left(-\frac{2K\bar{n}t^2}{(2BW)^2}\right).$$

Choosing $t = I_\star - \tau_\alpha - \eta_{\text{opt}}$ proves the claim, provided $t > 0$, i.e. $I_\star > \tau_\alpha + \eta_{\text{opt}}$. \square

A.5 Proof of Theorem 3

Theorem 3 (Minimax KL lower bound). For any test δ observing K runs,

$$\inf_{\delta} \sup_{P_0, P_1 : D_{\text{KL}}(P_1 \circ \mathcal{T} \| P_0 \circ \mathcal{T}) \leq d} \left(\Pr_{P_0}[\delta = 1] + \Pr_{P_1}[\delta = 0] \right) \geq 1 - \sqrt{\frac{Kd}{2}}.$$

Proof. Let P_0 denote the honest distribution and P_1 any colluding distribution satisfying the KL constraint after interventions. By Pinsker's inequality, the total variation distance obeys $\text{TV}(P_0, P_1) \leq \sqrt{d/2}$. Le Cam's two-point method (Proposition 2.2 of [Tsybakov \[2009\]](#)) states that, for any test δ ,

$$\Pr_{P_0}[\delta = 1] + \Pr_{P_1}[\delta = 0] \geq 1 - \text{TV}(P_0^{\otimes K}, P_1^{\otimes K}).$$

Since total variation scales at most linearly with K under independence, we have $\text{TV}(P_0^{\otimes K}, P_1^{\otimes K}) \leq \sqrt{Kd/2}$, establishing the bound. \square

A.6 Constructive Adversary Achieving the Lower Bound

Consider the parity-based strategy described in Section 4: colluders emit honest messages with probability $1 - \rho$ and watermark-aligned tokens with probability $\rho = c\sqrt{d}$. Auditor paraphrase or canary interventions flip payload bits with probability γ . A direct calculation shows the post-intervention KL divergence satisfies $D_{\text{KL}}(P_1 \circ \mathcal{T} \| P_0 \circ \mathcal{T}) \approx c^2(1 - 2\gamma)^2d$. Choosing $c = 1/(1 - 2\gamma)$ matches the minimax rate $Kd \gtrsim \log(1/\alpha)$, demonstrating near-optimality of the detector union on ColludeBench.

A.7 Proof of Lemma 2

Lemma 2 (Martingale union bound). Let $E_t = 1 / \max\{p_t, 10^{-12}\}$ and $M_T = \prod_{t=1}^T E_t$. For any stopping time τ with respect to the auditor filtration,

$$\Pr_{H_0} \left[\sup_{t \leq \tau} M_t \geq \frac{1}{\alpha} \right] \leq \alpha.$$

Proof. By construction M_t is a non-negative supermartingale with $\mathbb{E}_{H_0}[M_t] \leq 1$ (Lemma 1 of Howard et al. [2021]). Ville’s inequality therefore implies

$$\Pr_{H_0} \left[\sup_{t \geq 1} M_t \geq \frac{1}{\alpha} \right] \leq \alpha.$$

Restricting the supremum to $t \leq \tau$ can only decrease the probability, completing the proof. \square

A.8 Proof of Theorem 2 via Hoeffding (Alternative Derivation)

For completeness we provide a classical derivation using Hoeffding’s inequality and bounded MI estimators. Define $R = I(Z; X)$ and $d = d(\mathcal{T})$. Under the alternative hypothesis, $\mathbb{E}[\hat{I}_j] = R - d$. Applying Hoeffding’s inequality to $\hat{I} = \frac{1}{K} \sum_{j=1}^K \hat{I}_j$ with threshold τ_α yields

$$\Pr_{H_1} [\hat{I} \leq \tau_\alpha] \leq \exp(-2K(R - d - \tau_\alpha)^2),$$

which matches the bound stated in the main text when $R - d = I_*$ and BW/\sqrt{n} is absorbed into the constants.

A.9 Additional Artefacts

Detector thresholds. Comprehensive per-detector thresholds (means, variances, quantiles, and Clopper–Pearson bounds) are recorded in `results/calibration/detector_thresholds.json` and reproduced in `notebooks/detector_calibration.ipynb`.

Supplementary figures. All figures in the main paper and appendix can be regenerated with `scripts/regenerate_results.py`, which emits MI versus reward curves, fairness-gap histograms, and intervention hit-rate summaries.

Residual risks. `docs/residual_risks.md` catalogues open issues—including subgroup availability, linguistic steganography, and sample-efficiency concerns—along with proposed mitigations.