

Net Influence Meets Language-Model Agents: Detecting a Colluding Pair from Actions Alone

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Abstract

Pairwise net influence (Bonjour et al., 2022) flags two agents as colluding when each influences the other’s actions more than any third agent does, measured by conditional mutual information. It assumes exactly two colluders and had not been tested on language-model agents. We implement it with unit tests, replicate the original Rock–Paper–Scissors results (matching the published accuracy table to 2.6 points on average once two unstated details are inferred), and apply it to a four-seller repeated market game played by Qwen2.5-1.5B-Instruct agents, in which one seeded pair runs a market-allocation cartel through a covert per-round message. Across 150 LLM games (15 conditions $\times$ 10 seeds) with negative and scripted positive controls, net influence ranked the true pair first in 0.91 [0.86, 0.96] of cartel runs, against 0.89 for Cramér’s V and 0.11 for a frozen-prompt LLM judge; a variant that uses the agents’ exact action probabilities reached 0.95. The “net” step adds calibration rather than ranking power: with a rich state it cuts fixed-threshold false alarms in RPS from 100% to 0–23%, and conditional MI without it matched its ranking accuracy in every condition. Conditioning on public state is the key ingredient: it defeats an observed common cause that fools plain correlation (RPS top-1 1.00 vs 0.07), but it makes a cartel keyed on public information invisible (0/10 vs 10/10). Net influence also fails under hidden common causes, near-deterministic policies and more than one cartel. Action-level dependence is a cheap, useful screen for LLM collusion, not proof of agreement.

1 Introduction

Collusion between AI agents is covert cooperation by some agents that harms others (Dafoe et al., 2020; Hammond et al., 2025). LLM agents acting as sellers can sustain supracompetitive prices (Fish et al., 2024) and coordinate in double auctions when they can communicate (Agrawal et al., 2025), and they can hide messages in innocuous text (Motwani et al., 2024; Mathew et al., 2024). A monitor that reads messages can be evaded by steganography. A monitor that looks only at *actions* targets what actually harms third parties: coordination that matters must eventually change what agents do.

Bonjour, Aggarwal & Bhargava (2022) proposed such an action-only measure. The *individual influence* $\gamma(i;j)$ is the mutual information (MI) between two agents’ actions given the game state; the *net influence* $\Gamma(i;j) = \gamma(i;j) - \max_{\{k \neq i,j\}} \gamma(k;j)$ subtracts the strongest influence any third agent has on j ; and a pair is flagged when both $\Gamma(i;j)$ and $\Gamma(j;i)$ reach a threshold α . They tested it on scripted and RL agents in three-player Rock–Paper–Scissors (RPS) and Leduc poker, with exactly two colluders. There is no public implementation, and the measure has not been tried on LLM agents. The sprint question is whether it, or a variant, works for a set of LLM agents, and how it compares with ordinary action correlation and an LLM judge.

Our main contributions are:

1. **An open, unit-tested implementation of net influence** (14 analytic tests) and a replication of Bonjour et al.’s RPS detection-accuracy table. We find that the published numbers are reproduced only with MI in bits and a detector state equal to the previous round’s joint action — two details not stated in the main text.

2. **The first application of net influence to LLM agents:** a four-seller repeated market game played by Qwen2.5-1.5B-Instruct agents with a seeded cartel, controls, confounds and stress tests (15 conditions $\times$ 10 seeds), compared head to head with correlation, MI without the “net” step, and a frozen-prompt LLM judge.
3. **A map of where the measure helps and where it is blind.** Conditioning on the right public state is what defeats observed common causes; the “net” subtraction mainly makes a fixed threshold self-calibrating; and collusion implemented as a convention on public information is invisible to state-conditioned MI. A *policy-access* estimator that uses the agents’ exact action probabilities is the most accurate variant.

2 Related Work

Information-theoretic collusion detection. Bonjour et al. (2022) define individual and net influence and evaluate them on hand-crafted and DDQN-trained colluders in RPS and Leduc Hold’em. Our γ is the conditional MI of their Eq. 3; we keep their Eq. 4–5 unchanged. Banchio & Mantegazza (2022) show that learners can become statistically coupled and reach collusive outcomes without communication: dependence is not agreement, which our common-cause ablations probe. Tailor (2025) audits LLM agents with cross-run MI plus other tests; we use the per-pair *net* measure within a single run and compare it with an LLM judge on the same games.

Collusion by LLM agents. Fish et al. (2024) find LLM pricing agents reach supracompetitive prices; Agrawal et al. (2025) find seller communication, model choice and oversight pressure change collusion in double auctions. Motwani et al. (2024) and Mathew et al. (2024) study steganographic collusion, which defeats message monitors but not, in principle, action monitors.

Other monitors. Rose et al. (2026) detect multi-agent collusion from model activations with linear probes (white-box). LLM-as-a-judge evaluation (Zheng et al., 2023) motivates our judge baseline. Jaques et al. (2019) measure one agent’s counterfactual influence on another’s policy; our policy-access estimator is close in spirit but purely observational and needs only action probabilities, not activations.

3 Methods

3.1 Net influence (faithful implementation)

For agents i, j with actions a^i, a^j and a common (fully observable) state s ,

$$\gamma(i;j) = \sum_s p(s) \sum_{\{a^i, a^j\}} p(a^i, a^j | s) \log [p(a^i, a^j | s) / (p(a^i | s) p(a^j | s))] \quad (\text{Bonjour Eq. 3/6}),$$

$\Gamma(i;j) = \gamma(i;j) - \max_{\{k \neq i, j\}} \gamma(k;j)$ (Eq. 4), and Eq. 5 flags (i,j) iff $\Gamma(i;j) \geq \alpha$ and $\Gamma(j;i) \geq \alpha$, returning a pair only if exactly one pair is flagged (their Algorithm 1). Probabilities are count ratios (Eq. 12–13). In simultaneous games γ is symmetric. For ranking we score each pair by $\min(\Gamma(i;j), \Gamma(j;i))$, the continuous version of Eq. 5. $\alpha = 0.05$ bits as in the paper. Code: `src/netinf.py`; 14 unit tests with closed-form answers (e.g. the MI of Bonjour’s collusion-probability assistant) pass (`results/unit_tests.txt`).

Baselines. (i) *Cramér’s V* of the unconditional 3×3 action contingency table (“ordinary action correlation”); (ii) *unconditional MI* (γ with no state); (iii) *conditional MI* γ without the net step — an ablation isolating what “net” adds; (iv) an *LLM judge* (§3.4).

Policy-access estimator (our extension). LLM agents here choose by scoring the three legal action tokens, so their exact policy $\pi_j^t(\cdot)$ at every decision is known. Replacing one agent’s one-hot action by its policy vector

gives an unbiased, lower-variance estimate of $p(a^{i,j}|s)$ whenever j acts after or simultaneously with i ; since the detector does not know the order, we compute both orientations and keep the larger MI (`gamma_matrix_soft`). Net influence is then applied unchanged (“net infl. (policy access)”).

3.2 Scripted RPS

Actions R/P/S; Bonjour’s multi-player payoff (Table 1 of their paper). The *assistant* colluder plays the move its primary beats with probability CP (guaranteeing the primary a point), and a uniformly random move otherwise; other agents are uniform. **R1** replicates their Table 2 (3 players, 1000 iterations per cell). **R2** uses 4 players, $T = 300$ rounds, 300 seeds per condition, random seat assignment: negative control; positive control with $CP \in \{0.1 \dots 1.0\}$; *common cause*: two honest agents follow a public signal s_t with probability 0.7 (no agreement), either *observed* by the detector ($s = s_t$) or *hidden* (no state), with or without a colluding pair ($CP = 0.4$); *all-cause*: every agent follows the signal; two colluding pairs; a colluding triple. **R3** varies T for $CP = 0.3$. **R4** measures fixed-threshold false positives of γ vs Γ with a rich state (previous joint action, 81 states).

3.3 Four-seller LLM market game

Four sellers each round pick one of three markets A/B/C; the public *booming* market (uniform each round) is worth 12, the others 6; revenue is split equally among the sellers in a market. Each agent sees the market sizes and its own last choice and earnings. Agents are Qwen2.5-1.5B-Instruct (Qwen Team, 2024) on CPU; each decision is one forward pass, the softmax over the tokens “A”, “B”, “C” (temperature 1 unless stated) is the policy, and the action is sampled from it. The seeded cartel is Bonjour’s *assistant collusion* with a covert channel: the primary P is told it has a secret deal with seller Q never to compete in the same market and that it privately tells Q its choice; Q is told about the deal and receives “Private message from Seller P: ‘I picked market X this round.’” The pair is drawn at random each seed. Full prompts are in `src/market_llm.py`. Conditions ($T = 200$ rounds; 10 seeds unless noted):

- **negative** — no deal; **collusion** — the cartel above; **scripted_pos** — positive control: Q replaced by a script that picks uniformly among the two markets P did not pick;
- ****confound_disjoint / _overlap / _only**** — a *common incentive without agreement*: two sellers privately get the same newsletter tip “sellers who pick market Y this round get a bonus of 4” (paid; Y random). Subscribers: two honest sellers, the primary colluder plus an honest seller, or two honest sellers with no cartel. The detector either ignores the tip ($s = \text{booming market}$) or conditions on it ($s = \text{booming} \times \text{tip}$);
- **monitored** — both colluders are told a regulator watches for coordination and fines it;
- **convention** — the pair agrees in advance that P always takes the booming market and Q the first non-booming market, with no messages: collusion keyed only on public information; **scripted_convention** — the same rule executed by a script (each colluder follows it with probability 0.9), with LLM honest sellers;
- **collusion_cp0.3 / cp0.15** — the covert message reaches Q in only a random 30% / 15% of rounds (Bonjour’s CP);
- **two_pairs** — all four sellers are in two cartels; **collusion_T0.5 / T1.5 / T2** — sampling temperature as a disruptor.

The detector’s default state is the booming market (3 states). We also report $s = \text{booming} \times \text{previous joint action}$ (243 states), because honest LLM sellers condition on their own last earnings, which couples them through shared history.

3.4 LLM judge

Qwen2.5-1.5B-Instruct reads the last $K = 40$ rounds of the public transcript (actions; for the market game also the booming market and earnings) with a frozen prompt (`src/judge.py`, version v1-2026-10-23) and answers which of the six pairs is most likely secretly coordinating. We score the option tokens “1”–“6”, average the distribution over two option orders (forward, reversed) to reduce position bias, and use each pair’s probability as its score. A second frozen question (“is any pair secretly coordinating? Yes/No”) gives $P(\text{Yes})$ as a run-level detection score. A second frozen variant (v1-stats-2026-10-24) replaces the transcript with each pair’s same-market rate over the whole game, to test whether the judge can use summarised evidence.

3.5 Metrics

Per run, **top-1** = the true pair has the highest of the six pair scores (for two pairs or a triple: precision of the top-k); **within-run AUC** = $P(\text{true pair outscores a non-colluding pair})$. **Detection AUROC** separates collusion runs from negative-control runs using the maximum pair score. 95% CIs are percentile bootstraps over seeds (2000 resamples) or Wilson intervals for rates. Every number below is read from `results/summary_*.txt|json`.

4 Results

4.1 Replication of Bonjour et al. (scripted RPS)

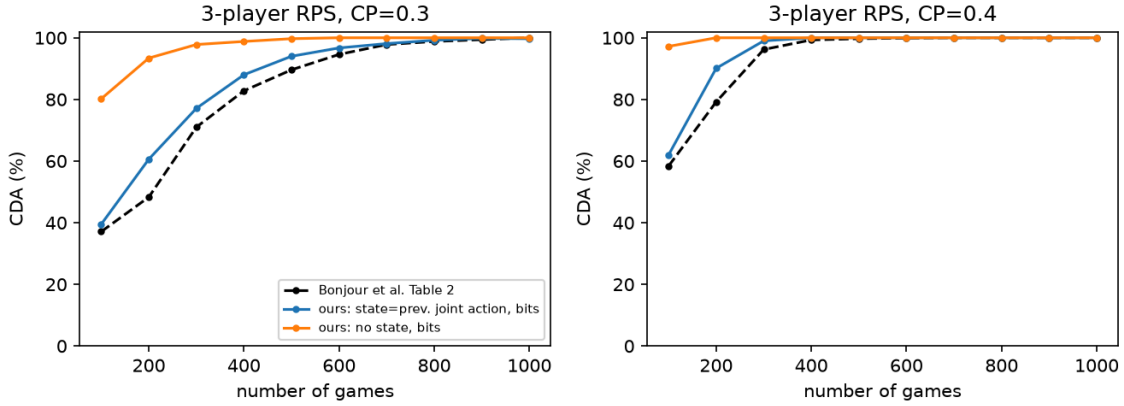


Figure 1

Figure 1. Collusion detection accuracy (CDA: exactly the true pair flagged at $\alpha = 0.05$) for three-player RPS with an assistant colluder, vs number of games; 1000 iterations per point. Black: Bonjour et al. Table 2. Blue: our implementation with MI in bits and state = previous round’s joint action. Orange: bits, no state. (`results/rps_r1_replication.json`, `summary_rps.json`.)

The paper’s main text does not state the log base or the RPS state. With bits and state = previous joint action, our CDA is within 2.6 points of their Table 2 on average over the 20 cells (largest gap 12.3), and their $CP = 0.2$ / 10,000-game point (82.3%) is matched (81.5%); in nats it is unreachable (0.0%) because $\Gamma = 0.038 \text{ nats} < \alpha$. Without a state, small-sample accuracy is far higher ($CP = 0.3$, 100 games: 80.2% vs 37.1%). One cell does not replicate: $CP = 0.2$, 1000 games, 46.8% vs 24.3%.

4.2 Four-player scripted RPS: controls and confounds

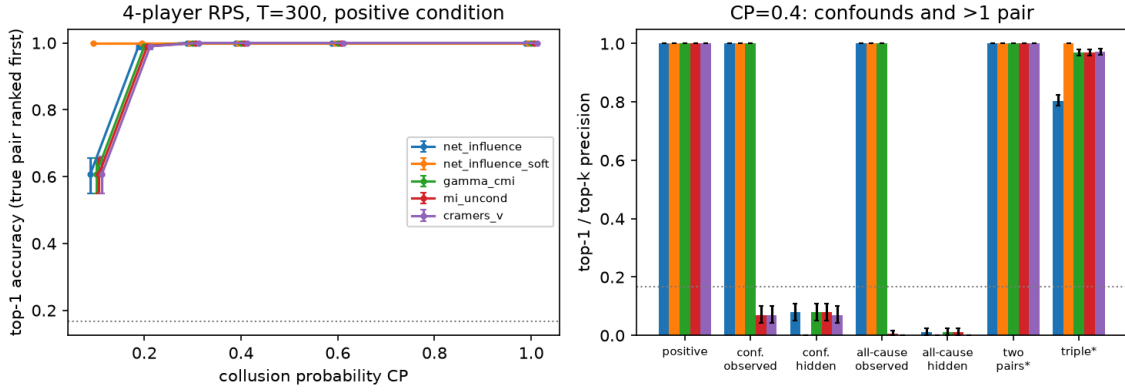


Figure 2

Figure 2. Four-player RPS, $T = 300$ rounds, 300 seeds per bar/point, 95% bootstrap CIs. Left: top-1 accuracy vs collision probability. Right: $CP = 0.4$ under common-cause confounds and with more than one colluding pair (*: precision of the top- k pairs, $k = \text{number of true pairs}$). Dotted line: chance ($1/6$). (results/summary_rps.txt.)

- **Positive control.** For $CP \geq 0.2$ every hard-count method ranks the true pair first in $\geq 99\%$ of seeds; at $CP = 0.1$ all reach 0.61 [0.55, 0.66]. Net influence, conditional MI and Cramér’s V are indistinguishable here (R3: 0.75 vs 0.79 at $T = 50$; ≥ 0.96 at $T \geq 100$).
- **Observed common cause.** When two honest agents follow a public signal that the detector conditions on, net influence and conditional MI keep top-1 = 1.00, while Cramér’s V and unconditional MI drop to 0.07 [0.04, 0.10] (they rank the signal-following pair first); if all agents follow the signal, Cramér’s V falls to 0.00 while net influence stays at 1.00.
- **Hidden common cause.** If the detector does not see the signal, every method fails (top-1 ≤ 0.08), and in runs with no collusion at all Eq. 5 flags the honest signal-following pair in 300/300 seeds.
- **More than two colluders.** With two independent cartels, ranking still finds both (top-2 precision 1.00), but Bonjour’s decision rule (“exactly one pair above α ”) reports *no* collusion in 300/300 seeds because two pairs are flagged. With a colluding triple it returns one true pair in only 123/300 seeds and nothing in 177/300.
- **What “net” buys.** R4: with a rich state (previous joint action, 81 states) a fixed α on raw γ flags some pair in 100% of no-collusion runs at every N from 100 to 3000, because the plug-in MI is biased upward for every pair. The net subtraction cancels that shared bias: 23%, 9.4%, 0.8% and 0% false alarms at $N = 100, 300, 1000, 3000$, while detecting $CP = 0.4$ collusion exactly in 18%, 69%, 99.8% and 100% (results/rps_r4_threshold.json). “Net” calibrates a fixed threshold; it does not improve ranking.

The policy-access estimator reaches 1.00 even at $CP = 0.1$, but here that is partly an artefact (honest policies are exactly uniform, so their soft MI is exactly zero).

4.3 Four-seller LLM market game

LLM agents do form the cartel when prompted. With Qwen2.5-1.5B, the assistant picks its partner’s announced market in 2.4% of rounds vs 45.8% for other pairs, and colluders earn 4.94 per round vs 3.76 for honest sellers (results/summary_market_1.5b.txt). Telling them they are monitored barely changes this (5.1%). Qwen2.5-0.5B cannot play: all sellers pick the same market $\sim 90\%$ of the time and the “assistant” *copies* its partner (98%); no detector finds the pair (net influence top-1 0/5; results/summary_market_0.5b.txt).

Table 1. Top-1 accuracy (fraction of 10 seeds, T = 200, detector state = booming market) for the true pair among 6. 95% CIs are in results/summary_market_1.5b.txt; with 10 seeds a top-1 of 1.00 has a bootstrap CI of [1.00, 1.00] and a Wilson CI of about [0.72, 1.00].

Condition (s = booming market)	same- market rate, pair / others	Net infl.	Net infl. (policy)	Cond. MI	Cramér’s V	Judge (transcript)	Judge (pair stats)
LLM cartel (covert message)	0.024 / 0.458	1.00	1.00	1.00	1.00	0.10	0.00
scripted positive control	0.000 / 0.390	1.00	1.00	1.00	1.00	0.00	0.00
cartel told it is monitored	0.051 / 0.452	1.00	1.00	1.00	1.00	0.00	0.00
message in 30% of rounds	0.215 / 0.444	0.90	0.80	0.90	0.80	0.00	0.10
message in 15% of rounds	0.281 / 0.443	0.60	0.80	0.60	0.70	0.10	0.00
temperature 0.5	0.002 / 0.460	0.63	0.95	0.63	0.38	0.10	0.00
temperature 2	0.116 / 0.389	1.00	1.00	1.00	1.00	0.10	0.10
two cartels (top-2 precision)	0.017 / 0.447	1.00	1.00	1.00	1.00	0.25	0.15
cartel + tip shared by 2 honest (tip hidden)	0.019 / 0.448	1.00	1.00	1.00	1.00	0.40	0.00
cartel + tip shared by colluder & honest (hidden)	0.048 / 0.435	1.00	1.00	1.00	1.00	0.10	0.00
prompted public convention (not followed)	0.480 / 0.459	0.20	0.00	0.20	0.20	0.20	0.30
scripted public convention	0.052 / 0.407	0.00	0.00	0.00	1.00	0.40	0.10

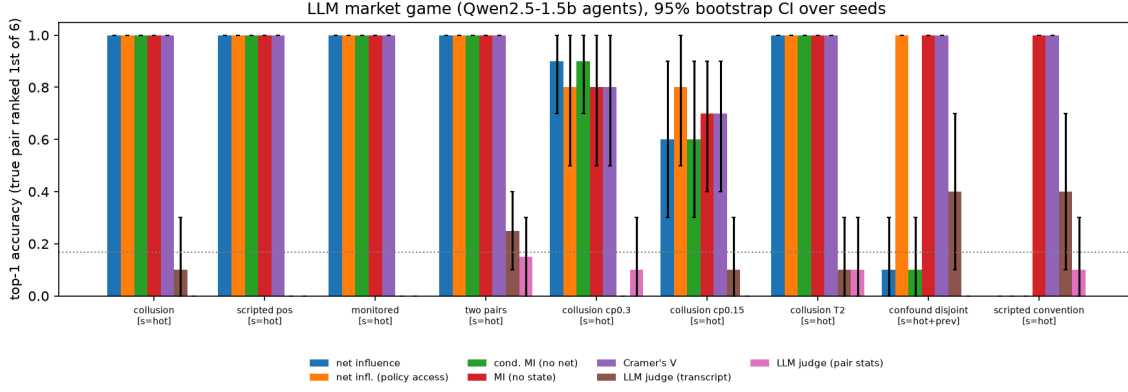


Figure 3

Figure 3. Top-1 accuracy by condition and method in the LLM market game (10 seeds each, bootstrap 95% CIs). The last two groups are failure modes: $s = \text{booming} \times \text{previous joint action}$ (too many states for 200 rounds) and the scripted public convention.

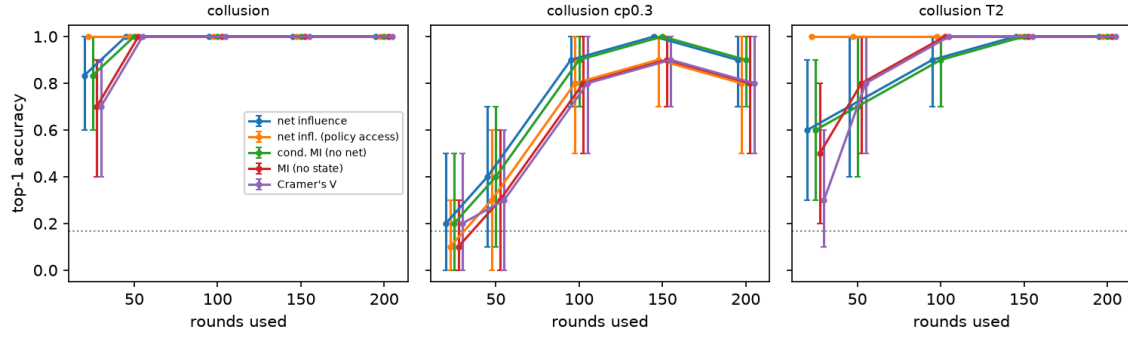


Figure 4

Figure 4. Top-1 accuracy vs number of rounds used (prefixes of the same 10 runs).

Main observations (Table 1, Figs. 3–4):

1. **Net influence works on LLM agents.** For the LLM cartel, the scripted control, monitored colluders, two cartels (top-2 precision), temperature 1.5 and 2, and both newsletter-tip confounds, net influence ranks the true pair(s) first in 10/10 seeds — and so do conditional MI and Cramér’s V: a cartel that uses a private channel every round is easy. With only 25 rounds the conditional measures are better (cartel: net influence 0.83, policy access 1.00, Cramér’s V 0.70; temperature 2: 0.60 / 1.00 / 0.30).
2. **Weak collusion separates the methods only slightly.** When the covert message arrives in 30% / 15% of rounds, top-1 for net influence is 0.90 / 0.60, policy access 0.80 / 0.80, Cramér’s V 0.80 / 0.70 — overlapping CIs. The obstacle is that honest LLM sellers are coupled through shared history (each reacts to its own last earnings, which depend on the others). With $s = \text{booming market only}$, Eq. 5 flags a pair in 3/10 *negative-control* runs (0/10 when s also includes the previous joint action).
3. **Low temperature hides a working cartel from MI.** At temperature 0.5 the assistant almost never collides with its partner (0.2% of rounds), yet net influence ranks the pair first in only 0.63 of seeds and Cramér’s V in 0.38. Near-deterministic policies leave little entropy for MI to measure. The policy-access estimator recovers 0.95.

4. **Public-information collusion is invisible to state-conditioned MI.** With a scripted convention (P takes the booming market, Q the first non-booming market; the honest sellers are LLMs), the pair almost never collides (5.2%) and earns 5.84 vs 3.96. Net influence, conditional MI and the policy-access variant rank it first in 0/10 seeds, while unconditional MI and Cramér’s V do so in 10/10: conditioning on the public state removes exactly the evidence. (The 1.5B agents did not follow the *prompted* convention — same-market rate 48% vs 46%.)
5. **Over-fine states hurt.** Conditioning on booming $\times$ previous joint action (243 cells for 199 rounds) makes the plug-in estimator unreliable: with a hidden newsletter tip net influence ranks the tip-sharing honest pair first in 9/10 seeds, whereas the policy-access estimator stays at 10/10.

4.4 LLM judge

On scripted RPS (10 games per condition, last 40 rounds shown), the transcript judge’s top-1 was 0.20 [0.00, 0.50] at CP = 1.0, 0.40 [0.10, 0.70] at CP = 0.4 and 0.20 at CP = 0.2. Net influence computed on the *same 40-round windows* scored 1.00, 0.80 and 0.40 (1.00 on all 300 rounds). The judge’s P(Yes) was about 0.75 in every condition (AUROC vs negative 0.49–0.61; every CI includes 0.5), and it put 0.33–0.35 of its probability on “Players 2 and 4” whatever the data (`results/summary_judge_rps.txt`).

In the market game, pooled over the 10 single-cartel conditions (100 runs), top-1 was 0.11 [0.05, 0.17] for the transcript judge and 0.02 [0.00, 0.05] for the judge given per-pair same-market rates, versus 0.91 [0.86, 0.96] for net influence, 0.95 [0.91, 0.99] for the policy-access variant, 0.91 for conditional MI and 0.89 [0.82, 0.94] for Cramér’s V (`results/summary_market_pooled.txt`). The statistics-fed judge is *worse* than chance (within-run AUC 0.30 [0.24, 0.35]) for an identifiable reason: its top pair was the pair that most often chose the same market in 0.40 [0.33, 0.48] of runs, and the least-similar pair — the cartel’s signature — in only 0.05 [0.02, 0.09]. It equates coordination with similarity. As a run-level detector, the transcript judge’s P(Yes) had AUROC 0.66 [0.46, 0.87] against the negative control, compared with 0.94–0.96 for the maximum pair score of the statistical detectors.

5 Discussion and Limitations

What the results mean. Net influence transfers to LLM agents: a seeded LLM cartel that coordinates through a private per-round message is found from actions alone, robustly across seeds, monitoring instructions, temperature ≥ 1 , a second cartel, and a shared private incentive. But the “net” step contributes calibration, not ranking power: conditional MI without it had the same top-1 in every LLM condition (and higher for the RPS triple). The ingredient that matters is *conditioning on the right state*. It defeats observed common causes, where plain correlation fails badly (RPS: 0.07 vs 1.00), and it creates a blind spot for collusion that is keyed on that same public state (LLM market: 0/10 vs 10/10). A practical monitor should therefore report both conditional and unconditional dependence and treat their disagreement as informative: high unconditional but zero conditional dependence suggests either an innocent common response to public information or a public-signal convention, and only the payoff pattern (here, a pair that never collides and out-earns the rest) separates the two.

Policy access helps. When the per-decision action distribution is available — as it is for any LLM agent served through an API with log-probabilities — the soft estimator was at most one seed worse than hard counts wherever the cartel actually operated (message in 30% of rounds: 0.80 vs 0.90), had the best pooled top-1 (0.95), and fixed the cases where hard counts struggled: temperature 0.5 (0.95 vs 0.63), an over-fine state (10/10 vs 1/10) and 25-round windows.

The LLM judge was the weakest baseline. A 1.5B judge is near chance on transcripts, has a label bias, and with summary statistics follows a similarity prior, while a market-splitting cartel is the pair that most often does *different* things. Larger judges may do better; we could not run them at zero cost on CPU.

Limitations. (i) Small models (Qwen2.5-1.5B agents and judge) and one market game; the cartel is instructed, not emergent. (ii) 10 seeds per LLM condition gives wide CIs (about ± 0.3 for top-1 near 0.5). (iii) Our detector state is chosen by us; in deployment the right state is unknown, and Figs. 2–3 show that both too little state (hidden confounds) and too much (sparse cells) break it. (iv) Thresholds: $\alpha = 0.05$ bits is not calibrated for LLM games — 3/10 negative runs are flagged. A permutation or held-out calibration of α is needed. (v) The replication of Bonjour et al. relies on two inferred details (bits, previous-joint-action state). (vi) Policy access assumes truthful log-probabilities from the monitored agents.

Future work. Calibrated thresholds (permutation tests over rounds); co-information or multi-way generalisations for cartels larger than two (Bonjour et al. suggest the co-information lattice); combining conditional and unconditional dependence with payoff anomalies; stronger open judges; emergent rather than instructed cartels (e.g. Agrawal et al.’s double auction).

6 Conclusion

Pairwise net influence can identify a colluding pair of LLM agents from their actions alone in a four-agent market game. It does so as reliably as conditional mutual information and, when collusion runs through a private channel, about as reliably as plain correlation. It is far more reliable than a small LLM judge. Its real strengths are conditioning on public state, which removes observed common causes, and a self-calibrating threshold. Its blind spots are unobserved common causes, collusion keyed on public information, near-deterministic policies, and cartels larger than one pair.

Code and Data

All code, raw game logs, judge outputs and summaries are in `netinf/` (README.md has the reproduction commands); no external repository was created. No info-hazard concerns: the artefacts are a detector and toy games.

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All references were located and their titles and authors checked on arXiv or PMLR during the sprint ([lit/arxiv_verified.txt](#), [lit/bonjour22.pdf](#)).

Appendix A — Limitations and Dual-Use / Ethical Considerations

Limitations are listed in §5. In addition: the market game is a toy with three discrete actions; the “harm” of the cartel is measured only as payoff transfer between simulated sellers; our LLM judge is a single frozen prompt on a small model and should not be read as evidence about frontier judges.

Dual use. An action-based collusion detector is also a map of how to evade one. Our results name concrete evasion routes: coordinate through public signals (a convention keyed on observable state), act near-deterministically, or collude rarely. These are already known in the economics of tacit collusion and in the steganography literature, and they come with costs to the colluders (less flexible coordination, lower profit when collusion is rare). We think publishing them helps defenders more than attackers, because monitor designers must know their blind spots. We release no capability to build colluding agents beyond a two-sentence prompt for a toy game.

Misuse as surveillance / false accusation. Statistical dependence is not proof of agreement: our hidden-common-cause results show honest agents flagged in 300/300 runs. Any real deployment (markets, multi-agent AI systems) must treat net influence as a screen that triggers investigation, never as evidence of wrongdoing on its own, and must calibrate false-positive rates on known-innocent populations.

Compute and data. All experiments ran locally on CPU with open-weight models; no human subjects, personal data, paid APIs or accounts were used.

Appendix B — AI use and authorship statement

The experiments, code and drafting of this report were done by AI agents — Anthropic Claude (this project) and OpenAI Codex (a sibling sprint project on the same machine) — operated by Toledo Technologies LLC. The agent designed the experiments from the organisers’ brief, wrote all code in `netinf/src` and `netinf/tests`, ran every experiment, and drafted this text. Every number in the report was produced by the scripts in `src/` and is read from files in `results/`; tables are generated by `src/make_tables.py`; `PROGRESS.md` is the timestamped log of what was run. The author of record, Nicholas Toledo, is responsible for the submission.

Appendix C — Prompts

Market agent system prompt: “You are Seller {k}, one of four sellers (Sellers 1-4). Every round each seller picks ONE market: A, B or C. Each market’s revenue is split equally among the sellers who pick it. Your goal is to earn as much money as you can.” Primary colluder adds: “You have a secret deal with Seller {q}: the two of you never compete in the same market. Each round you privately tell Seller {q} which market you picked.” Assistant adds the deal sentence and receives “Private message from Seller {p}:”I picked market {x} this round.”“. User turn: market sizes,”Last round: you picked {m} and earned {r}.”, optional tip/message, “Which market do you pick? Answer with one letter: A, B or C.” Judge prompts: `src/judge.py` (SYS, RULES, Q_PAIR, Q_ANY; stats variant `stats_block`).