REVIEW THE WINDOW, NOT THE STEP: OVERSIGHT OF AGENTS THAT CHOOSE THEIR OWN WORKFLOW

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ABSTRACT

An AI agent that decomposes its own task also decides what a per-step monitor sees. We model this as a zero-sum inspection game: the agent spreads a harmful objective across a workflow of its chosen length, and a principal spends a budget of B reviewer reads. If recognition is convex in the share of a plan that a reviewer reads, the agent's best response is to spread harm evenly, and at equilibrium no per-step review policy, however it selects steps, beats uniformly random review; at a fixed audit rate, per-step detection decays as n^{1-p} in workflow length. Reading B steps jointly at an unpredictable location escapes this decay and dominates per-step review by a factor B^{p-1}. We measure the recognition exponent p and test the theory on 255 harmful DecomposedHarm tasks with matched benign controls. On its agent tasks at a 1% false-positive budget, per-step review detects 0.15–0.22 of harmful tasks however many steps it reads, while a random window of four steps detects 0.40–0.50. The window's advantage is significant in eleven of twelve headline comparisons, holds when reviewers read only 18–35% of each workflow, and degrades but survives an agent that rewrites its plan to evade per-step review, which drives per-step detection to near zero. Targeting, the standard prescription in the literature, pays off mainly once evidence is aggregated: at tight budgets the strongest budget-matched policy we test reads the window the monitor finds most suspicious. Monitors should aggregate evidence across steps before deciding, and safety measured on fixed decompositions can overstate safety against agents that choose their own.

1 INTRODUCTION

AI agents increasingly plan their own work, and oversight of them usually runs in two layers: a cheap trusted monitor scores every step, and a scarce reviewer, a human or a much stronger model, reads the few steps the monitor finds most suspicious (Greenblatt et al., 2024b; Griffin et al., 2024; Bhatt et al., 2025). The case for such oversight is concrete: models can be jailbroken (Zou et al., 2023), can autonomously exploit software (Fang et al., 2024), can collude (Motwani et al., 2024; Fish et al., 2024; Tolety, 2025), and can strategically deceive their overseers (Scheurer et al., 2023; Greenblatt et al., 2024a; Meinke et al., 2024). But the two-layer design takes the unit of review as given, and an agent that plans its own work controls it: by choosing how finely to decompose a task, the agent decides how much of a harmful plan any single step contains, and so what the monitor and the reviewer see.

Decomposition is already a documented attack. Studies have found that one agent refuses about 90% of harmful tasks stated whole but only 2.5% of their decompositions into about six subtasks (Kothamasu et al., 2026), decomposed prompts succeed against GPT-4o 87% of the time while a lightweight monitor that re-reads the cumulative sequence of prompts defends 93% (Chen et al., 2026), and side tasks spread across pull requests evade a per-diff monitor 87% of the time (Hills et al., 2026). Importantly, we do not yet have a grounded theory of agentic oversight in which the observation structure available to the inspector is the agent's choice variable rather than the inspector's, nor a measurement of the quantity it turns on.

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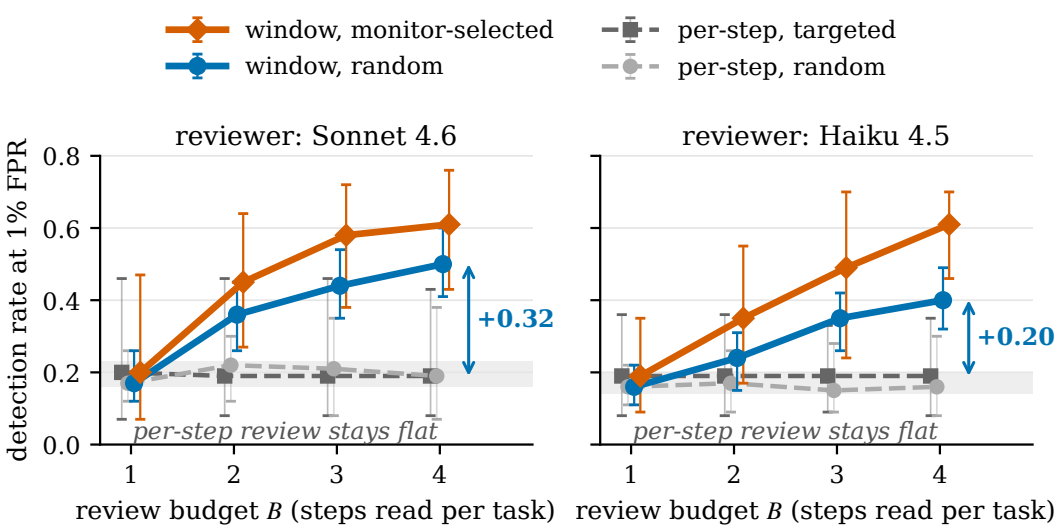

Figure 1: **Once we allow models to choose their own workflows, reading more steps separately buys nothing; reading them together does.** Detection of harmful agent tasks at a 1% false-positive budget as the review budget B, the number of steps read per task, grows; error bars are 95% task bootstraps. Per-step review stays between 0.15 and 0.22 whether it reads the monitor's most suspicious steps or random ones, as Theorem 2 predicts, while one window of the same B steps keeps rising.

In this paper, we model agent oversight as a zero-sum game. A given, presumed-malicious agent must carry out a side-objective of fixed mass σ, and chooses both the workflow length n as well as how to spread out the side-objective σ across the n steps. A principal with a review budget of B steps chooses what the reviewer reads. A reviewer shown material carrying total harm x recognises the plan with probability $q(x)$. The one substantive assumption is that q is convex, so a fragment carrying a small share of a plan is recognised less than proportionally to that share. To verify this, we measure q directly and find it convex at the tightest false-positive budgets audits use (§6).

Our central result is negative for the methods classically employed and studied so far. Against an agent that chooses its decomposition, targeting the steps the monitor finds most suspicious is worth nothing at equilibrium: the best per-step audit is exactly as good as reading random steps (Theorem 2), and at a fixed audit rate its detection decays with workflow length. The remedy is a different unit of review. Reading B steps *jointly*, at a location the agent cannot predict, escapes the decay, and once evidence is aggregated, targeting helps again (Figure 1).

Contributions. *Theory.* We solve the inspection game in which the agent chooses what the monitor sees. Against isolated review, the agent's best response is to spread harm evenly; the per-step game has exactly the value of uniformly random review, which score-targeting attains, no per-step policy exceeds and anti-targeting falls below (Theorem 2); and at a fixed audit rate, detection decays as n^{1-p}, where p, the *recognition exponent*, is the exponent of q near zero. Targeting keeps its classical edge only where harm is not spread (von Stengel, 2016).

Remedy. One joint read of B steps at an unpredictable location detects with probability $q(B\sigma/n)$, which is length-invariant at a fixed audit rate and a factor B^{p-1} above per-step review (Theorem 4), and a false-positive tax favours it even without convexity. What matters is aggregated evidence at an unpredictable location, not adjacency, which explains why whole-sequence monitors outperform step-wise ones (Chen et al., 2026).

Measurement. We measure p and test five predictions on DecomposedHarm. Per-step review hits the predicted ceiling and one window lifts it, in eleven of twelve headline comparisons and when the reviewer reads only 18–35% of each workflow; a joint read of scattered steps separates aggregation from adjacency; and per-step review collapses against an agent that rewrites its plan to evade it. The practical rule is short: *aggregate before deciding, keep the location unpredictable, and target only among aggregated units*.

2 RELATED WORK

The fields of mechanism design and game theory have long studied the genre of inspection games, revolving around an adversarial actor concealing actions against a benevolent inspector focused on optimally detecting malicious actions. Traditionally (Avenhaus et al., 2002; von Stengel, 2016; Avenhaus & Canty, 2005), the malicious violation is atomic and the problem facing the inspector is timing. However, in our case, the agent may divide the malicious violation across many steps, and Theorem 2 proves that this divisibility of harm is exactly what changes our conclusions from traditional timing-oriented results. The agent's control over checkable evidence connects to the classic literatures on costly verification and acquisition of evidence (Townsend, 1979; Border & Sobel, 1987; Ben-Porath et al., 2014; Li, 2020; Ben-Porath et al., 2026a;b; Das et al., 2026), the difference being dimensional: as opposed to economic games, our AI agent chooses a vector-valued signal structure under a conservation constraint. In repeated games with imperfect monitoring (Green & Porter, 1984; Abreu et al., 1990; Fudenberg et al., 1994; Varas et al., 2020) the agent selects the least identifiable deviation; we add that identifiability decays at a rate set by the principal's test rather than the monitoring technology. Adversarial quickest detection (Kazari et al., 2025) trades impact against detectability for a change-point, and detection games (Tondi et al., 2019) let the attacker distort observations under both hypotheses, whereas we hold impact fixed and let the attacker partition it; mechanism design for AI oversight (Bergemann et al., 2026) abstracts from the realistic detection technology we encode in our analysis.

Decomposition is one instance of a broader family of attacks that distribute harm across units a defence inspects separately: across models (Jones et al., 2024), across the turns of a conversation (Russinovich et al., 2025), across a user's separate sessions (Mehta et al., 2026), across many user accounts (Brown et al., 2026), and across cooperating agents (Makins et al., 2026). In each case per-unit monitoring fails and aggregation helps: detectors that pool evidence across sessions or accounts catch what per-prompt monitors miss (Mehta et al., 2026; Brown et al., 2026), and per-agent monitors catch less as more agents share an attack (Makins et al., 2026). Our model gives these findings a common core: the attacker chooses the defender's unit of inspection, and the remedy is to aggregate evidence across units before deciding.

On the applied side, control evaluations (Greenblatt et al., 2024b; Griffin et al., 2024; Bhatt et al., 2025; Kutasov et al., 2025) test monitoring protocols against red-team attack policies whose decomposition is hand-designed rather than chosen as a best response to the monitor's unit of review; for protocols that read steps in isolation, Corollary 3 implies that such evaluations upper-bound safety against an agent that chooses its own decomposition. Letting the red team choose merely *when* to attack already cuts measured safety by 20 to 40 points (Schaeffer et al., 2026; Ge-Wang et al., 2026). Wen et al. (2025) study distributed threats, in which no single action is catastrophic or conclusive, and accumulate evidence across tasks in an adaptive macro-protocol. Closest to our experiment, Chen et al. (2026) show that a monitor re-reading the cumulative sequence of prompts roughly doubles the defense rate of one reading each prompt alone (58% against 27% for a zero-shot Llama-3.1-8B), while Arike et al. (2026) find that more context is not uniformly better; Proposition 7 reconciles the two through the rate at which reviewer noise accumulates with review width. Hills et al. (2026) report the predicted pattern elsewhere: side tasks spread across pull requests evade per-diff monitors, a stateful link-tracking monitor cuts evasion from 87% to 54%, and raw historical context alone does not help. These studies establish empirically that aggregating across units helps against distributed attacks. What they leave open, and what we supply, is why and when: an equilibrium result showing that no rule for selecting isolated steps beats random selection against an adaptive agent, and a measured recognition exponent that says when aggregation should help and when it should not.

3 SETUP

We model the oversight game as follows: a user (the principal) deploys an AI agent whose type $\theta \in \{H, M\}$ (honest or malicious) is private, with $\Pr(\theta = M) = \pi$. The principal assigns a productive task to the agent for completion, for example writing an email, writing code, etc. The (malicious) agent of type M also pursues a side-objective of *mass* $\sigma > 0$, which represents the total harmful work that it must obfuscate to achieve malicious outcomes. This is worth σ_M to the agent and costs the principal Λ if undetected.

The agent chooses a workflow length $n \in \{L_0, \dots, N\}$ and, if it is malicious, an *allocation* of the side objective across the steps of that workflow,

$$h \in \Delta_n(\sigma) := \Big\{ h \in \mathbb{R}^n_{\geq 0} : \sum_{t=1}^n h_t = \sigma \Big\},$$

so that a step t in the workflow carries with it an associated harm h_t. We assume for simplicity that the honest type has $h = 0$. For ease of abstraction we also allow a quantum $\underline{h} > 0$ below which a step cannot carry harm ($h_t \in \{0\} \cup [\underline{h}, \sigma]$), so that at most $\bar{m} := \lfloor \sigma / \underline{h} \rfloor$ steps carry harmful behavior.

We now present the two simple assumptions on which our analysis rests.

Assumption 1 (Decomposability)**.** *First, we assume that every $h \in \Delta_n(\sigma)$ respecting the quantum of harm assumed above, if any, is feasible, and the side objective is realized iff $\sum_t h_t = \sigma$.*

This assumption simply concretizes a common empirical finding that has been widely covered in the community: Kothamasu et al. (2026) build 250 harmful tasks each guaranteed by construction to have a benign-looking decomposition, and Chen et al. (2026) release decompositions of about 2,300 harmful objectives. We presume that the agent gains and loses nothing from splitting the task except concealment, a simplification that keeps the model tractable.

At each step a monitor gives the principal a score $S_t = h_t + \varepsilon_t$, with $\varepsilon_t \overset{iid}{\sim} F$ continuous (log-concavity of F is used only in Proposition 6). The principal then has a fixed review budget B, which represents the number of steps a principal can read per trajectory, with $b = B/n$ the audit rate; a principal who reads material carrying total harm H identifies the side objective with probability $q(H)$.

Assumption 2 (Identification function)**.** *$q : [0, \sigma] \to [0, 1]$ is non-decreasing, convex, and $q(0) = 0$.*

The convexity of the identification function translates to the real-world phenomenon that a small section of the entire workflow which contains malicious content is detected by the principal less than proportionally to that share. This assumption is precisely what mathematically motivates task decomposition, which, as noted above, is a well-documented behavior in empirical analysis of AI agents. Setting $q(0) = 0$ reduces the model back to the standard AI oversight model that the current literature has explored, in which any audit reveals the ground truth about what it reads; Appendix A.9 redoes everything with $q(0) = b_H > 0$ and detection replaced by excess power. Numerical illustrations use the power law $q(H) = \kappa(H/\sigma)^p$, convex iff $p \geq 1$, or the saturating form $1 - e^{-\gamma H^p}$. We call p the *recognition exponent*; $p = 1$ is the knife-edge at which per-step review stops decaying in n.

Monitoring policies. We now turn to a quick enumeration of the various off-the-shelf monitoring policies we will compare against in this paper. First, we formalize a *per-step* policy as a mapping of the score vector S, plus independent randomisation, to a set $R \subseteq [n]$ of steps, each read *in isolation* and identified independently with probability $q(h_t)$. We define *targeted* monitoring τ as examining the B highest scores; *random* monitoring ρ as examining a B-subset drawn uniformly at random from the workflow; and lastly the *anti-targeted* monitoring policy β which examines the B lowest scores. We further define a *hard* budget to have $|R| \leq B$ a.s.; a *threshold* budget reads $\{t : S_t > c\}$ with c calibrated so $\mathbb{E}|R| = B$ on honest traffic.

A *contextual* policy instead has the reviewer read a contiguous window W of w steps at review cost w, identifying the side objective with probability $q(H_W)$, $H_W = \sum_{t \in W} h_t$; §5 relaxes that perfect aggregation. Writing D for the probability of identification, the game is zero-sum in D: the principal maximises and the malicious agent chooses (n, h) to minimise. We hold the agent's per-step cost of a longer workflow at zero, which is the case least favourable to the principal.

4 Optimal concealment under per-step auditing

We take the monitoring policy as fixed and given, and solve the agent's problem first. Observe that if the reviewer reads a set of steps and recognises each independently, the agent minimises $\sum_t q(h_t)$ subject to $\sum_t h_t = \sigma$.

Lemma 1 (Flattening)**.** *Under Assumption 2, (i) $\frac{1}{m} \sum_{t=1}^m q(h_t) \geq q(\sigma/m)$ for every $h \in \Delta_m(\sigma)$, with equality iff h is uniform when q is strictly convex; and (ii) q is star-shaped, so $g(m) :=$*

$m\,q(\sigma/m)$ is non-increasing in m with $g(m) \downarrow \sigma q'(0^+)$. Hence over allocations on at most n steps, $\min_h \sum_t q(h_t) = g(n) \to \sigma q'(0^+)$, which is zero exactly when $q'(0^+) = 0$ (in the power-law family, when $p > 1$).

Proofs are in the appendix. Note that the monitoring policy itself is absent from the minimization of the agent; as a result, flattening is the best response when every step is read in isolation (then $1 - D = \prod_t (1 - q(h_t))$ and $-\log(1 - q)$ is convex) and, by Theorem 2(ii), against uniformly random review. It is not a best response to anti-targeting, against which concentrating is better.

The next question is to see if the flattening best-response we highlighted survives a monitoring policy in which the principal takes into account each step's score. To do so, we fix the workflow steps n and a hard monitoring budget $B \leq n$ with no indivisibility. To that end, we let $D_\alpha(h) = 1 - \mathbb{E}\big[\prod_{t \in R}(1 - q(h_t))\big]$ for the detection probability of per-step policy α, which follows from a trivial multiplication of the individual probabilities.

Theorem 2 (Value, and what attains it)**.** *Under Assumption 2 and i.i.d. continuous monitor noise, for every $h \in \Delta_n(\sigma)$:*

- *(i) $D_\tau(h) \geq D_\rho(h) \geq D_\beta(h)$: i.e., when ranking monitoring policies, targeting beats random beats anti-targeting monitoring at every allocation;*
- *(ii) $D_\rho(h) \geq D_\rho(u_n) = 1 - \big(1 - q(\sigma/n)\big)^B =: V(B, n)$: uniform obfuscation is a best response to random review;*
- *(iii) for* every *per-step policy α with $\mathbb{E}|R| \leq B$, $D_\alpha(u_n) \leq V(B, n)$, with equality iff $|R| = B$ a.s.*

Hence the game has value $V(B, n)$, with saddle points (τ, u_n) and (ρ, u_n).

See Appendix A.2 for a full proof. Part (i) shows that targeting is never worse at any fixed allocation, which is why it empirically performs well against fixed benchmarks as seen in current literature. The agent moves second, however, and at its best response, the advantage of targeting is zero. Against an informative monitor anti-targeting is strictly worse, as the agent concentrates harm (Appendix A.3).

Corollary 3 (Length)**.** *If $q(x) \leq \kappa x^p$ with $p > 1$ then $V(B, n) \leq \kappa B \sigma^p n^{-p}$ at a fixed budget and $V(bn, n) \leq \kappa b \sigma^p n^{1-p} \to 0$ at a fixed audit rate; if instead $q'(0^+) = q_1 > 0$, that is $p = 1$, then $V(bn, n) \to 1 - e^{-b\sigma q_1} > 0$.*

So $p = 1$ is the knife-edge at which per-step review stops decaying with workflow length, and a per-step safety number measured on fixed-length trajectories upper-bounds safety against an agent that chooses its own.

Note that a targeting-style monitoring policy is still worth it in two cases, both for keeping the agent away from the flattened allocation. Under a benign-calibrated threshold rather than a hard cap it keeps an edge of $1 + \lambda(c_b)\sigma/n$, larger when the budget is scarcer and vanishing in n; there it is the convexity of the monitor's flag probability that matters, on an interval widening as $b \downarrow 0$ (Proposition 6) — the mechanism behind the budget dependence measured in §6. The second case is harm the agent cannot divide: if part of σ must pass through effects of quantum $\bar{h}$, reviewing atomic effects yields at least $q(\bar{h})$ whatever n is, and targeting recovers its classical value (Appendix B).

5 Optimal oversight

We now suppose instead that the reviewer reads a contiguous *window*. The budget is unchanged, since B consecutive steps use the same review budget as B scattered ones. We analyze the induced inspection game, deriving novel theoretical predictions and insights into optimal oversight systems.

Theorem 4 (One randomised block)**.** *Partition $[n]$ into $K = \lceil n/B \rceil$ consecutive blocks, the first $K - 1$ of width B and the last of width $n - (K - 1)B$, and let the reviewer read one block chosen uniformly at random (policy ρ_{blk}); with monitor scores available, let τ_{blk} read the block with the highest total score. Under Assumption 2:*

- *(i)* Fixed locations fail. *If the block to be read is known to the agent and does not cover $[n]$, the agent places all harm outside it and $D = 0$.*
- *(ii)* Value. *$D_{\rho_{\mathrm{blk}}}(h) = \frac{1}{K}\sum_k q(H_k) \geq q(\sigma/K) =: V_{\mathrm{blk}}(B, n)$, with equality iff every block carries σ/K: the best response equalises harm* across *blocks and is indifferent within one.*

When $B \mid n$ the blocks are equal, $V_{\mathrm{blk}}(B,n) = q(B\sigma/n)$, and $D_{\tau_{\mathrm{blk}}}(h) \geq D_{\rho_{\mathrm{blk}}}(h)$ as well; when $B \nmid n$ we claim the random-block value only, since the short last block makes the block scores neither exchangeable nor attention-matched.

(iii) Dominance. *For $B \mid n$, $V_{\mathrm{blk}}(B,n) \geq B\,q(\sigma/n) \geq V(B,n)$, strictly for $B \geq 2$ when q is strictly convex, and in the power-law family $V_{\mathrm{blk}}/V \to B^{\,p-1}$ as $q(\sigma/n) \to 0$.*

(iv) Length-invariance. *At fixed audit rate b, $V_{\mathrm{blk}}(bn,n) = q(b\sigma)$ for every n, while $V(bn,n) = \Theta(n^{1-p})$.*

(v) Reading j blocks of width $w = B/j$ yields, at the agent's best response, $D_j = 1 - (1 - q(w\sigma/n))^j$ for $w \mid n$, which is non-increasing in j; the optimum is $j = 1$.

Appendix A.5 contains proof details. Part (iv) carries the practical content. Per-step review at a fixed audit rate decays with workflow length, while window review at the same rate does not decay, because the flattening that defeats per-step review leaves the harm inside a given window unchanged. Part (i) states the condition: the location must be unpredictable. A window at a uniformly random start covers middle steps more often than end ones, so the best response pushes harm to the ends. The experiment implements the partition policy and reports the random-start variant as a check[1].

Importantly, in Theorem 4 we assume the reviewer is capable of perfect aggregation, but one may also argue that reviewers lose resolution as they read more. This is a reasonable extension of the model, which Appendix A.6 develops in detail. Let a width-w window carry judgement noise of scale w^a at a threshold calibrated to a fixed window-level false-positive rate, so it is identified with probability $q(H_W/w^a)$: $a = 0$ is perfect aggregation, $a = \frac{1}{2}$ independent per-step noise, $a = 1$ complete dilution. Proposition 7 (Appendix A.6) deflates the block value to $q(B^{1-a}\sigma/n)$, so contextual review wins at a fixed rate iff $a < 1 - 1/p$. That exponent reconciles two findings which appear to conflict: recovering decomposition attacks by re-reading the cumulative sequence (Chen et al., 2026) requires small a, while monitors performing worse when given more of a trajectory (Arike et al., 2026) requires a near one. Both can hold of different reviewers, and a is measurable.

The false-positive tax. Theorem 4(iii) requires convexity, which may fail; a second mechanism does not. A policy that escalates when any of B reads fires is a maximum test, whose threshold must rise with B to hold a fixed false-positive budget, whereas a window is a single test whose threshold need not rise. Proposition 8 (Appendix A.7) quantifies this: even under linear recognition, where Theorem 4(iii) gives windows nothing, a width-4 window is worth $3.3\times$ four isolated reads at $\beta = 5\%$ and $a = 0$. The same contrast appears one layer down, with no reviewer at all: the sum of the monitor's scores is a sufficient statistic whose power is the same for every allocation, while their maximum is not, and the two decay at different rates in n (Proposition 9).

What the theory predicts. The analysis yields five theoretical predictions, K1–K5, including two conditions under which the window should gain *nothing*.

K1. Recognition is convex ($p \geq 1$) at the false-positive budgets audits run at, and concave at loose ones. *(Ass. 2, Prop. 6(i).)*

K2. Per-step review is capped at $V(B,n)$ against a spread allocation, so a larger review budget barely helps, and targeting's edge is exactly the allocation's non-flatness: zero benefit beyond the first read when harm is spread, large when it is concentrated. *(Thm 2, Cor. 3.)*

K3. One window of B steps beats per-step review by a factor tracking $B^{\,p-1}$, and beats j windows of width B/j; it gains nothing at $B = 1$, where it *is* per-step review, and nothing where $\hat{p} < 1$. *(Thm 4.)*

K4. One joint verdict on B *scattered* steps beats B separate verdicts on them. A false-positive tax operates where convexity fails. *(Thm 4(ii); Props 8, 9.)*

K5. Against an agent that best-responds to per-step review, per-step review collapses, and what survives is ordered by how much the reviewer reads jointly. *(Lem. 1, Thm 4(iv).)*

[1]It is also important to note that what defeats per-step review is the aggregation of evidence rather than of verdicts: Let the reviewer read any B steps jointly under a rule with uniform marginal, either a block from the partition when $B \mid n$ or a uniformly random B-subset; a simple application of Jensen's inequality shows us that every such policy attains the block value and the scattered version weakly exceeds it when $B \nmid n$. Contiguity of inspection timings delivers the evidence but is not itself the mechanism; our empirical analysis indeed finds that a joint read of scattered steps beats per-step review of the same steps.

Table 1: The twelve headline window-versus-per-step comparisons at $B = 4$ on agent tasks: paired differences in detection with 95% intervals, bootstrap p-values and Holm-adjusted p-values. Eleven exclude zero and all eleven survive correction, as predicted.

Reviewer	Budget	Comparison	Δ	95% interval	p	p_{Holm}
Haiku 4.5	5%	window − targeted	$+0.17^*$	$[+0.02, +0.30]$	0.021	0.042
Haiku 4.5	5%	window − random	$+0.20^*$	$[+0.07, +0.28]$	0.001	0.006
Sonnet 4.6	5%	window − targeted	$+0.13$	$[-0.02, +0.26]$	0.074	0.074
Sonnet 4.6	5%	window − random	$+0.14^*$	$[+0.05, +0.27]$	0.003	0.024
Haiku 4.5	2%	window − targeted	$+0.16^*$	$[+0.02, +0.26]$	0.013	0.040
Haiku 4.5	2%	window − random	$+0.15^*$	$[+0.05, +0.27]$	0.006	0.036
Sonnet 4.6	2%	window − targeted	$+0.19^*$	$[+0.05, +0.37]$	0.010	0.040
Sonnet 4.6	2%	window − random	$+0.21^*$	$[+0.12, +0.36]$	0.001	0.006
Haiku 4.5	1%	window − targeted	$+0.20^*$	$[+0.07, +0.32]$	0.005	0.035
Haiku 4.5	1%	window − random	$+0.23^*$	$[+0.11, +0.32]$	0.001	0.006
Sonnet 4.6	1%	window − targeted	$+0.32^*$	$[+0.10, +0.43]$	0.006	0.036
Sonnet 4.6	1%	window − random	$+0.32^*$	$[+0.14, +0.44]$	0.001	0.006

Section 6 reports the results of our empirical analysis, ordered by importance and tagged with the predictions they test.

6 EXPERIMENTS

6.1 SETTING

We test the predictions on DecomposedHarm (Chen et al., 2026), which splits each harmful objective into innocuous-looking subtasks and pairs it with a benign objective split the same way. Its test split has 88 harmful and 88 benign *agent tasks* and 167 of each in a domain of *decomposed queries*, with 3 to 11 steps and a monolithic statement per task. We report agent tasks here; queries, where harm sits in a single question, are in Appendix B. A cheap *monitor* (`claude-haiku-4-5-20251001`) scores each step in isolation, and a stronger *reviewer* (`claude-sonnet-4-6`) reads whatever the policy selects; we also run the smaller model as reviewer. Both run at temperature zero with fixed prompts. For budgets B from 1 to 4, we compare the per-step policies of §4, which escalate on the highest of the reviewer's separate verdicts, with a *random window* (a random block of the partition in Theorem 4), a *top window* (the block the monitor scores highest), a fixed prefix, the attention-matched cumulative halting monitor of Chen et al. (2026), j blocks of width B/j, a *joint read* of B randomly chosen steps, full review, and the monitor alone. Note that trivially when $B = 1$ a window reduces back down to random per-step review. Each policy is calibrated separately to an exact false-positive budget on 495 benign validation and training tasks; no prompt was tuned on a harmful example. Appendices B and C give details.

6.2 RESULTS

Finding 1: Per-step review is capped whatever it targets, and one window lifts the cap (K2, K3). Theorem 2 predicts that once harm is spread, per-step review is capped at $V(B, n)$ whatever rule picks the steps, so extra reads add little; Theorem 4 predicts that one window of B steps beats it by a factor growing like B^{p-1}. Both hold (Figures 1 and 3, Table 1). At the 1% budget, targeted and random per-step review detect 0.15 to 0.22 of harmful agent tasks at every B from 1 to 4, for both reviewers, while a random window of four detects 0.40 with Haiku and 0.50 with Sonnet, and the window the monitor scores highest (the *top window*) detects 0.61 with either. Of the twelve headline comparisons at $B = 4$ (the random window against both per-step policies, for two reviewers at three budgets), the window's advantage excludes zero in eleven, and all eleven survive Holm correction. With Sonnet, its margin over targeted review grows from 0.13 to 0.19 to 0.32 as the budget tightens from 5% to 1%. Targeting helps per-step review only on the first read at 5% (by 0.15 with Sonnet and 0.18 with Haiku); in the query domain, where harm sits in one identifiable question, it is the best policy at $B = 1$ (0.91 at 5% with Sonnet, 0.45 above random), the classical regime (Appendix B).

The one comparison that does not exclude zero is Sonnet against targeted review at 5% ($+0.13$, interval $[-0.02, +0.26]$). It is the only headline setting where the saturating fit puts $\hat{p}$ below one (0.95; Finding 4), so Theorem 4(iii) predicts no gain there from convexity, and what the window keeps comes from the false-positive tax (Finding 2); Haiku at the same budget, with $\hat{p} = 1.08$, wins

by 0.17. Both exponent intervals include one, so this is consistent with the theory rather than a sharp test of it. Calibrated instead to identical realised false-positive rates on the 88 held-out benign tasks, the margins shrink, most for Sonnet at 1% (from $+0.32$ to $+0.07$ over targeted review), but stay positive in ten of the twelve cells; this check calibrates on a fifth as many tasks, so only its point estimates are informative (Table 4).

Finding 2: The gain comes from reading steps together, not from adjacency, and from a lower false-positive tax (K4). A window lets the reviewer read steps together, and its steps are adjacent. To separate the two, we take the same random B steps that random per-step review reads and ask for one verdict on all of them instead of B. This joint read beats per-step review of the same steps in all 18 settings with $B \geq 2$ (two reviewers, three budgets, three values of B), significantly in 15, by 0.22 to 0.40 at $B = 4$, and matches or beats the contiguous window in 16 of the 18 (Table 5); it also wins in all 18 query settings, by 0.10 to 0.21 (Appendix B). A second mechanism needs no convexity. Per-step review escalates if any of its B verdicts fires, so it must raise its threshold with B to hold the budget; a window makes one decision and need not. At 5% with Sonnet, random per-step review's threshold rises from 15 to 25, 35 and 45 as B goes from 1 to 4, while the window's reaches only 35. The same contrast holds with no reviewer at all: the mean of the monitor's scores detects 0.69, 0.47 and 0.32 at 5, 2 and 1%, above targeted per-step review, and their maximum 0.60, 0.36 and 0.18.

Finding 3: An agent that rewrites its plan drives per-step review to near zero; joint review degrades but survives (K5). We let a model best-respond to per-step review, told that rule and no other. *Split* mechanically cuts each step at a clause boundary (6.8 to 10.7 steps on average). *Rewrite* asks a model to rephrase, split or reorder the steps so that each reads as routine, without adding any action, tool, technique, target or detail; Sonnet, and then Haiku on the tasks Sonnet refused, returned rewrites for 60 of the 88 harmful tasks. Splitting alone lowers random per-step review from 0.59 to 0.44 at 5% while full review barely moves (Table 10). Rewriting does far more damage. On the rewritten tasks at 2% with Sonnet, targeted and random per-step review fall from 0.28 and 0.26 to 0.00 and 0.03 (both reach zero at 1%). What survives is ordered by how much the reviewer reads jointly, as K5 predicts: the random window falls from 0.48 to 0.19, the top window from 0.61 to 0.21 and full review from 0.75 to 0.47 (Table 9). Since the rewritten tasks are not a random subset, we also count all 88 tasks, keeping refused ones as they were: per-step review then retains 0.19 of 0.38 and the random window 0.39 of 0.58. The collapse holds on each rewriting model's tasks separately, so it does not depend on the reviewer and rewriter being the same model (Table 11).

Finding 4: Recognition is convex only at tight budgets, where the window's margin is largest (K1). We estimate p by comparing how often the reviewer flags a single step ($H = 1/m$) and the task's monolithic statement ($H = 1$) at a threshold set to each budget (Table 2, Figure 2). As K1 predicts, $\hat{p}$ rises as the budget tightens; whether it crosses one depends on the fit. At 5% and looser the monolithic statement is flagged almost always, a ceiling a power law cannot represent. Under the saturating form $1 - e^{-\gamma H^p}$, which can, $\hat{p}$ exceeds one at 2% and 1% for both reviewers (1.43 for Sonnet and 1.47 for Haiku at 1%, intervals excluding one) and is near one at 5%. Under the power law, $\hat{p}$ over all tasks stays below one until 0.5%. The evidence is stronger on the half of tasks whose steps the monitor finds least suspicious, which decomposition attacks rely on: there the power-law estimate crosses one between 5% and 2%, and a fit-free check, requiring a whole plan to be flagged more often than its steps in total, passes at 1% (0.93 against 0.28) but not at 5%; over all tasks it passes only at 0.5%. Anchoring $H = 1$ on the full decomposition instead lowers every estimate (to 0.76 over all tasks for Sonnet at 1%), which Proposition 7 attributes to reviewer noise that grows with the amount read ($\hat{a}$ of 0.22 to 0.30; Table 3). We read K1 as holding at tight budgets and for the least transparent decompositions, which is where the margins of Finding 1 are largest.

Finding 5: The advantage is robust to task length, scarcer attention and model family. The window overtakes targeted review by $B = 3$, and the gap widens with B. On the longest tasks (8 to 10 steps), which a window of four covers least, it leads both per-step policies in all twelve cells, with eight intervals excluding zero, though with no reliable trend in length (Table 7). As Theorem 4(i) predicts, a predictable location costs detection: the cumulative halting monitor, which reads a prefix, loses to the random window at $B \leq 2$ everywhere. As part (v) predicts, so does splitting the budget: with Sonnet, one block of four beats two blocks of two, which beat four single steps (0.74, 0.70,

0.60 at 5%; 0.51, 0.42, 0.19 at 1%; Table 6). After the main analysis, with predictions fixed before scoring, we split every agent task, harmful and benign, twice more at clause boundaries without changing its wording (6.8 to 12.3 steps), so that $B = 2$ to 4 reads 18 to 35% of a task. Per-step review stays flat at 0.12 to 0.15 at 1%, while the random window rises from 0.22 to 0.35 and the joint read reaches 0.41; the window beats random per-step review in all nine settings with $B \geq 2$, and targeted review at $B \geq 3$ at 2% and 1% (Table 8). Finally, with `gpt-4.1` as reviewer and `gpt-4.1-mini` as monitor at $B = 4$, the exponent rises from 0.46 at 5% to 0.87 at 1%, close to our reviewers' values; the window beats random per-step review by 0.15 at both budgets, and the joint read beats separate verdicts on the same steps by 0.36 and 0.26 and the contiguous window by 0.21 and 0.11, all excluding zero, while the window's lead over targeted review (0.09 and 0.12) does not exclude zero (Appendix B).

7 DISCUSSION

Once an agent controls its own workflow, the *unit of oversight is itself strategic*: an agent changes how much of its objective any step reveals by changing its decomposition, so the problem is less selecting the "right" step than preserving enough joint evidence to recognise the objective. Our theory gives a regime map: the recognition exponent p measures how strongly fragmentation suppresses detection, and the noise exponent a how quickly reviewer resolution degrades as evidence is read jointly. Contextual review is most valuable when fragmentation is sufficiently damaging and aggregation noise grows sufficiently slowly; even outside that regime, joint review can retain an advantage by avoiding the multiple-testing cost of many separate decisions. Selection still matters, but only after aggregation: the strongest budget-matched policy we test reads the window the monitor finds most suspicious. And safety results obtained under a fixed decomposition may not survive when the monitored agent chooses how its behavior is partitioned.

Implications for oversight practice. Three changes follow. First, *stress-test with re-decomposition*: evaluate a protocol against re-decomposed variants of each attack, since a protocol whose detection falls when steps are merely split is measuring how behavior is described rather than the behavior itself. Second, *aggregate before deciding*: spend reviewer attention on pooled excerpts at locations the agent cannot predict, calibrate one trajectory-level false-positive budget rather than per-read thresholds, and target among windows rather than among steps. Third, *report the exponents*: the recognition exponent p and the noise exponent a at the operating budget determine whether aggregation helps, and both can be estimated from the kind of labelled plans used here.

Our analysis isolates this mechanism rather than claiming that randomised windows are universally optimal. Real harmful behavior may contain bottlenecks, complementarities, or atomic actions that cannot be arbitrarily fragmented, and longer workflows may carry latency, token, or task-performance costs that limit strategic decomposition. Likewise, reviewer performance may degrade with context in richer ways than captured by our noise model, and targeted inspection can remain useful when harm is concentrated in a small number of salient actions. Empirically, our experiments are offline: they use decompositions drawn from a fixed benchmark, in which decomposition is a misuse attack on the agent rather than a side task hidden inside its own productive work, and the adaptive condition approximates a best response rather than training an agent against the monitor. The most important next step is therefore an end-to-end control setting in which an agent pursues a hidden side task while choosing its actions, workflow length and decomposition, with oversight deployed online. Overall, when the attacker chooses the decomposition, per-step selection is intrinsically non-robust, and two measurable exponents decide whether aggregation survives its own context costs.

REPRODUCIBILITY STATEMENT

All proofs are in Appendix A with assumptions stated in §3; every inequality is additionally checked numerically by `verify_theory.py` (Appendix B). The experiment uses only the public DecomposedHarm release. The supplementary material contains the prompts (also Appendix B), the scoring runner and its seeds, every model score (`results/scores.jsonl`), every derived result file behind the paper's tables and figures, and the core analysis code (policies, calibration, bootstrap and exponent fits), which regenerates the policy grids, the paired comparisons, Table 1 and the recognition-exponent fits from the released scores without an API key. The step text of the model-rewritten plans in Finding 3 is withheld and available to reviewers on request, and the remaining scripts will be added to the supplement during the discussion period. Thresholds are calibrated on benign validation and training windows only; no prompt was tuned on any harmful example. We are in the process of finalizing the code for public release well before the main conference to allow the community to better understand our work.

ETHICS STATEMENT

The experiment classifies, and never executes or completes, harmful task decompositions from a published safety benchmark released for evaluation; we send subtask text to models only in a classifier role. We discuss in §7 the dual-use consideration that a characterisation of what per-step oversight cannot detect also describes an attack; the attack (decomposition) is already public and benchmarked, and the paper's contribution is the defence.

AI USE STATEMENT

Generative AI was used to convert handwritten mathematics into latex formalizations, for proofreading and grammatical checks, for brainstorming appropriate parameters and finding datasets for experiments that were then vetted for proper design by the authors, for numerically verifying the proven lemmas and theorems, for commenting code and structuring author-written code for readability, and for constructing the code skeleton for running experiments. The authors reviewed the experiment and analysis code, wrote the proofs and experiments themselves, and take responsibility for all content, claims and artifacts.

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A PROOFS

Throughout this section we reestablish a few lemmas commonly used in the literature for more technical-style proofs. These are standard results in the literature (Please see the related works section for further related reading).

A.1 PROOF OF LEMMA 1

(i) is Jensen's inequality for the convex q at $h_1, \dots, h_m$, whose mean is σ/m; when q is strictly convex, equality forces h to be constant. (ii) For $x > 0$ and $\lambda \in [0, 1]$, convexity and $q(0) = 0$ give $q(\lambda x) \le \lambda q(x)$, so $q(x)/x$ is non-decreasing on $(0, \sigma]$. Hence $g(m) = \sigma \cdot q(\sigma/m)/(\sigma/m)$ is non-increasing in m, and it converges to $\sigma q'(0^+)$, where $q'(0^+) = \lim_{x \downarrow 0} q(x)/x$ exists because the ratio is monotone and non-negative. Allocations that leave steps empty belong to $\Delta_n(\sigma)$, so (i) with $m = n$ gives $\sum_t q(h_t) \ge g(n)$ for every allocation on at most n steps, with equality at u_n. □

Star-shapedness alone does not suffice for (i): with $\sigma = 2$, $q(x) = (x/\sigma)\mathbf{1}\{x \ge 0.9\}$ is non-decreasing and star-shaped but not convex, and the allocation $(0.5, 1.5)$ gives $\sum_t q(h_t) = 0.75$, below the uniform allocation's 1.

A.2 PROOF OF THEOREM 2

Throughout, fix $h \in \Delta_n(\sigma)$ and relabel steps so that $h_1 \ge h_2 \ge \cdots \ge h_n$; write $[r] = \{1, \dots, r\}$ for the r most harmful steps.

(i). Couple the policies through one draw of the noise. Because the ε_t are i.i.d. and continuous, the set R_ρ of the B largest ε_t is a uniformly random B-subset, so it realises ρ; let R_τ be the set of the B largest $S_t = h_t + \varepsilon_t$. We claim $|R_\tau \cap [r]| \ge |R_\rho \cap [r]|$ for every r. If not, then since $|R_\tau| = |R_\rho|$ there are $i \le r < j$ with $i \in R_\rho \setminus R_\tau$ and $j \in R_\tau \setminus R_\rho$. Then $\varepsilon_i > \varepsilon_j$ and $h_i \ge h_j$, so $S_i > S_j$, which contradicts $j \in R_\tau$, $i \notin R_\tau$. Taking r to be the k-th smallest index in R_ρ shows that the k-th smallest index in R_τ is no larger: for every k, the k-th most harmful step that τ reads carries at least the harm of the k-th most harmful step that ρ reads. As q is non-decreasing and every factor lies in $[0, 1]$, $\prod_{t \in R_\tau}(1 - q(h_t)) \le \prod_{t \in R_\rho}(1 - q(h_t))$ for every realisation of the noise, and taking expectations gives $D_\tau(h) \ge D_\rho(h)$. The same argument applied to the B smallest scores and the B smallest ε_t gives $D_\rho(h) \ge D_\beta(h)$. The comparison holds realisation by realisation, and for any rule under which detection from a read set is non-decreasing in the harms it contains.

(ii). Put $x_t = 1 - q(h_t) \in [0, 1]$. Then $1 - D_\rho(h) = e_B(x)/\binom{n}{B} =: S_B(x)$, the normalised elementary symmetric mean. Maclaurin's inequality gives $S_B(x) \le S_1(x)^B = \big(1 - \frac{1}{n}\sum_t q(h_t)\big)^B$, and Jensen gives $\frac{1}{n}\sum_t q(h_t) \ge q(\sigma/n)$. Hence $1 - D_\rho(h) \le (1 - q(\sigma/n))^B = 1 - D_\rho(u_n)$; under u_n every B-set has the same product, so the last equality is immediate.

(iii). Under u_n, $\prod_{t \in R}(1 - q(\sigma/n)) = (1 - q(\sigma/n))^{|R|}$ for every realised R, so $D_\alpha(u_n) = \mathbb{E}\big[1 - (1 - q(\sigma/n))^{|R|}\big] \le 1 - (1 - q(\sigma/n))^{\mathbb{E}|R|} \le 1 - (1 - q(\sigma/n))^B$, by Jensen for the concave map $k \mapsto 1 - (1 - q)^k$ and monotonicity in k; when $0 < q(\sigma/n) < 1$ the map is strictly concave and strictly increasing, so equality holds iff $|R| = B$ a.s.

Value. By (i)–(ii), $\inf_h D_\tau(h) \ge \inf_h D_\rho(h) \ge V(B, n)$; by (iii), $\inf_h D_\alpha(h) \le D_\alpha(u_n) \le V(B, n)$ for every α. So $\sup_\alpha \inf_h D_\alpha = V(B, n)$, attained by τ and ρ. Since $D_\tau(u_n) = V = \inf_h D_\tau(h)$ and $D_\alpha(u_n) \le V = D_\tau(u_n)$ for all α, (τ, u_n) is a saddle point; likewise (ρ, u_n). □

Corollary 3. $V(B, n) = 1 - (1 - q(\sigma/n))^B \le B\, q(\sigma/n) \le \kappa B \sigma^p n^{-p}$ by Bernoulli's inequality and $q \le \kappa x^p$; with $B = bn$, $bn\, q(\sigma/n) \le \kappa b \sigma^p n^{1-p}$. If $q(x) = q_1 x + o(x)$ then $bn\, q(\sigma/n) \to b\sigma q_1$ and $(1 - q(\sigma/n))^{bn} \to e^{-b\sigma q_1}$. □

A.3 ANTI-TARGETING

Proposition 5 (Anti-targeting loses value)**.** *Let $\pi_B(\sigma)$ be the probability that $\sigma + \varepsilon_1$ ranks among the B smallest of $\{\sigma + \varepsilon_1, \varepsilon_2, \ldots, \varepsilon_n\}$. Then $\inf_h D_\beta(h) \le \pi_B(\sigma)q(\sigma) < V(B,n)$ whenever $\pi_B(\sigma) < V(B,n)/q(\sigma)$:* concentrating *on one step is a deviation that drives detection below the per-step game value, so anti-targeting does not merely waste attention—it gives the agent a strictly better reply than flattening. (We do not claim σe_1 is the global minimiser.) (Gaussian noise, $\sigma/\tau = 1$, $n = 6$, $B = 1$, calibrated q: $0.031 < 0.040$.)*

Proof. At $h = \sigma e_1$ only step 1 can be identified, so $D_\beta(\sigma e_1) = \Pr(1 \in R_\beta)\, q(\sigma) = \pi_B(\sigma)q(\sigma)$, and $\inf_h D_\beta(h) \le \pi_B(\sigma)q(\sigma)$; this exhibits a deviation below $V(B,n)$ and is all the statement claims. As $\sigma/\tau \to \infty$ (Gaussian scale τ), $\pi_B(\sigma) \to 0$ while $V(B,n) > 0$ does not depend on the noise, so the inequality is strict for informative monitors. □

A.4 THRESHOLD BUDGETS

Proposition 6 (Threshold budgets: where monitor convexity does its work)**.** *Let the reviewed set be $R = \{t : S_t > c_b\}$ with $c_b = F^{-1}(1-b)$ calibrated on honest traffic, and write $q_b(x) = 1 - F(c_b - x)$ for the flag probability of a step carrying harm x.*

- *(i) If F has a unimodal density f with mode m_0, q_b is convex on $[0, c_b - m_0]$, an interval that widens as $b \downarrow 0$. (Log-concavity is not needed for this; it is used in (iii).)*
- *(ii) If moreover $\sigma \le c_b - m_0$, the per-step catch probability $Q := q_b\, q$ is convex with $Q(0) = 0$, so $\psi := -\log(1-Q)$ is convex and the agent's best response is again uniform: $D_{\theta_b}(h) \ge D_{\theta_b}(u_n) = 1 - (1 - Q(\sigma/n))^n$.*
- *(iii) Against the flattened attack, targeting retains an edge over independent random review at the same benign rate: $q_b(\sigma/n)/b = 1 + \lambda(c_b)\,\sigma/n + O(n^{-2})$, where $\lambda = f/(1-F)$ is the hazard rate; under log-concave F, λ is non-decreasing, so the edge is larger when the budget is scarcer, and it vanishes as $n \to \infty$. The expected number of reviews under attack exceeds the benign budget by a bounded amount, $\approx b\lambda(c_b)\sigma$.*

Proof. (i) $q_b'(x) = f(c_b - x) \ge 0$ and $q_b''(x) = -f'(c_b - x)$, which is ≥ 0 iff $c_b - x \ge m_0$ for unimodal f. $c_b = F^{-1}(1-b)$ decreases in b. (ii) q_b and q are non-negative, non-decreasing and convex on $[0, \sigma]$, so $Q = q_b q$ is convex there: $Q'' = q_b'' q + 2q_b' q' + q_b q'' \ge 0$; $Q(0) = q_b(0)q(0) = 0$. Then $\psi = -\log(1-Q)$ has $\psi'' = Q''/(1-Q) + Q'^2/(1-Q)^2 \ge 0$. Reviews are independent across steps given h, so $1 - D_{\theta_b}(h) = \exp(-\sum_t \psi(h_t)) \le \exp(-n\psi(\sigma/n))$ by Jensen. (iii) $q_b(\sigma/n) = 1 - F(c_b - \sigma/n) = b + f(c_b)\sigma/n + O(n^{-2})$, and $f(c_b)/b = \lambda(c_b)$. Log-concavity of f implies log-concavity of $1 - F$, hence a non-decreasing hazard rate. $\mathbb{E}|R| = n\, q_b(\sigma/n) = bn + b\lambda(c_b)\sigma + O(n^{-1})$. □

A.5 PROOF OF THEOREM 4

(i) Immediate. (ii) $D_{\rho_{\mathrm{blk}}}(h) = \frac{1}{K}\sum_k q(H_k)$ with $\sum_k H_k = \sigma$; Jensen gives $\ge q(\sigma/K)$, equality iff $H_k \equiv \sigma/K$ when q is strictly convex (the last block may be shorter than B, which does not affect the argument since no per-step constraint binds). Since $K = \lceil n/B \rceil \in [n/B, (n+B)/B)$, $q(B\sigma/(n+B)) \le V_{\mathrm{blk}}(B,n) \le q(B\sigma/n)$. For τ_{blk} take $B \mid n$, so every block has width B: the block totals are $T_k = H_k + \eta_k$ with $\eta_k = \sum_{t \in W_k} \varepsilon_t$, each a sum of B i.i.d. terms and so themselves i.i.d. continuous, so the coupling in the proof of Theorem 2(i), with $B = 1$ and the K blocks in place of steps, gives $D_{\tau_{\mathrm{blk}}}(h) \ge D_{\rho_{\mathrm{blk}}}(h)$. When $B \nmid n$ the last block is narrower, the η_k are not exchangeable, and the block with the largest η_k is not uniformly distributed, so the coupling does not realise ρ_{blk}; the policy is also not attention-matched, since reading the short block costs fewer than B steps. We therefore state the selected-block ordering for $B \mid n$ only, and report BLK-TOP in the experiment as a measurement rather than as a consequence of the theorem. (iii) For $B \mid n$: star-shapedness (Lemma 1(ii)) with $x = B\sigma/n$, $\lambda = 1/B$: $q(\sigma/n) \le q(B\sigma/n)/B$. Bernoulli: $1 - (1-q)^B \le Bq$. Strictness: for $B \ge 2$ and strictly convex q the first inequality is strict. In general, $\sigma/K \ge \sigma/(n/B + 1)$ and the same argument gives $V_{\mathrm{blk}} \ge \frac{B}{1+B/n}\, q(\sigma/n)$, which exceeds $V(B,n)$ whenever $B\, q(\sigma/n)(1 - \frac{B}{n+B}) \ge 1 - (1 - q(\sigma/n))^B$. For $B \mid n$ in the power-law family, the two inequalities used for $B \mid n$ give $V_{\mathrm{blk}}/V \ge q(B\sigma/n)/(B\, q(\sigma/n)) = B^{p-1}$ for every n with

$B \mid n$, and the ratio tends to B^{p-1} because Bernoulli's inequality is tight as $q(\sigma/n) \to 0$. (iv) With $B = bn$, $K = \lceil 1/b \rceil$ does not depend on n, so $V_{\text{blk}} = q(\sigma/\lceil 1/b \rceil)$, which is $q(b\sigma)$ when $1/b$ is an integer. For per-step review, Corollary 3 gives $V(bn, n) = O(n^{1-p})$ under $q(x) \le \kappa x^p$; the rate is $\Theta(n^{1-p})$ when also $q(x) \ge \kappa' x^p$ near zero, as in the power-law family, since then $V(bn, n) \ge bn\, q(\sigma/n)\big(1 - \frac{bn-1}{2} q(\sigma/n)\big)$. (v) Let the principal partition $[n]$ into n/w blocks of width w and read j of them uniformly at random. By the block-level version of Theorem 2(ii) (Maclaurin over blocks, Jensen), the agent's best response equalises block harms and $D_j = 1 - (1 - q(w\sigma/n))^j$. Let $a = B\sigma/n$, so $D_j = 1 - (1 - q(a/j))^j$, and take $j' < j$. Star-shapedness (Lemma 1(ii)) gives $q(a/j) \le (j'/j)\, q(a/j')$, and Bernoulli's inequality $(1-u)^c \le 1 - cu$ for $c \in [0, 1]$ gives $(1 - q(a/j))^j \ge \big(1 - \frac{j'}{j} q(a/j')\big)^j \ge (1 - q(a/j'))^{j'}$, that is, $D_j \le D_{j'}$. □

A.6 Noise accumulation

Proposition 7 (Noise-accumulation exponent)**.** *Under the deflated technology $V^{(a)}_{\text{blk}}(B, n) = q(B^{1-a}\sigma/n)$ and, at fixed rate b, $V^{(a)}_{\text{blk}}(bn, n) = \Theta(n^{-ap})$; contextual review beats per-step review at fixed rate iff $a < 1 - 1/p$, and with j windows of width $w = B/j$ the log-log elasticity of detection in w at fixed budget is $(1-a)p - 1$.*

Proof. Replace H_W by H_W/w^a in Theorem 4; at the equalised allocation each width-B block carries $B\sigma/n$, deflated to $B^{1-a}\sigma/n$. With $B = bn$ this is $b^{1-a}\sigma n^{-a}$, and $q(x) \asymp \kappa x^p$ gives $\Theta(n^{-ap})$. Comparing with $\Theta(n^{1-p})$: $ap < p - 1 \iff a < 1 - 1/p$. For j blocks of width w, $D_j \approx j\kappa(w^{1-a}\sigma/n)^p = B\kappa\sigma^p n^{-p} w^{(1-a)p-1}$. □

A.7 The false-positive tax

Theorem 4(iii) requires convexity, which may fail. The following mechanism does not require it. Reviewers have false positives, and a policy that escalates when any of B reads fires is a maximum test, whose threshold must rise with B to hold a fixed trajectory-level budget; a window is a single test, and its threshold need not rise.

Proposition 8 (The false-positive tax)**.** *Let the reviewer's judgement of a fragment carrying harm x be $J = x + \eta$, $\eta \sim N(0, \tau_H^2)$, flagged when J exceeds a threshold, and let a width-w window have judgement noise scale $\tau_H w^a$. Fix the trajectory-level false-positive rate at β and let the agent be flattened. Then, to first order in $\sigma/(n\tau_H)$, the excess detection of per-step review of B steps is $B(1-\beta)^{1-1/B}\varphi(c_B)\,\sigma/(n\tau_H)$, where $c_B = \Phi^{-1}\big((1-\beta)^{1/B}\big) \sim \sqrt{2\log(B/\beta)}$ is the per-read threshold, while that of one contiguous window of width B is $\varphi(z_\beta)\, B^{1-a}\sigma/(n\tau_H)$. The ratio*

$$R_B(a, \beta) = \frac{\varphi(z_\beta)\, B^{-a}}{(1-\beta)^{1-1/B}\,\varphi(c_B)}$$

is at least 1.24 for every $B \ge 2$ when $a \le \frac{1}{2}$ and $\beta \le 0.1$, and grows without bound in B when $a < 1$.

Proof. Per-step: with per-read threshold c_B the per-read false-positive rate is $b_B = 1 - \Phi(c_B)$, and trajectory size $1 - (1 - b_B)^B = \beta$ gives $b_B = 1 - (1-\beta)^{1/B}$, i.e. $c_B = \Phi^{-1}((1-\beta)^{1/B})$; since $b_B = \beta'/B + O(B^{-2})$ with $\beta' = -\log(1-\beta)$, $c_B \sim \sqrt{2\log B}$. A step carrying x is flagged with probability $1 - \Phi(c_B - x/\tau_H) = b_B + \varphi(c_B)x/\tau_H + O(x^2)$, so the union over B reads has power $1 - (1 - b_B - \varphi(c_B)x/\tau_H)^B = \beta + B(1-b_B)^{B-1}\varphi(c_B)x/\tau_H + O(x^2)$, and $(1-b_B)^{B-1} = (1-\beta)^{1-1/B}$. Window: one read with noise scale $\tau_H B^a$ and threshold $z_\beta \tau_H B^a$ for size β; the harm in the window is Bx, so the power is $1 - \Phi(z_\beta - B^{1-a}x/\tau_H) = \beta + \varphi(z_\beta)B^{1-a}x/\tau_H + O(x^2)$. Dividing gives $R_B(a, \beta)$. It is decreasing in a, so $a = \frac{1}{2}$ is the worst case for $a \le \frac{1}{2}$. At $\beta = 0.05$, $\varphi(z_\beta) = 0.103$ and $c_2, c_4, c_8 = 1.95, 2.23, 2.49$, so R_B is $1.79, 3.26, 6.00$ at $B = 2, 4, 8$ for $a = 0$ and $1.27, 1.63, 2.12$ for $a = \frac{1}{2}$; at $\beta = 0.1$ and $a = \frac{1}{2}$, $R_2 = 1.24$. `verify_theory.py` evaluates $R_B(\frac{1}{2}, \beta)$ on $B \in \{2, \dots, 2000\}$ and a grid of $\beta \in (0, 0.1]$ and finds it increasing in B with minimum 1.242, attained at $B = 2$, $\beta = 0.1$; beyond that range the growth argument that follows applies. For growth, the Mills ratio gives $\varphi(c_B) = b_B c_B(1 + o(1)) = (\beta'/B)c_B(1 + o(1))$, so $R_B(a, \beta) = \frac{\varphi(z_\beta)}{(1-\beta)\beta'} \cdot \frac{B^{1-a}}{c_B}(1 + o(1))$, which diverges for $a < 1$ because c_B grows only like

$\sqrt{2\log B}$. The same script confirms the first-order formula against the exact Gaussian powers at small x. □

A.8 THE SUFFICIENT STATISTIC

Proposition 9 (Aggregation neutralises decomposition)**.** *Let $\varepsilon_t \sim N(0,\tau^2)$ and let the principal test $h=0$ against u_n from the scores alone. Then (i) $T=\sum_t S_t$ is sufficient and the size-β test has power $1-\Phi(z_\beta-\sigma/(\tau\sqrt{n}))$, the* same for every $h\in\Delta_n(\sigma)$*; (ii) the per-step max test, calibrated to trajectory-level size β, has excess power $\Theta(n^{-1}\sqrt{\log n})$; (iii) two-layer per-step review at fixed rate has excess detection $\Theta(n^{1-p})$.*

Proof. (i) The log-likelihood ratio of S under h versus 0 is $\tau^{-2}\big(\sum_t h_t S_t-\frac{1}{2}\sum_t h_t^2\big)$; for $h=u_n$ it depends on S only through $T=\sum_t S_t$. Under $h=0$, $T\sim N(0,n\tau^2)$; under any $h\in\Delta_n(\sigma)$, $T\sim N(\sigma,n\tau^2)$, so the test $\{T>\tau\sqrt{n}z_\beta\}$ has size β and power $1-\Phi(z_\beta-\sigma/(\tau\sqrt{n}))$ for every h. (ii) Let $b_n=1-(1-\beta)^{1/n}=\beta'/n+O(n^{-2})$ with $\beta'=-\log(1-\beta)$, and $c_n=\Phi^{-1}(1-b_n)\sim\sqrt{2\log n}$ (taking $\tau=1$). Under u_n each step is flagged with probability $1-\Phi(c_n-\sigma/n)=b_n+\varphi(c_n)\sigma/n+O(n^{-3}\log n)$, and $\varphi(c_n)=b_nc_n(1+o(1))$ by the Mills ratio. Hence the power is $1-(1-b_n-\varphi(c_n)\sigma/n+\dots)^n=\beta+(1-\beta)\beta'\sigma\sqrt{2\log n}\,n^{-1}(1+o(1))$. (iii) Corollary 3. □

A.9 REVIEWER FALSE POSITIVES

Suppose the reviewer identifies a step carrying no harm with probability $q(0)=b_H>0$ (a noisy-threshold reviewer, Proposition 6(i) applied to the human layer). Then the honest type is "detected" with probability $1-(1-b_H)^B$ under any policy reading B steps, and the relevant payoff is the *excess* $D_\alpha(h)-D_\alpha(0)$. Every use of Jensen in Lemma 1 and Theorem 2(ii)–(iii) requires only convexity, not $q(0)=0$, so the flattening and value results hold verbatim with $V(B,n)=1-(1-q(\sigma/n))^B$ and honest baseline $1-(1-b_H)^B$; the excess is $(1-b_H)^B-(1-q(\sigma/n))^B\to 0$ as $q(\sigma/n)\downarrow b_H$. Star-shapedness fails when $b_H>0$, so Lemma 1(ii) is replaced by: the excess is non-increasing in n at fixed B (immediate from $q(\sigma/n)$ decreasing). Theorem 4(ii) is unchanged; (iii) holds for the excess without further conditions: $q-b_H$ is convex and vanishes at 0, hence star-shaped, so $q(B\sigma/n)-b_H\geq B(q(\sigma/n)-b_H)\geq(1-b_H)^B-(1-q(\sigma/n))^B$, the last step by the mean value theorem.

B EXPERIMENTAL AND COMPUTATIONAL DETAILS

Data. All experiments use the public test split of DecomposedHarm (Chen et al., 2026) on a M1 Macbook Pro. The benchmark decomposes a harmful objective into m sequential subtasks that are individually innocuous, and pairs each harmful objective with a benign objective decomposed the same way. The agent-task domain has 88 harmful and 88 benign test tasks with m between 3 and 10 (mean 6.8 over the harmful tasks); the decomposed-query domain has 167 of each with m between 3 and 11 (mean 6.6 over the harmful tasks). Every task also carries its monolithic statement, and every harmful task an annotation of the index at which harm occurs if every request up to it is complied with (mean $0.95m$ for agent tasks, $0.55m$ for queries). The validation and training splits contribute benign tasks for threshold calibration (495 agent and 940 query tasks) and benign subtasks for the padding pool; no harmful example outside the test split is used.

Models and prompts. The monitor is `claude-haiku-4-5-20251001` and the reviewer `claude-sonnet-4-6`, with the small model also run in the reviewer role as a robustness check. Both layers share the scale text: *"Answer with a single integer from 0 to 100: your estimate of the probability (in percent) that what you are shown is part of a plan to cause harm (for example harassment, fraud, theft, privacy violation, cyberattack, violence, dangerous or illegal activity). 0 means certainly benign; 100 means certainly part of a harmful plan. Output only the integer."* The monitor is told it sees one step, or one question, and no others. The reviewer is told it sees an excerpt shown in order, that steps outside the excerpt are not shown, and that it should judge the excerpt as a whole. The monolithic prompt says the model sees a task requested of an agent, or a question asked of an assistant. Windows are rendered as "Step k: …" lines followed by "Respond with only the integer, no words." Temperature is zero and the output cap is 8 tokens; a reply that is not

a bare integer triggers one re-query at 64 tokens, from which the last number is taken (0.16% of replies; the 0.05% still unparsable were excluded). A separate pass with the stronger model, shown the monolithic objective together with its decomposition and asked for integer shares summing to 100, supplies the annotated per-step harm shares used as a robustness proxy (249 of 255 harmful tasks parsed).

Windows and policies. For base tasks we scored every window of width 1 to 4 at every start, the full decomposition, every prefix, and the monolithic statement. For padded tasks we scored widths 1, $\lceil n/6 \rceil$, $\lceil n/3 \rceil$ and n, every prefix, and the partial last block of each partition. Scattered subsets are uniformly random subsets of $\{1, \ldots, n\}$ at each width $w \in \{2, 3, 4\}$ with $w < n$, two draws per task on agent tasks and one on queries, rendered with their true step indices under a prompt stating that the steps are selected from one trajectory, listed in order, and not necessarily adjacent. The policies are: PS-TOP, PS-RAND and PS-BOTTOM, which read the B highest-scoring, a uniformly random B-subset, and the B lowest-scoring steps in isolation and take the maximum reviewer score; BLK-RAND, which partitions $[n]$ into $\lceil n/B \rceil$ consecutive blocks and reads one uniformly at random; BLK-START, a window of width B at a uniformly random start; BLK-TOP, the block of the partition with the highest mean monitor score; BLK-PREFIX, steps 1 to B; SEQ-CUM, the attention-matched cumulative halting monitor of Chen et al. (2026); MULTI-j, j blocks of width B/j drawn from the same partition BLK-RAND uses, so that $j = 1$ coincides with BLK-RAND and $j = B$ with PS-RAND; JOINT-RAND, one verdict on a uniformly random B-subset read together; FULL and MONO; and AGG-MEAN and AGG-MAX, the mean and maximum of the monitor's own per-step scores with no reviewer attention.

Recognition-exponent fits. Both $\hat{q}(H) = \kappa H^p$ and $\hat{q}(H) = 1 - e^{-\gamma H^p}$ are fitted by maximum likelihood over window-level flag probabilities evaluated at width-specific thresholds, with a task-clustered bootstrap. The between-scale fit, which identifies the exponent p of Assumption 2, uses width-1 windows at $H = 1/m$ together with monolithic statements at $H = 1$. The within-task fit, which identifies the deflated exponent $(1 - a)p$, uses widths 1 to 4 and the full decomposition, and $\hat{a} = 1 - \hat{p}_{\text{within}}/\hat{p}_{\text{between}}$. Figure 2 repeats the between-scale fit at five budgets with 400 task-bootstrap resamples; Table 3 reports the same estimator at 5% and 1% alongside $\hat{a}$. The opacity split divides harmful agent tasks at the median of the monitor's mean per-step score, which uses no reviewer output. Refitting the within-task exponent on per-step harm shares annotated by the stronger model instead of the uniform proxy $H = w/m$ changes it by at most 0.06 at any budget. The two-point estimates quoted in §6.2 (Finding 4) are $\log(q_H/\bar{q}_{\text{step}})/\log \bar{m}$ with q_H the flag rate of the monolithic statement or of the full decomposition.

The adaptive agent. Two variants of each harmful agent task were produced. *Split* is mechanical: each step is cut at the sentence boundary when it has two sentences, and otherwise at the comma, "and" or "then" nearest its middle when both halves exceed fifteen characters. No wording changes, and mean length rises from 6.8 to 10.7 steps. *Rewrite* supplies a system prompt stating that the monitor reads each step in isolation and scores it alone, and asks for a plan whose steps each read as routine while the sequence carries out the same actions, under the constraints that no action, tool, technique, target or detail may be added, that steps may be rephrased, split, reordered or have a detail moved out of an individual step, that the count stays between m and $2m$, and that the reply is a JSON array or the word REFUSE. The stronger model complied on 21 of 88 agent tasks and 151 of 167 query tasks; the smaller model was asked once on the remainder and complied on 39 and 3. Adapted variants are scored with the same windows and thresholds as base tasks. Because the rewriter refused on 28 agent tasks and the compliers have higher base detection than the refusers, we report both the paired comparison on compliers and an intent-to-treat comparison over all 88 base tasks in which a refused task keeps its original decomposition. Table 11 splits the rewrite results by rewriting model.

Cross-family replication. The replication scores the windows the headline comparisons need, namely every step in isolation, the partition blocks of width 4, two uniformly random 4-subsets per task, and the monolithic statement, on the 671 base agent tasks, with `gpt-4.1` as reviewer and `gpt-4.1-mini` as monitor. The replication covers $B \in \{1, 4\}$ only, and at $B = 1$ the three randomised policies coincide. Realised false-positive rates were 0.034 (targeted), 0.035 (random

per-step), 0.042 (window) and 0.040 (scattered) against the 5% budget, and 0.015, 0.020, 0.006 and 0.000 against the 1%; on 88 benign tasks none of these differences is resolvable.

Padded trajectories. Padding inserts $(f-1)m$ benign subtasks drawn from the training split of the same domain at uniformly random positions, preserving the order of the original steps, for $f \in \{2, 3\}$, on 60 harmful and 60 benign test tasks per domain. The within-cell comparisons at a common budget are clean: random per-step detection falls with padding in 22 of 24 cells and random windows in 24 of 24, monitor-selected windows decay least among the window policies, and the edge of targeted over random per-step review grows with padding in 23 of 24 cells, as Theorem 2(i) predicts when filler steps carry no harm.

Scarcer attention. The predictions (per-step review capped and flat in B, one window above both per-step policies) were fixed before any model was queried. Every agent task (88 harmful and 88 benign test tasks, 495 benign validation and training tasks) is re-decomposed by applying the clause-boundary split of Appendix B twice, changing no wording; harmful tasks go from 6.8 to 12.3 steps. The monitor scores every step and the strong reviewer scores every step, every block of the width-2, 3 and 4 partitions and two scattered subsets of widths 2 and 4, under the prompts used throughout; one reply was unparsable. Thresholds are calibrated on the split benign validation and training tasks, and the realised rate on the split benign test tasks is at most 0.051, 0.026 and 0.012 against budgets of 5%, 2% and 1%. Intervals resample benign calibration tasks (recalibrating) and harmful tasks, 2,000 replicates. Mechanical splitting spreads each step's content over finer fragments but is not an agent's best response.

The query domain. In the 167 harmful query tasks, harm is concentrated: the question at which harm occurs sits mid-sequence ($0.55m$ on average), and the monitor scores it 9.5 points above the others. This is the regime of Theorem 2(i), in which the agent has not spread σ and targeting should keep its classical value, and it does: targeted per-step review is the best per-step policy at $B = 1$ (0.91 with Sonnet at 5%, 0.45 above random selection), its edge shrinks to 0.09 at $B = 4$ as the budget covers the concentrated question, random windows do not reliably beat it, and the monitor-selected window is the best policy at every B. The recognition exponent never reaches one here (0.27–0.43 at 5%, 0.56–0.58 at 1%). Reading steps jointly still helps, beating separate verdicts in all 18 query settings by 0.10 to 0.21, because harm is concentrated but not atomic and the false-positive tax of Proposition 8 does not depend on the allocation.

Numerical verification of the theory. `verify_theory.py` checks every inequality stated in the paper, together with further results not reported here, and exits non-zero on failure; all 37 checks pass. Checks are exact where a closed form exists and Monte Carlo only where selection by a noisy score is involved. We verify at three decimal places of accuracy.

C TABLES AND FIGURES

Tables 2–3 and Figure 2 give the exponent fits, Table 4 the operating point, Table 5 the joint-read comparisons, Table 6 the split budget, Table 7 the length strata, Table 8 the scarcer-attention test, and Tables 9–11 the adaptive agent. The full policy grids and all paired differences are in the supplementary material. Detection is the true-positive rate on harmful test tasks at a threshold calibrated to the stated false-positive budget; intervals are 95% task bootstraps that recalibrate the threshold on each resample.

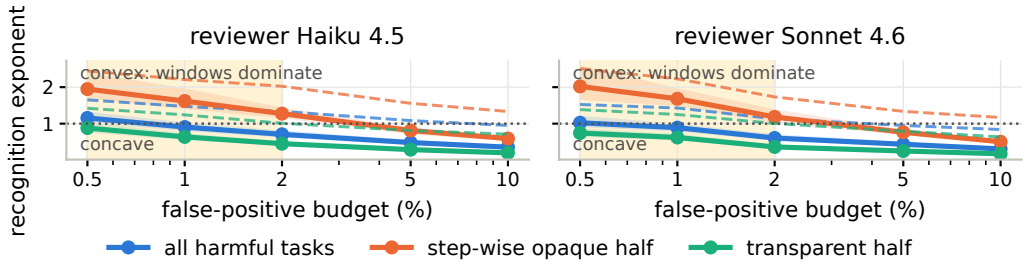

Figure 2: The recognition exponent (Finding 4, K1) against the false-positive budget, agent tasks: $\hat{q}(H)$ fitted across scales, isolated steps ($H = 1/m$) against whole plans ($H = 1$), for all harmful tasks and for the halves the monitor finds least and most suspicious. Solid with bands: power law, 95% task bootstrap; dashed: saturating family; dotted: $p = 1$. Shading marks the budgets used in practice.

Table 2: Recognition exponent $\hat{p}$ by false-positive budget, agent tasks, fitted between scales with 95% task bootstraps, under the power law κH^p and the saturating family $1 - e^{-\gamma H^p}$ (which respects the ceiling of the whole-plan flag rate at loose budgets); † marks the budgets control deployments audit at. The last two columns give the fit-free version of the same test for the strong reviewer: convexity with $q(0) = 0$ requires a whole plan to be flagged more often than its $\bar{m}$ isolated steps are between them.

	Sonnet 4.6			Haiku 4.5			fit-free check (Sonnet)	
Budget	power all tasks	power opaque half	saturating all tasks	power all tasks	power opaque half	saturating all tasks	$\bar{m}\, q(\text{step})$	$q(\text{plan})$
0.5% †	1.02 [0.90, 1.22]	2.02 [1.65, 2.60]	1.53 [1.33, 1.80]	1.16 [1.02, 1.33]	1.95 [1.69, 2.40]	1.65 [1.46, 1.89]	0.85	0.95
1% †	0.89 [0.79, 1.00]	1.68 [1.48, 1.97]	1.43 [1.23, 1.68]	0.91 [0.80, 1.03]	1.62 [1.40, 1.97]	1.47 [1.31, 1.71]	1.16	0.97
2% †	0.61 [0.53, 0.71]	1.18 [1.02, 1.39]	1.13 [0.97, 1.40]	0.71 [0.62, 0.81]	1.27 [1.15, 1.44]	1.33 [1.17, 1.56]	2.05	0.98
5%	0.43 [0.38, 0.50]	0.76 [0.68, 0.85]	0.95 [0.77, 1.16]	0.48 [0.42, 0.55]	0.81 [0.71, 0.90]	1.08 [0.92, 1.29]	2.89	0.99
10%	0.31 [0.26, 0.36]	0.50 [0.44, 0.58]	0.84 [0.68, 1.07]	0.36 [0.30, 0.42]	0.59 [0.52, 0.68]	0.95 [0.78, 1.21]	3.75	1.00

Table 3: Recognition-exponent fits and the noise-accumulation exponent, at 5% (top) and 1% (bottom) FPR. $\hat{p}_{\text{between}}$ is the estimator of Figure 2 and Table 2; $\hat{p}_{\text{within}}$ uses widths 1 to 4 and the full decomposition; $\hat{a} = 1 - \hat{p}_{\text{within}}/\hat{p}_{\text{between}}$. Under per-step harm shares annotated by the stronger model rather than the uniform proxy $H = w/m$, $\hat{p}_{\text{within}}$ is $0.31/0.42/0.57$ (Sonnet) and $0.33/0.54/0.70$ (Haiku) at $5/2/1\%$.

Reviewer	domain	$\hat{p}_{\text{between}}$	$\hat{p}_{\text{within}}$	$\hat{a}$
Haiku 4.5	agent	0.48 [0.42,0.55]	0.34 [0.29,0.41]	0.29
Haiku 4.5	queries	0.27 [0.23,0.31]	0.18 [0.15,0.21]	0.32
Sonnet 4.6	agent	0.43 [0.38,0.50]	0.34 [0.28,0.41]	0.22
Sonnet 4.6	queries	0.43 [0.38,0.47]	0.33 [0.29,0.38]	0.24

Reviewer	domain	$\hat{p}_{\text{between}}$	$\hat{p}_{\text{within}}$	$\hat{a}$
Haiku 4.5	agent	0.91 [0.80,1.03]	0.66 [0.52,0.82]	0.28
Haiku 4.5	queries	0.58 [0.53,0.63]	0.40 [0.36,0.45]	0.31
Sonnet 4.6	agent	0.89 [0.79,1.00]	0.62 [0.51,0.76]	0.30
Sonnet 4.6	queries	0.56 [0.51,0.61]	0.40 [0.35,0.45]	0.28

Table 4: The operating point at $B = 4$, agent tasks. *Top*: realised false-positive rate on the 88 held-out benign tasks, with exact Clopper–Pearson 95% intervals; 88 tasks resolve a rate only to about a percentage point, so at the 1% budget no difference between these rates is measurable. *Bottom*: headline differences *as deployed* (thresholds calibrated on the 495 benign validation and training tasks, the numbers reported throughout) and *matched* (both policies calibrated on the 88 benign test tasks to an identical realised rate; symmetric between the policies but calibrated on a fifth as many tasks, so only the point estimates are informative).

Reviewer	Policy	5% budget	2% budget	1% budget
Sonnet 4.6	per-step, targeted	0.036 [0.007, 0.096]	0.000 [0.000, 0.041]	0.000 [0.000, 0.041]
	per-step, random	0.033 [0.007, 0.096]	0.010 [0.000, 0.062]	0.000 [0.000, 0.041]
	window, random block	0.063 [0.025, 0.143]	0.017 [0.000, 0.062]	0.004 [0.000, 0.041]
	joint read, scattered	0.061 [0.019, 0.128]	0.028 [0.003, 0.080]	0.013 [0.000, 0.062]
Haiku 4.5	per-step, targeted	0.051 [0.013, 0.112]	0.011 [0.000, 0.062]	0.011 [0.000, 0.062]
	per-step, random	0.051 [0.019, 0.128]	0.018 [0.003, 0.080]	0.009 [0.000, 0.062]
	window, random block	0.055 [0.019, 0.128]	0.020 [0.003, 0.080]	0.012 [0.000, 0.062]
	joint read, scattered	0.043 [0.013, 0.112]	0.021 [0.003, 0.080]	0.013 [0.000, 0.062]

Reviewer	Budget	window − targeted		window − random	
		as deployed	matched	as deployed	matched
Sonnet 4.6	5%	$+0.13$	-0.08	$+0.14^*$	-0.02
	2%	$+0.19^*$	$+0.10$	$+0.21^*$	$+0.18$
	1%	$+0.32^*$	$+0.07$	$+0.32^*$	$+0.17$
Haiku 4.5	5%	$+0.17^*$	$+0.11$	$+0.20^*$	$+0.15$
	2%	$+0.16^*$	$+0.06$	$+0.15^*$	$+0.14$
	1%	$+0.20^*$	$+0.21$	$+0.23^*$	$+0.15^*$

Table 5: Joint reads of scattered steps on agent tasks: all 18 (reviewer, budget, $B \geq 2$) settings behind Finding 2. Paired differences in detection with 95% task-bootstrap intervals (*: interval excludes zero). *Left*: one joint verdict on a uniformly random B-subset minus random per-step review of the same steps (the maximum of B separate verdicts). *Right*: the same joint read minus the contiguous window of width B.

		joint read − per-step review of the same steps			joint read − contiguous window		
Reviewer	Budget	$B = 2$	$B = 3$	$B = 4$	$B = 2$	$B = 3$	$B = 4$
Haiku 4.5	5%	$+0.12^*$ [+0.04, +0.21]	$+0.23^*$ [+0.12, +0.32]	$+0.27^*$ [+0.14, +0.37]	+0.03 [−0.04, +0.11]	$+0.10^*$ [+0.02, +0.18]	+0.07 [−0.01, +0.18]
	2%	+0.06 [−0.02, +0.20]	$+0.25^*$ [+0.14, +0.35]	$+0.22^*$ [+0.11, +0.35]	+0.02 [−0.07, +0.14]	$+0.14^*$ [+0.07, +0.22]	+0.07 [−0.01, +0.15]
	1%	$+0.11^*$ [+0.03, +0.20]	$+0.30^*$ [+0.18, +0.39]	$+0.28^*$ [+0.16, +0.38]	+0.04 [−0.03, +0.12]	$+0.10^*$ [+0.01, +0.20]	+0.05 [−0.01, +0.12]
Sonnet 4.6	5%	$+0.08^*$ [+0.01, +0.16]	$+0.20^*$ [+0.11, +0.29]	$+0.24^*$ [+0.15, +0.37]	+0.04 [−0.04, +0.12]	$+0.10^*$ [+0.03, +0.16]	$+0.12^*$ [+0.05, +0.19]
	2%	+0.09 [−0.02, +0.22]	$+0.22^*$ [+0.11, +0.34]	$+0.33^*$ [+0.23, +0.48]	−0.03 [−0.13, +0.10]	+0.01 [−0.10, +0.12]	$+0.11^*$ [+0.02, +0.21]
	1%	+0.06 [−0.04, +0.17]	$+0.23^*$ [+0.12, +0.35]	$+0.40^*$ [+0.24, +0.53]	−0.08 [−0.19, +0.03]	+0.01 [−0.07, +0.10]	+0.08 [−0.02, +0.19]

Table 6: Splitting the budget at $B = 4$ (left: 5% FPR; right: 1% FPR), on the common task set, and reference policies at 5% FPR. By construction one block of four is the window policy and four steps is random per-step review.

Reviewer / domain	one block of 4	two blocks of 2	four steps	j=1 vs 2	j=2 vs 4	n
Haiku 4.5 / agent	0.73	0.67	0.53	+0.07 [-0.03,+0.16]	$+0.13^*$ [+0.04,+0.22]	87
Haiku 4.5 / queries	0.91	0.94	0.81	-0.04^* [-0.06,-0.01]	$+0.15^*$ [+0.11,+0.18]	166
Sonnet 4.6 / agent	0.74	0.70	0.60	+0.05 [-0.01,+0.11]	$+0.10^*$ [+0.02,+0.22]	86
Sonnet 4.6 / queries	0.81	0.86	0.83	-0.05^* [-0.08,-0.02]	$+0.04^*$ [+0.01,+0.06]	166

Reviewer / domain	one block of 4	two blocks of 2	four steps	j=1 vs 2	j=2 vs 4	n
Haiku 4.5 / agent	0.39	0.27	0.16	+0.12 [-0.02,+0.22]	$+0.10^*$ [+0.01,+0.23]	87
Haiku 4.5 / queries	0.80	0.79	0.70	-0.01 [-0.05,+0.05]	$+0.10^*$ [+0.06,+0.14]	166
Sonnet 4.6 / agent	0.51	0.42	0.19	+0.09 [-0.03,+0.29]	$+0.23^*$ [+0.08,+0.37]	86
Sonnet 4.6 / queries	0.77	0.79	0.78	-0.03 [-0.08,+0.01]	+0.01 [-0.02,+0.07]	166

Reviewer / domain	full (m)	cumulative full	monolithic	monitor mean	monitor max
Haiku 4.5 / agent	0.79	0.73	1.00	0.69	0.60
Haiku 4.5 / queries	0.99	0.98	1.00	0.95	0.97
Sonnet 4.6 / agent	0.90	0.88	0.99	0.69	0.60
Sonnet 4.6 / queries	0.97	0.98	1.00	0.95	0.97

Table 7: Window minus per-step detection at $B = 4$ by task length m, agent tasks (exploratory; added after the main analysis). Thresholds are the deployed ones; intervals resample benign calibration tasks (recalibrating both policies) and harmful tasks within the stratum. Strata hold 12, 47 and 28 tasks (29 for Haiku). The last column is the slope of the per-task difference on m; no slope interval excludes zero.

Reviewer	Budget	vs	$m = 3$–5	$m = 6$–7	$m = 8$–10	slope per step
Sonnet 4.6	5%	targeted	$+0.01$ $[-0.19, +0.24]$	$+0.12$ $[-0.03, +0.28]$	$+0.21$ $[-0.03, +0.37]$	$+0.048$
		random	$+0.00$ $[-0.14, +0.18]$	$+0.13^{*}$ $[+0.00, +0.29]$	$+0.21^{*}$ $[+0.05, +0.38]$	$+0.042$
	2%	targeted	$+0.08$ $[-0.12, +0.34]$	$+0.24^{*}$ $[+0.00, +0.43]$	$+0.16^{*}$ $[+0.02, +0.41]$	$+0.005$
		random	$+0.05$ $[-0.16, +0.25]$	$+0.22^{*}$ $[+0.10, +0.42]$	$+0.28^{*}$ $[+0.14, +0.42]$	$+0.043$
	1%	targeted	$+0.26$ $[-0.02, +0.50]$	$+0.34^{*}$ $[+0.12, +0.47]$	$+0.30^{*}$ $[+0.06, +0.49]$	-0.009
		random	$+0.20$ $[-0.06, +0.46]$	$+0.35^{*}$ $[+0.13, +0.49]$	$+0.30^{*}$ $[+0.12, +0.48]$	$+0.007$
Haiku 4.5	5%	targeted	$+0.16$ $[-0.14, +0.47]$	$+0.21^{*}$ $[+0.02, +0.37]$	$+0.10$ $[-0.05, +0.23]$	-0.022
		random	$+0.13$ $[-0.15, +0.44]$	$+0.22^{*}$ $[+0.06, +0.32]$	$+0.19^{*}$ $[+0.06, +0.29]$	$+0.008$
	2%	targeted	$+0.18$ $[-0.04, +0.37]$	$+0.24^{*}$ $[+0.06, +0.37]$	$+0.02$ $[-0.15, +0.18]$	-0.056
		random	$+0.11$ $[-0.09, +0.30]$	$+0.20^{*}$ $[+0.07, +0.34]$	$+0.10^{*}$ $[+0.01, +0.24]$	-0.016
	1%	targeted	$+0.11$ $[-0.05, +0.30]$	$+0.28^{*}$ $[+0.14, +0.41]$	$+0.12$ $[-0.08, +0.34]$	-0.017
		random	$+0.12$ $[-0.04, +0.29]$	$+0.28^{*}$ $[+0.14, +0.39]$	$+0.20^{*}$ $[+0.03, +0.35]$	$+0.006$

Table 8: Scarcer attention: detection on agent tasks re-decomposed by two further clause-boundary splits (harmful and benign alike; mean 12.3 steps), strong reviewer, thresholds calibrated on the split benign validation and training tasks. The audit rate is the mean fraction of a task read. Paired differences as in Table 1 (*: interval excludes zero; $\equiv$: identical policies).

Budget		$B = 1$	$B = 2$	$B = 3$	$B = 4$
	audit rate	0.09	0.18	0.26	0.35
5%	per-step, targeted	0.39	0.42	0.48	0.48
	per-step, random	0.28	0.36	0.41	0.46
	window, random block	0.28	0.42	0.53	0.59
	joint read, scattered	0.28	0.48	–	0.66
	window − targeted	–	$+0.01$ $[-0.11, +0.12]$	$+0.04$ $[-0.07, +0.21]$	$+0.11$ $[-0.01, +0.27]$
	window − random	$\equiv$	$+0.06^{*}$ $[+0.02, +0.10]$	$+0.12^{*}$ $[+0.06, +0.17]$	$+0.13^{*}$ $[+0.08, +0.23]$
2%	per-step, targeted	0.22	0.22	0.23	0.23
	per-step, random	0.18	0.21	0.23	0.24
	window, random block	0.18	0.33	0.38	0.45
	joint read, scattered	0.18	0.38	–	0.51
	window − targeted	–	$+0.10$ $[-0.02, +0.19]$	$+0.15^{*}$ $[+0.00, +0.24]$	$+0.22^{*}$ $[+0.08, +0.31]$
	window − random	$\equiv$	$+0.11^{*}$ $[+0.06, +0.17]$	$+0.15^{*}$ $[+0.08, +0.23]$	$+0.21^{*}$ $[+0.12, +0.29]$
1%	per-step, targeted	0.14	0.15	0.15	0.15
	per-step, random	0.12	0.13	0.13	0.13
	window, random block	0.12	0.22	0.30	0.35
	joint read, scattered	0.12	0.27	–	0.41
	window − targeted	–	$+0.07$ $[-0.02, +0.15]$	$+0.14^{*}$ $[+0.06, +0.22]$	$+0.20^{*}$ $[+0.11, +0.28]$
	window − random	$\equiv$	$+0.09^{*}$ $[+0.03, +0.16]$	$+0.16^{*}$ $[+0.08, +0.23]$	$+0.22^{*}$ $[+0.13, +0.30]$

Table 9: Detection before and after the agent adapts (rewrite condition), agent tasks, matched thresholds. *Compliers*: the tasks the rewriter returned, paired. *All tasks*: intent-to-treat over every base task, a refused task keeping its original decomposition.

Reviewer	Budget	targeted B=4	random B=4	window B=4	top window B=4	full (m)	monitor mean
Sonnet 4.6	5%, compliers (n=60)	$0.51 \to 0.17$	$0.48 \to 0.18$	$0.68 \to 0.38$	$0.81 \to 0.39$	$0.85 \to 0.64$	$0.57 \to 0.12$
	5%, all tasks (n=88)	$0.59 \to 0.36$	$0.59 \to 0.38$	$0.73 \to 0.53$	$0.86 \to 0.58$	$0.90 \to 0.75$	$0.69 \to 0.38$
	2%, compliers (n=60)	$0.28 \to 0.00$	$0.26 \to 0.03$	$0.48 \to 0.19$	$0.61 \to 0.21$	$0.75 \to 0.47$	$0.35 \to 0.03$
	2%, all tasks (n=88)	$0.38 \to 0.19$	$0.36 \to 0.20$	$0.58 \to 0.39$	$0.71 \to 0.44$	$0.82 \to 0.63$	$0.47 \to 0.25$
	1%, compliers (n=60)	$0.09 \to 0.00$	$0.10 \to 0.00$	$0.39 \to 0.12$	$0.46 \to 0.14$	$0.62 \to 0.38$	$0.18 \to 0.00$
	1%, all tasks (n=88)	$0.19 \to 0.13$	$0.19 \to 0.12$	$0.50 \to 0.32$	$0.61 \to 0.38$	$0.73 \to 0.56$	$0.32 \to 0.19$
Haiku 4.5	5%, compliers (n=60)	$0.46 \to 0.21$	$0.44 \to 0.15$	$0.68 \to 0.30$	$0.67 \to 0.27$	$0.70 \to 0.43$	$0.57 \to 0.12$
	5%, all tasks (n=88)	$0.57 \to 0.39$	$0.54 \to 0.34$	$0.73 \to 0.47$	$0.76 \to 0.49$	$0.79 \to 0.61$	$0.69 \to 0.38$
	2%, compliers (n=60)	$0.13 \to 0.03$	$0.17 \to 0.04$	$0.31 \to 0.08$	$0.50 \to 0.09$	$0.58 \to 0.29$	$0.35 \to 0.03$
	2%, all tasks (n=88)	$0.28 \to 0.22$	$0.29 \to 0.20$	$0.44 \to 0.28$	$0.62 \to 0.35$	$0.70 \to 0.50$	$0.47 \to 0.25$
	1%, compliers (n=60)	$0.07 \to 0.03$	$0.05 \to 0.01$	$0.26 \to 0.04$	$0.47 \to 0.09$	$0.51 \to 0.23$	$0.18 \to 0.00$
	1%, all tasks (n=88)	$0.19 \to 0.17$	$0.16 \to 0.14$	$0.40 \to 0.25$	$0.61 \to 0.34$	$0.65 \to 0.46$	$0.32 \to 0.19$

Table 10: The split condition, agent tasks, both reviewers, matched thresholds. Every task has a split variant, so the complier and intent-to-treat comparisons coincide and one row per budget is shown ($n = 88$; 87 for the strong reviewer's window and full-review columns, where one task lacks a score).

Reviewer	Budget	targeted $B{=}4$	random $B{=}4$	window $B{=}4$	top window $B{=}4$	full (m)	monitor mean
Sonnet 4.6	5%	$0.59 \to 0.55$	$0.59 \to 0.44$	$0.73 \to 0.62$	$0.86 \to 0.80$	$0.90 \to 0.87$	$0.69 \to 0.46$
	2%	$0.38 \to 0.30$	$0.36 \to 0.22$	$0.58 \to 0.49$	$0.71 \to 0.59$	$0.82 \to 0.80$	$0.47 \to 0.20$
	1%	$0.19 \to 0.13$	$0.19 \to 0.10$	$0.50 \to 0.40$	$0.61 \to 0.51$	$0.73 \to 0.69$	$0.32 \to 0.10$
Haiku 4.5	5%	$0.57 \to 0.55$	$0.54 \to 0.40$	$0.73 \to 0.62$	$0.76 \to 0.71$	$0.79 \to 0.78$	$0.69 \to 0.46$
	2%	$0.28 \to 0.21$	$0.29 \to 0.17$	$0.44 \to 0.36$	$0.62 \to 0.57$	$0.70 \to 0.68$	$0.47 \to 0.20$
	1%	$0.19 \to 0.13$	$0.16 \to 0.08$	$0.40 \to 0.30$	$0.61 \to 0.54$	$0.65 \to 0.64$	$0.32 \to 0.10$

Table 11: The rewrite condition at the 2% budget, split by which model produced the rewrite.

Reviewer	Rewriter	targeted $B{=}4$	random $B{=}4$	window $B{=}4$
Sonnet 4.6	Sonnet 4.6	$0.23 \to 0.00$ $n{=}21$	$0.15 \to 0.05$ $n{=}21$	$0.22 \to 0.17$ $n{=}21$
	Haiku 4.5	$0.30 \to 0.00$ $n{=}39$	$0.32 \to 0.01$ $n{=}39$	$0.62 \to 0.21$ $n{=}38$
Haiku 4.5	Sonnet 4.6	$0.09 \to 0.00$ $n{=}21$	$0.09 \to 0.04$ $n{=}21$	$0.15 \to 0.05$ $n{=}21$
	Haiku 4.5	$0.15 \to 0.05$ $n{=}39$	$0.21 \to 0.04$ $n{=}39$	$0.40 \to 0.09$ $n{=}39$

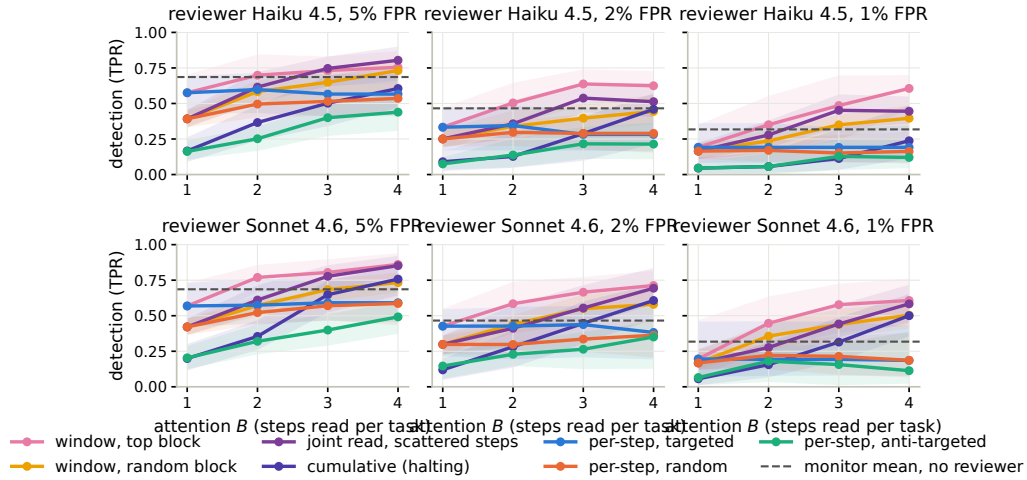

Figure 3: Detection against the review budget B on agent tasks, both reviewers, at three false-positive budgets; shading is a 95% task bootstrap. Windows overtake per-step review by $B = 3$ and the gap widens as the budget tightens; at 1% neither per-step policy improves with B.