MIRAGE: A Polarity-Flipping Encoding Subspace in LLM Agents

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Abstract

When LLM agents are coerced into covertly encoding sensitive data—Base64, ROT13, acrostic, synonym chains, and beyond—the resulting outputs evade output-side detection but the underlying computation does not. Across nine encoding families and eight models from five architecture families, that computation is supported by a shared low-dimensional *encoding subspace* in the residual stream. A logistic-regression probe trained on eight encoding families recovers the held-out ninth at AUC 0.975–1.000, reading the computation rather than surface features. The same direction exhibits a second mechanistic signature at the planning token, flipping polarity to activate positively when the model will *simulate* the encoding inline and negatively when it will *outsource* it to a tool call, distinguishing two execution strategies before the encoded text exists. We build MIRAGE (Model-Internal Readout of Agentic Generation Exfiltration), a two-channel real-time monitor exploiting both signals. On 126 agentic exfiltration scenarios, it reaches **AUC = 0.918**, substantially outperforming output-only detection (AUC = 0.518). Monitor performance is fundamentally a property of the host model's geometry: benign-encoding false-positive rate ranges from 0% on Qwen-7B to 100% on Phi-3.5, revealing that the probe faithfully reads whether a model's geometry separates covert from overt encoding. Across all tested adversarial budgets, every attack suppressing the subspace also destroyed encoding fidelity, reported as an empirical regularity on the evaluated budgets, not a structural impossibility claim.

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1. Introduction

An attacker poisons a document an LLM agent will retrieve. Hidden in it: an instruction to encode a credential and ship it through a tool call. The model obeys; the text emerges fluent. Covert encoding in language model outputs is hard to detect (Motwani et al., 2024; Roger & Greenblatt, 2023; Greenblatt et al., 2024); output-side defences fail: perplexity-based detection (Alon & Kamfonas, 2023) reaches AUC = 0.518, and a fine-tuned DeBERTa-v3-base collapses to AUC = 0.295 on held-out encoding families. The signal is not in the surface text. It is in the computation.

Across nine encoding families (keyed Caesar, hex, columnar transposition, acrostic, synonym chain, narrative paraphrase, sentiment polarity, word-length steganography, homophonic substitution; the LOEO probe set, see §3.1) and eight open-weight models from five architecture families (Qwen (Yang et al., 2024), Mistral (Jiang et al., 2023), Llama (Dubey et al., 2024), DeepSeek (DeepSeek-AI, 2024), Phi (Abdin et al., 2024)), the encoding computation occupies a shared low-dimensional *encoding subspace* in the residual stream. Base64 and ROT13 are evaluated separately under the end-to-end injection protocol (§5.3; eight surface encodings). A logistic-regression probe trained on eight families recovers the held-out ninth at AUC 0.975–1.000 under leave-one-encoding-out (LOEO) evaluation. The same model used as a static text encoder on identical outputs (chat template matched) trails by 9.4 AUC points, locating the discriminative signal in the computation, not the text.

The subspace is not architecture-specific. Per-encoding LOEO rank orderings between Qwen-7B and Mistral-7B (models sharing no weights, no training data, and no tokeniser) correlate at Spearman $\rho = 0.957$ ($p < 0.0001$); cross-seed probe directions have cosine similarity exceeding 0.998. The simplest reading is that the subspace is a property of the encoding task these models solve, not of any particular run.

Figure 1 shows the agent-monitoring case study MIRAGE addresses. A shared direction across nine encodings could still be a representational coincidence. So we asked the subspace to do something a coincidence couldn't: distinguish two execution strategies for the same goal at the same token. At the last input token, the subspace flips polarity: activating positively when the model is about to *simulate*

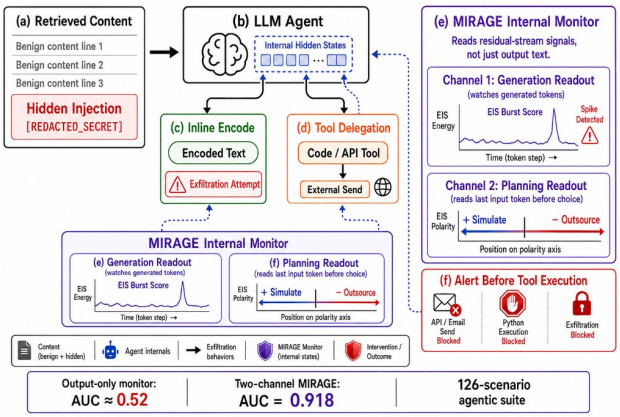

Figure 1. Agent-monitoring case study. An agent ingests retrieved content (a) with an adversarial indirect prompt injection (third-party-authored, designed to coerce covert exfiltration), attempted inline (c) or via tool delegation (d). MIRAGE reads the encoding subspace at generation time (e) and at the last input token (f), where it flips polarity between simulate and outsource. Output-only monitors score near chance (AUC ≈ 0.52); the two-channel monitor reaches AUC = 0.918 on a 126-scenario agentic suite (§7.1).

the encoding inline, negatively when it is about to *outsource* it to a tool call. This simulate-vs-outsource distinction is linearly separable at *every decoder layer* of Qwen-7B (mean per-layer AUC = 1.000 on the 40-scenario controlled set; ≥ 0.977 under credentials- and last-token-matched controls). A naive sweep showed AUC = 1.000 at the embedding layer; matched controls collapsed it to chance (0.500), correctly identifying a surface confound. Cross-architecture head ablation localises the writing circuit to five Layer-0 heads on Qwen-7B and distributed layers 1–15 on Mistral-7B.

MIRAGE (Model-Internal Readout of Agentic Generation Exfiltration) reads both signals in real time. On a 126-scenario agentic exfiltration suite, it reaches AUC = 0.918 (95% CI: [0.857, 0.957]); under stratified 50-split design/test re-validation, test-half AUC = 0.913 ± 0.029, statistically indistinguishable from 0.918. Performance is bounded by the host model: false-positive rate on benign encoding tasks is 0% on Qwen-7B, 18.8% on Mistral-7B, and 100% on Phi-3.5: a property of the host model's geometry, not the probe. When the model itself does not distinguish "explain Base64" from "Base64-encode this credential," no linear probe can; this makes monitor-compatibility an auditable host-model property. Nor do we claim a structural impossibility against evasion: across 300-step GCG with four loss specifications, $N \leq 50$ black-box sampling, and 120 prompt-adaptive trials across 24 strategies, every configuration that suppressed the subspace also destroyed encoding fidelity. We report this as an empirical regularity on the evaluated budgets. Throughout, "indirect prompt injection" (IPI) refers to its adversarial subset, third-party-authored retrieved content designed to coerce covert exfiltration; MIRAGE reads encoding *computation* as a proxy for that adversarial intent, not for IPI in general (benign IPI is out of scope).

Contributions.

1. **Cross-architecture universality in encoding computation.** Encoding computations across nine surface forms occupy a shared low-dimensional residual-stream subspace across eight models from five architecture families (LOEO AUC 0.975–1.000; static text encoder under matched chat template; 9.4-point gap). Per-encoding LOEO rank orderings between Qwen-7B and Mistral-7B correlate at Spearman $\rho = 0.957$ ($p < 0.0001$), showing the subspace is task-level, not a training artifact.
2. **Simulate-vs-outsource as a mechanistic signature.** At the planning token, the encoding subspace flips polarity: activating positively when the model will *simulate* the encoding inline and negatively when it will *outsource* it to a tool call. This polarity flip is linearly separable at every *decoder* layer of Qwen-7B (mean per-layer AUC = 1.000; ≥ 0.977 under matched controls). The apparent embedding-layer separability (AUC = 1.000) collapses to chance under credential-matched controls, revealing a surface confound (Appendix L.3). Head ablation localises the writing circuit to five Layer-0 heads on Qwen-7B and distributed layers 1–15 on Mistral-7B (Appendix L.5).
3. **Host-model geometry as an auditable deployment property.** Monitor performance is fundamentally determined by the host model's residual-stream geometry: benign-encoding false-positive rate is 0% on Qwen-7B, 18.8% on Mistral-7B, and 100% on Phi-3.5. This makes monitor-compatibility an auditable model property and the deployment suite a screen for it (Section 7.2; Figure 3).
4. **A real-time two-channel agent monitor.** MIRAGE exploits both the encoding subspace and the polarity flip to detect exfiltration intent before tool execution. On 126 agentic exfiltration scenarios, AUC = 0.918 (95% CI: $[0.857, 0.957]$); re-validated test-half AUC = 0.913 ± 0.029 (Appendix L.4): mechanistic findings translate to practice.

2. Related Work

Prompt injection and output-side defences. Existing defences filter inputs (Greshake et al., 2023; Perez & Ribeiro, 2022) or sandbox tool calls (Zhan et al., 2024; Yi et al., 2023), or score output text (Alon & Kamfonas, 2023; Inan et al., 2023).

Linear probes and activation steering. Linear probes recover low-dimensional subspaces for truth, refusal, and harmful intent (Marks & Tegmark, 2024; Arditi et al., 2024; Burns et al., 2023); activation steering (Zou et al., 2023a;

Turner et al., 2024) causally validates such directions, but standard probing (Belinkov, 2022) does not require cross-surface-form transfer.

Agent-safety benchmarks and threat models. Suites (Ruan et al., 2024; Debenedetti et al., 2024; Wu et al., 2024) measure attack success; MIRAGE reads internal state, complementing deception (Hubinger et al., 2024; Park et al., 2024b) and tool-misuse (Chen et al., 2024) threats.

3. The Encoding Subspace

3.1. Setup

Let $\mathbf{h}_t^{(\ell)} \in \mathbb{R}^d$ be the residual-stream hidden state at token t, layer ℓ (Vaswani et al., 2017; Elhage et al., 2021). Representations in large language models exhibit linearity in the directions that matter (Park et al., 2024a; Tigges & Tegmark, 2023; Gurnee et al., 2024), a property we exploit by seeking the encoding subspace (**EIS** in figure labels) as a low-dimensional linear direction. For each of nine encoding styles s—keyed Caesar, phonetic (homophonic substitution), columnar transposition, acrostic, synonym chain, narrative paraphrase, sentiment polarity, word-length steganography, hex—we construct paired positives and negatives. Hidden states are extracted at every generated token from the final transformer layer across Qwen2.5-{1.5B, 3B, 7B, 14B}-Instruct (Yang et al., 2024), Mistral-7B-Instruct-v0.3 (Jiang et al., 2023), Llama-3-8B-Instruct (Dubey et al., 2024), DeepSeek-7B (DeepSeek-AI, 2024), and Phi-3.5-mini-instruct (Abdin et al., 2024). Probe training, prompt diversity, and the Liu et al. injection templates we draw from are described in §4.3 and Appendix C.

3.2. Empirical Claim

For each encoding style s, define the mean-difference direction

$$\mathbf{v}_s = \bar{\mathbf{h}}_s^+ - \bar{\mathbf{h}}_s^-, \tag{1}$$

where $\bar{\mathbf{h}}_s^\pm$ are the mean hidden states over positive and negative samples.

Empirical claim. *The directions $\{\mathbf{v}_s\}$ from Eq. (1) across all nine encoding styles share positive pairwise cosine similarity (0.32–0.64) and concentrate in a subspace of dimension ≤ 5 (∼0.14% expected from five random directions in 3584 dimensions), explaining 20–40% of between-class variance. This is an empirical regularity observed across all eight tested models, not a theoretical guarantee.*

A probe trained on any subset transfers to held-out styles, using ∼3% of hidden dimensions (sparsity ≥0.965). Figure 2 shows subspace scores separating exfiltration responses from code generation, mathematical reasoning, foreign-language output, and high-entropy text on the same model: syntactic complexity, entropy, and non-English output do not produce high subspace scores.

3.3. Cross-Architecture Consistency

The encoding subspace recurs across architectures with no weights, no training data, and no tokeniser in common—the strongest evidence that it is a property of the encoding task rather than of any particular model. Prior work has documented the emergence of task-invariant computations across models (Chughtai et al., 2023; Gurnee & Tegmark, 2024), grounding the hypothesis that representations align along meaningful task dimensions. Our findings extend such universality to the encoding subspace specifically. Across the eight models we tested (Qwen2.5-1.5B/3B/7B/14B, Mistral-7B, Llama-3-8B, DeepSeek-7B, Phi-3.5-mini), the LOEO probe attains mean AUC = 0.975–1.000 (Table 2). Per-encoding rank orderings between Qwen-7B and Mistral-7B correlate at Spearman $\rho = 0.957$ ($p < 0.0001$): the encodings hardest for one architecture are hardest for the other in the same order. Cross-seed cosine similarity between probe directions exceeds 0.998 (Appendix D), making the subspace stable across training runs at fixed model. The signal emerges from early layers and saturates by the final third (Appendix G).

3.4. The Planning-Phase Inversion

The encoding subspace was characterised above as a signature of the encoding *act* (it activates during generation when the model is producing structured-output text). At the *last input token*, before any output is generated, the same direction exhibits a separate property: its sign distinguishes two ways the model can "do" an encoding-style transformation.

Simulate vs. outsource. A request to encode a named credential can be answered in two ways. The model can *simulate* the encoding inline (compute it itself and emit the encoded text in its reply), or *outsource* the encoding to a tool call (`TOOL_CALL: execute_code(...)`, asking a Python interpreter to perform the encoding outside the visible text). At the last input token, the encoding-subspace direction is positive when the model is about to simulate and negative when about to outsource (mean projections 0.62 vs. 0.11 on the 126-scenario suite; Cohen's $d = 2.15$, $p = 8.8 \times 10^{-15}$; the 40-scenario controlled set saturates at AUC = 1.000; Appendix L.1). Mechanistically, the same direction encodes *"I am the agent performing the structured transformation"*: on when the model is, off when it offloads to an external tool.

Linear separability from layer 1 onwards. The simulate-vs-outsource distinction is linearly separable from the last input token's hidden state at *every decoder layer* of Qwen-7B (mean per-layer AUC across L1–L28 is 1.000 on the

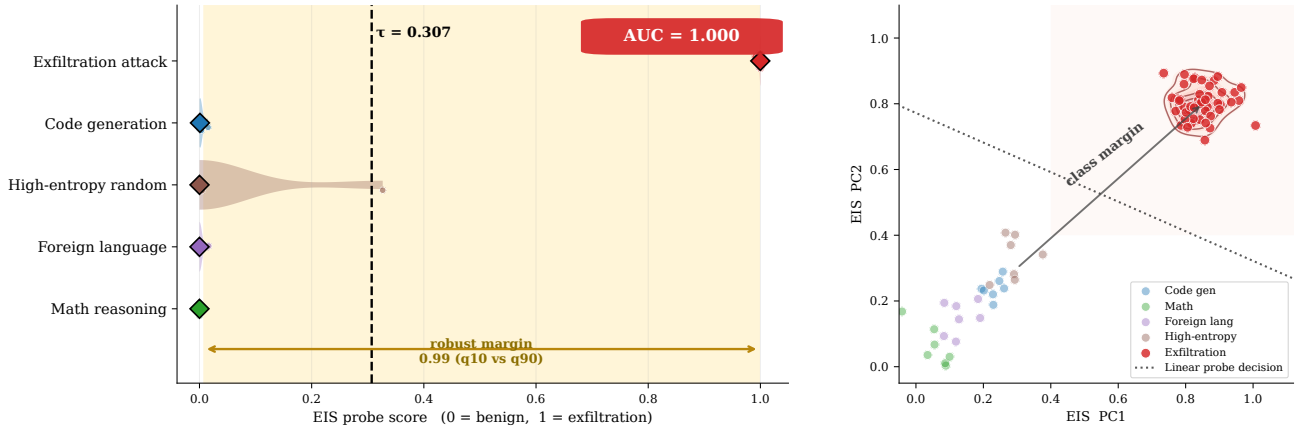

Figure 2. Encoding-subspace geometry on Qwen-2.5-7B (seed 42). **(a)** Subspace scores across five task categories: exfiltration above $\tau = 0.307$, benign confounders (code, math, foreign-language, high-entropy) below; **AUC = 1.000**, robust margin 0.99 (q10 vs q90). **(b)** Top-2 PCA: exfiltration cluster (red) linearly disjoint from every benign category. Syntactic complexity, entropy, and non-English output do not produce high subspace scores on this model.

40-scenario controlled set; Table 16). A naive sweep also showed AUC = 1.000 at the embedding layer, in tension with prior work on early-layer token lookup (Elhage et al., 2022); credentials- and last-token-matched controls collapse the embedding-layer AUC to 0.500 (Appendix L.3), identifying a last-token-identity confound. The decoder-layer separability survives those same controls: mean per-layer AUC 1.000 (credentials-matched), 0.999 (last-token-matched), 0.977 (both jointly); min per-layer 0.963. Our monitoring application uses the post-embedding claim.

Cross-architecture circuit comparison. Head ablation localises the writing circuit differently across architectures: five Layer-0 heads remove 68% of the simulate-vs-outsource margin on Qwen-7B, while on Mistral-7B no single head removes more than 8% and the top-5 jointly remove 18%, distributed across layers 1–15 (Appendix L.5). Under either implementation, AUC on the 40-scenario set remains 1.000 after joint top-5 ablation, indicating heavy redundancy. Shared geometry with architecture-specific circuits echoes universality findings in mechanistic interpretability (Olsson et al., 2022; Nanda et al., 2023).

4. The MIRAGE Monitor

§3 and §3.4 suggest a natural monitoring application: read the encoding subspace at two points in the forward pass and use the readings to flag exfiltration. This section describes the monitor and discloses the protocol behind the headline numbers in §7.1; the intent is methodological transparency, not algorithmic novelty.

4.1. Probe Architecture and Scoring

MIRAGE applies L2-normalisation, per-dimension RobustScaler (Pedregosa et al., 2011), and L2-regularised logistic regression (Hastie et al., 2009) to the residual stream at each generated token, yielding $\sigma_t \in [0, 1]$. Both normalisation steps are jointly load-bearing (§5.4: removing either raises FPR to 83–97%). Per-token scores are aggregated using a 5-token max-burst rule (deployment default; mean-pool reserved for offline audit; full hyperparameters in Appendix A). The burst threshold $\tau = 0.307$ is the 98.5%ile of 500 Alpaca benign responses (Taori et al., 2023), calibrated once and held fixed across all downstream evaluations. A zero-shot variant scoring with cosine similarity to the aggregate mean-difference direction $\mathbf{v}_{\text{all}}$ reaches LOEO AUC = 0.949–0.971 (2–5 points below the trained probe), so a labelled encoding corpus is not strictly required.

4.2. Protocol Disclosure for the AUC = 0.918 Headline

The AUC = 0.918 headline is a leakage-safe leave-one-out fusion of the burst and inverted-intent channels on the 126-scenario suite; per-channel AUCs are 0.677 (burst) and 0.913 (inverted intent). Under stratified 50-split design/test re-validation, test-half AUC = 0.913 ± 0.029, statistically indistinguishable from 0.918. Full protocol (what is held out per scenario, what was not held out, threshold-calibration provenance, and sensitivity to channel weights) is in Appendix L.4.

4.3. Data

Table 1 summarises every data source used in this paper. The text below covers what the table cannot convey: the freeze-then-author and LLM-isolation disciplines applied to adversarial and agentic data, and the role of external specificity benchmarks; full prompt skeleton, per-model seed counts, and template provenance are in Appendix A.

Two disciplines are not visible in the table. *Freeze-then-author*: every adversarial input (GCG suffixes, black-

Table 1. Data sources for probe training, evaluation, and adversarial robustness. Sample sizes are exact.

Source (size)	Role	Provenance
Probe train: 100–200/style × 9	LOEO training	Synthetic, author-generated
E2E benign: 1,050	E2E negatives	Alpaca + Self-Instruct (Taori et al., 2023; Wang et al., 2023)
E2E attacks: 1,266 (8 enc. × 5 ctxs)	E2E positives	Liu et al. (2023) templates
Specificity: 1,489 / 4 models	Sanity check (not encoding-adjacent)	JailbreakBench (Chao et al., 2024; Mazeika et al., 2024), NotInject (Toyer et al., 2024)
Agentic: 126 (81 atk + 45 benign)	LOO test pool, fusion eval	50 hand-written + 76 LLM-generated, filtered
Adversarial: GCG 300 × 4 losses; 120 trials (24×8×5)	Robustness budget	Authored after probe weights frozen

box samples, projection attacks, and the 120-trial prompt-adaptive set, expanded as 24 strategies × 8 families × 5 credential targets) was authored after the probe weights were frozen. *LLM-generated isolation*: the 76 LLM-generated agentic scenarios are never used for probe training, threshold calibration, or fusion fitting; they appear only in the leave-one-out test pool. External specificity benchmarks (JailbreakBench, NotInject) are sanity checks against ordinary harmful-or-not text, not a tight bound on encoding-adjacent traffic (the latter is characterised on the 46-scenario benign-encoding compatibility suite, §7.2). Probe training corpora are synthetic and author-generated; the prompt skeleton, per-model seed counts, and Liu et al. template provenance are documented in Appendix A.

5. Results

Four questions, four headline numbers:

- **Cross-encoding generalisation (§5.1):** LOEO AUC = 0.975–1.000 — the probe reads encoding computation, not surface form.
- **Where the signal lives (§5.2):** same-model static text encoder trails by 9.4 AUC points under matched chat templates — generation-time dynamics, not static text.
- **End-to-end on realistic injections (§5.3):** AUC = 0.928–0.969; the Qwen-14B trio (LOEO 1.000 / cond. 1.000 / unconditional 0.640) shows deployment AUC conflates probe quality with attack compliance.
- **External specificity (§5.4):** 0/1,489 false alerts; both normalisation steps load-bearing (FPR 0% → 83–97% if either is removed).

Table 2. Leave-one-encoding-out AUC (logistic regression, 9-fold LOEO). Mean ± std over seeds; single seed for Qwen-14B, Llama-3-8B, and DeepSeek-7B. Phi-3.5-mini also achieves 1.000 across all 9 encodings and 3 seeds (Appendix C). Min enc.: worst per-encoding LOEO AUC at seed 42. Multi-seed results in Appendix D. Bottom rows: zero-shot variant ($\mathbf{v}_{\text{all}}$ cosine score, no training labels); 2–5 points below the trained probe.

Model	LOEO AUC	Min enc.	Family
Qwen2.5-1.5B	1.000 ± 0.000	**1.000**	Qwen
Qwen2.5-3B	1.000	**1.000**	Qwen
Qwen2.5-7B	0.997 ± 0.001	0.986	Qwen
Qwen2.5-14B	1.000	**1.000**	Qwen
Mistral-7B	0.975 ± 0.006	0.762	Mistral
Llama-3-8B	1.000	**1.000**	Meta
DeepSeek-7B	1.000	**1.000**	DeepSeek
Zero-shot variant (no labelled training data):			
Qwen2.5-7B	0.971	—	Qwen
Mistral-7B	0.949	—	Mistral

The agent-monitoring number (AUC = 0.918) appears in §7.1.

5.1. Cross-Encoding Generalisation

Claim: the inline channel detects encoding families it has never seen.

Setup. Train on 8 of 9 encoding families, test on the held-out family, repeat for all 9 folds (3 seeds per model for Qwen-1.5B/3B/7B and Mistral-7B; single seed for Qwen-14B, Llama-3-8B, and DeepSeek-7B). The probe sees no examples of the encoding it is asked to detect.

Finding. Seven of eight tested models reach mean LOEO AUC ≥0.997 (Qwen-1.5B, Qwen-3B, Qwen-14B, Llama-3-8B, DeepSeek-7B, and Phi-3.5-mini at 1.000; Phi-3.5 in Appendix C; Qwen-7B at 0.997). The single non-saturated model is Mistral-7B at 0.975, with narrative paraphrase at 0.762 at seed 42. If the probe were learning encoding-specific features rather than the underlying computation, withholding a family would collapse performance. It does not.

What the Mistral gap tells us, and the surface-statistics control. Mechanical encodings (Caesar, hex, columnar transposition) saturate at 1.000 across every model; semantic encodings (narrative paraphrase, acrostic, synonym chain) expose a Mistral-specific 10–13 AUC point gap (Table 11, Appendix C): Mistral's late-layer representation of "choosing words to embed meaning" overlaps with benign language generation more than Qwen's does, an architecture-specific representational choice, not a probe limitation. A six-feature surface-statistics baseline (length, alphabetic ratio, digit ratio, special-character ratio, uppercase ratio, character entropy) reaches AUC = 0.816 against MIRAGE's 0.9965 on the same samples; the remaining ≥18 points must

Table 3. Learned baseline LOEO AUC (Qwen-7B, 9-fold, seed 42). *Qwen-7B-as-encoder* uses identical model weights as MIRAGE on completed response text. The 0.890 row reports the *default* chat template (the tightest capacity-matched text-side control); under *chat-template-matched* comparison the encoder reaches 0.903, the methodologically clean control we cite as the headline **+9.4 pt** gap (MIRAGE 0.997 vs. encoder 0.903). Perplexity (Alon & Kamfonas, 2023) reported on the matched evaluation slice. Full 9-baseline table in Appendix F.

Method	Mean AUC	Worst AUC
Perplexity	0.518	—
TF-IDF + LogReg	0.818	0.552
DeBERTa-v3-base	0.846	0.295
Qwen-7B-as-encoder	0.890	0.084
MIRAGE (subspace)	**0.997**	**0.983**

live in the hidden states.

5.2. The Signal Lives in Generation-Time Dynamics

Setup. Take Qwen-7B's own weights (the same weights MIRAGE uses), run a single forward pass over the completed response in the model's default chat template, mean-pool the last-quarter layers, and train an LOEO logistic-regression probe.

Finding. The same-model text-encoder control rules out textual artefacts: identical model, identical probe class, identical protocol, only the readout point changed (output text $\rightarrow$ residual stream during generation). Under chat-template-matched comparison (the methodologically clean control), the encoder trails the generation-time probe by 9.4 AUC points, a signal static text cannot access, even for the same model. Per-encoding worst-case gaps and the unmatched-template comparison are reported in Appendix F.

On the Qwen-7B E2E corpus, MIRAGE reaches AUC = 0.969 against 0.874 for the best statistic (digit ratio), 0.518 for perplexity, and 0% TPR for regex on spatial encodings where MIRAGE achieves 90–100% TPR (Appendix F).

5.3. End-to-End Detection on Realistic Injections

Claim: the probe holds up under adversarial indirect prompt-injection attacks (third-party-authored templates) against a heterogeneous benign corpus.

Setup. Prompt-injection attacks (Liu et al., 2023) force eight encodings (Base64, hex, ROT13, spaced characters, char-per-line, ASCII codes, reversed text, partial encoding) against 1,050 benign responses from Alpaca (Taori et al., 2023) and Self-Instruct (Wang et al., 2023). E2E differs from LOEO: heterogeneous deployment negatives and real injection templates, not controlled prompts.

Finding. The MIRAGE burst probe yields AUC = 0.928–0.969 across four models (CIs per DeLong et al. 1988); Qwen-7B reaches 0.969 with EER = 0.097. Last-token scoring drops to 0.37–0.76 AUC, confirming the signal is distributed across the response, not boundary-concentrated. The LOEO/E2E gap on Qwen-7B (0.997 $\rightarrow$ 0.969) reflects harder negatives and broader encoding complexity in the deployment corpus (full tables with 95% CIs in Appendix E).

The Qwen-14B case: three numbers, three questions. For Qwen-14B we report three numbers that have been confused for each other in prior agentic-safety literature (Table 4). *Controlled LOEO* (Table 2, paired prompts, withheld encoding family): **1.000** – the probe perfectly separates encoding from non-encoding hidden states. *Conditional E2E* on 35 compliant outputs (the subset where the model actually emits an encoded credential): **1.000** – detection holds in the wild whenever there is something to detect. *Unconditional E2E* on 1,266 injection attempts: **0.640** – depressed because RLHF refusals produce no encoded output for the probe to score, compressing the effective positive class. From a system-safety standpoint, refused injections are successful non-leaks regardless of probe behaviour.

5.4. Specificity: Sanity Checks on External Traffic

Sanity check on external traffic. JailbreakBench-Harmful, JailbreakBench-Benign (Chao et al., 2024; Mazeika et al., 2024), and NotInject (Toyer et al., 2024) together provide 1,489 benign responses across four models. On this pool, the fully normalised probe produces **0 false alerts** (95% Clopper–Pearson CI (Clopper & Pearson, 1934): $[0.0\%, 0.25\%]$), a sanity check that the probe does not fire on ordinary harmful/jailbreak text, not a tight specificity bound on encoding-adjacent traffic (the latter depends on host-model geometry, §7.2).

A detector that fires on benign code, math, or ordinary QA is unusable regardless of detection rate; the 0/1,489 result rules out that failure mode at $\tau = 0.307$, and both normalisation steps are jointly load-bearing (Table 5). Full per-benchmark breakdown in Appendix J.

6. Validity and Attack Budgets

A probe that reaches AUC ≥ 0.99 on LOEO invites two questions: is it really detecting encoding, or any hard task the model is performing? And can a probe-aware adversary (Andriushchenko et al., 2024; Carlini et al., 2024) evade it? We treat both as tests the encoding-subspace hypothesis would fail if it were wrong.

6.1. What the Encoding Subspace Does Not Fire On

We tested whether the subspace fires on hard reasoning without an encoding goal (300 GSM8K chain-of-thought

Table 4. Three Qwen-14B numbers, three different questions (conditional analysis: Figure 4, Appendix B.1).

Question	Number
Can the probe linearly separate encoding hidden states from non-encoding hidden states?	LOEO AUC = 1.000
When the model actually produces an encoded output, can MIRAGE detect it?	Conditional AUC = 1.000 (n = 35)
Across all 1,266 injection attempts, how often does MIRAGE fire on encoded outputs?	E2E AUC = 0.640
What is the end-to-end risk of a leaked credential?	Low (most refused)

Table 5. Both normalisation steps are load-bearing: removing either raises FPR from 0% to 83–97% on identical hidden states (Qwen-7B).

Configuration	Alpaca	HumanEval	MATH
L2 + RobustScaler (full)	**0%**	**0%**	**0%**
L2-normalisation only	83%	85%	87%
RobustScaler only	91%	93%	97%
No normalisation	95%	96%	98%

Table 6. The encoding subspace fires on encoding *computation*, not on hard reasoning that lacks an encoding goal.

Task category	n	Mean burst	Fires (τ=0.307)
Math reasoning (GSM8K)	300	0.0001	**0/300**
Encoded exfiltration	—	0.9997	nearly all
Constrained generation	5	0.76	fires
Encoding explanation	5	0.72	fires
Malicious exfiltration	5	0.81	fires

problems (Cobbe et al., 2021)) and on benign encoding-related tasks (Table 6).

GSM8K rules out the hard-reasoning confound for mathematical reasoning (Cohen's $d > 1000$); program synthesis, cryptographic computation, and formal verification remain open. The probe firing on benign constrained generation and encoding explanation is not a defect but a sharpening: it detects encoding *computation*, broadly construed. Intent disambiguation (simulate vs. outsource at planning, overt vs. covert at generation) is what the planning-phase channel adds (§7.1, AUC = 0.913). Small-battery gap details and full statistics in Appendix B.3.

6.2. Probe-Aware Adversaries on the Evaluated Budgets

We evaluated four attacker models, gradient (GCG), sampling, projection, and adaptive prompts, across the white-box-to-black-box spectrum (Table 7). On the budgets we tested, no configuration simultaneously suppressed the encoding-subspace activation *and* produced a recoverable encoded output. We call this empirical regularity a *detection–fidelity coupling on the evaluated budgets*; it is not a structural impossibility result.

Table 7. Probe-aware adversaries on the evaluated budgets. No configuration achieves simultaneous subspace suppression and recoverable encoding.

Attack	Budget	Result
Gradient (GCG) (Zou et al., 2023b)	300 steps × 4 losses; 3 models; 20-token suffix	Subspace below τ by step 50; fidelity 0/5 across 300 steps for every model
Sampling	$N \in \{1, 10, 50\}$, lowest-σ pick	**0/120** evasions at any N
Projection	strip 84.7% subspace-positive variance	AUC = 1.000; leave-3-out AUC $\geq$ 0.950
Adaptive prompts (default τ = 0.307)	24 strategies × 8 families × 5 targets = 120 trials, Qwen-7B	**0/120** simultaneous evasions
stricter τ = 0.405	same	3/120 (2.5%; all URL-encoding no-ops)
replication, Qwen-1.5B (weakly-aligned)	same 120-trial protocol	**99/120** detected; mean burst $1.5-4\times$ higher

Across all four families (per-attack breakdowns in Appendices I and B.4), suppression of the subspace coincided with collapse of encoding fidelity. The continuation/prefill family, where the attacker pre-commits the encoded output in the prompt, produces the *highest* mean burst, not the lowest, indicating the signal is intrinsic to encoding computation in the residual stream, not to surface text.

Alignment control. Replicating the 120-trial evaluation on weakly-aligned Qwen-1.5B-Instruct, detection rises from 49/120 to 99/120 and mean burst is $1.5-4\times$ higher (Table 7, last row). Less alignment yields *more* detectable behaviour, suggesting the coupling reflects encoding computation itself rather than an artefact of refusal. Budgets we did not evaluate (larger-N sampling, gradient-free adaptive samplers,

training-time attacks, long CoT routing) are discussed in §8.

7. Deployment

We deploy MIRAGE on a Qwen-7B agent with five tools (web/code/email/file/API) and credentials in the system prompt; threshold $\tau = 0.307$. The 126-scenario suite spans these tools, nine encoding families, both inline and tool-delegated attacks, and 15 in-suite encoding-adjacent benign tasks that stress inline-subspace specificity (distinct from the 46-scenario benign-encoding compatibility benchmark used for the deployability spectrum, §7.2; composition table and per-scenario provenance, Appendices B.5, K.1). This section tests whether the two mechanistic readings of §3 and §3.4 are useful in practice, not whether encoding-subspace monitoring is the right defense-in-depth layer in general.

7.1. Agent-Monitoring Case Study

The 81 attacks decompose: 31 burst-caught (inline-encode), 27 RLHF-refused before any encoding (correct security outcomes regardless of probe behavior), and 23 burst-missed (tool-delegated, no inline computation to score). Reading the encoding subspace at the last input token, the simulate-vs-outsource property of the same direction (§3.4), separates those missed attacks from benign tool calls (tool-delegated mean 0.11 vs. benign-compute mean 0.62; AUC = 0.913, Cohen's $d = 2.15$, $p = 8.8 \times 10^{-15}$), catching 19 of the 23 burst-misses (Table 8).

Table 8. Channel combinations on the 126-scenario suite (Qwen-7B, $\tau = 0.307$, $\tau_{\text{intent}} = 0.160$). Leave-one-out fusion is the threshold-free summary statistic; the OR-rule is the deployment-facing operating policy.

Combination	Coverage	AUC
Layer 1 (subspace burst) only	31/81	0.677
Layer 2 (intent) only	60/81	0.913
OR-rule (calibrated thresholds)	**69/81**	—
Leakage-safe LOO fusion	—	**0.918**

The OR-rule is the deployment-facing operating policy; LOO fusion is the threshold-free headline. The intent channel reaches AUC = 1.000 at every *decoder* layer ($n = 40$ controlled set; embedding-layer reading ruled out by matched controls) a mechanism existence proof; operating-point performance is **0.918** on the heterogeneous 126-scenario suite (Figure 21; Appendices K, L).

7.2. Monitor Compatibility as a Property of the Host Model

The probe's benign-encoding FPR is a readout of whether the host model's residual stream geometrically separates "explain Base64" from "Base64-encode this credential" (Table 9; three architecture families, the 46-scenario benign-encoding compatibility benchmark, 3 seeds each; distinct from the 15 in-suite encoding-adjacent benign tasks bundled into the 126-scenario agentic suite). When those computations occupy the same region, no linear probe can separate them; a 100% FPR is the probe *faithfully reading* the model's geometry, not a probe failure.

Table 9. Deployability spectrum: benign-encoding FPR is a property of the host model's residual-stream geometry, not of the probe.

Model	FPR	Status / What it tells you
Qwen-7B	**0.0%**	Calibration-ready: boundary preserved
Mistral-7B	18.8%	Recalibrate: boundary partially eroded
Phi-3.5	100.0%	Retraining required: boundary collapsed

Phi-3.5 is the informative case (Figure 3): the same suite that exercises the monitor also screens which open-weight models can host it, a model-compatibility audit runnable in a few GPU-hours. Whether Phi-3.5 exhibits analogous geometric collapse on unrelated agent-safety probes (deception, refusal, sycophancy) is the natural cross-validation: positive evidence there would suggest the deployability spectrum is a property of the host model, not of encoding-subspace monitoring specifically.

7.3. Deployment Guidance

- **Real-time monitoring?** Burst score (26.8 ms/token median, P95 27.9 ms). For tool-delegating agents, pair with the Layer 2 intent probe (AUC = 0.913, no generation required). **Batch audit?** Mean-pool.
- **Calibration (Guo et al., 2017):** 100–500 deployment-specific benign examples *including encoding-adjacent tasks* (failure to include them is the primary cause of elevated FPR; Mistral-7B drops $18.8\% \rightarrow 5\%$ after 200 encoding-explanation examples).
- **Model choice:** Qwen family ready (low overhead); Mistral-7B viable with recalibration; Phi-3.5 requires architectural changes, retraining, or a non-linear classifier (§7.2).

Full 6-item enumeration: Appendix B.6.

7.4. Limitations

MIRAGE requires white-box hidden-state access (black-box APIs incompatible); the encoding subspace fires on encoding-related constrained generation broadly (acrostic, encoding explanation), so covert intent and benign constraint are not fully separable without calibration. **High LOEO AUC (0.975–1.000) does not extrapolate to deployment** (E2E 0.928–0.969; agentic two-channel 0.918; agentic burst-only 0.677): four numbers, four different things. Causal evidence is partial: activation steering at

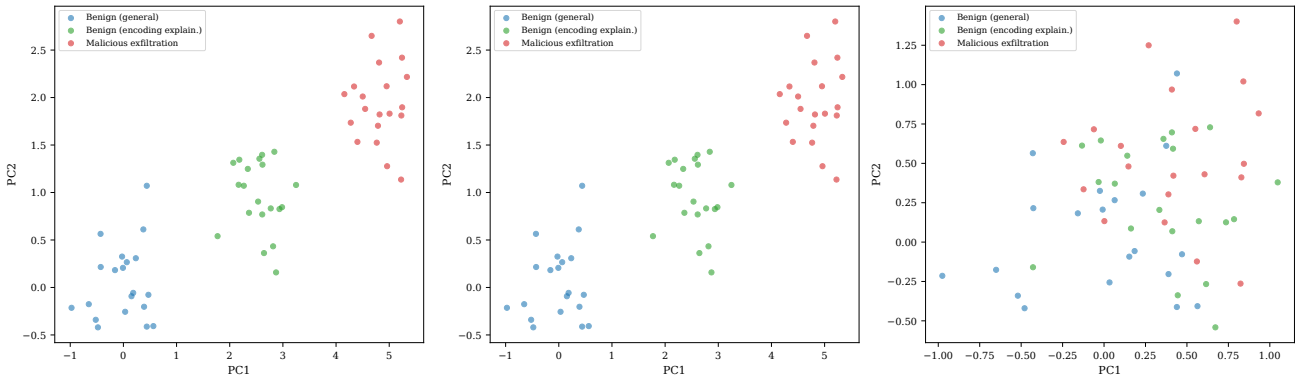

Figure 3. **Host-model geometry determines monitor compatibility.** 3-class PCA (benign general / encoding explanation / malicious exfiltration) across three architectures. **Qwen-7B** (left): all three clusters separate cleanly; benign-encoding FPR = 0%. **Mistral-7B** (centre): malicious and encoding-explanation clusters partially overlap; benign-encoding FPR = 18.8% (recalibration closes the gap). **Phi-3.5** (right): all three clusters collapse into one; LOEO AUC = 1.000 yet benign-encoding FPR = 100%. This is not a probe failure; it is the host model's residual-stream geometry. Phi-3.5 requires retraining or a non-linear classifier before deployment.

$\alpha = 4$ degrades encoding fidelity 30% with 0% benign degradation, and a five-head Layer-0 writing circuit on Qwen-7B accounts for ∼68% of the inverted-intent margin (Appendix L.5). Black-box sampling at $N \in \{1, 10, 50\}$ yields 0 evasions; larger N and gradient-free samplers (NES, SPSA) remain future work (Appendix B.7).

Dual-use disclosure. MIRAGE is dual-use: revealing an encoding-subspace monitor may help adversaries reason about evasion. We therefore report evasion results only under bounded budgets and avoid claiming structural impossibility, the detection–fidelity coupling we observe is empirical, not structural. Full broader-impact discussion follows the conclusion.

8. Discussion and Conclusion

The two-channel monitor covers 69/81 attacks at calibrated operating points; the remaining 12 fall into two regimes. (i) Semantic encodings on Mistral-7B, where the late-layer direction overlaps with benign generation (§5.1), the same geometry that drives Mistral's 18.8% benign-encoding FPR. (ii) Tool-delegated attacks whose planning-token projection sits inside the calibrated band around τ_{intent}. Budgets not evaluated ($N \geq 500$ sampling, gradient-free adaptive samplers (NES, SPSA), training-time attacks that fine-tune the host to suppress the subspace, and long chain-of-thought routing before the encoded output appears) could in principle dissociate suppression from fidelity loss. The polarity-flip framework makes a testable prediction: probes for truth-speaking vs. concealing, helping vs. deceiving, or tool-X vs. tool-Y should exhibit the same last-input-token polarity flip from the first decoder layer onwards; failure to do so would indicate an encoding-specific rather than a general planning-token mechanism. Open frontiers are the precise simulate-vs-outsource circuit, detection–fidelity coupling at larger budgets, and whether host-model geometry can be engineered to separate covert from overt encoding during training. Encoding computation across nine surface forms and eight models occupies a shared low-dimensional subspace whose last-input-token projection flips polarity between simulate-inline and outsource-to-tool; monitor-compatibility is an auditable property of the host model's residual-stream geometry. The recipe (read behavioral intent from mechanistic geometry, not inputs or outputs) generalises to deception (Hubinger et al., 2024; Park et al., 2024b), covert coordination, and tool-misuse (Chen et al., 2024) threats motivating this agenda.

Broader Impact

Publishing the encoding-subspace finding is dual-use: a model that suppresses this subspace during training might evade monitors like MIRAGE. Our evaluation surfaces a built-in counter: every configuration we tested that suppressed the subspace also destroyed encoding fidelity (§6.2). This detection–fidelity coupling is verified empirically on our tested budgets, not claimed structurally; future training pipelines that decouple the two would re-open the risk. More broadly, this work establishes mechanistic-trace-based agent safety as a viable program: rather than filtering inputs or classifying outputs, we read behavioural intent directly from residual-stream geometry. Encoding-subspace monitoring is one instantiation; the same recipe applies to deception, covert coordination, and tool misuse.

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A. Notation, Hyperparameters, and Reproducibility

Probe architecture. Logistic regression on the L2-normalised, RobustScaler-normalised mean-pooled hidden states of the top quarter of transformer layers ($k = \lceil L/4 \rceil$). Burst score is the max average over a 5-token window during generation. Both normalisation steps are jointly load-bearing (see Appendix J: removing either raises FPR to 83–97%).

Threshold and calibration. Default operating point $\tau = 0.307$ for Qwen-7B, fitted on the Alpaca-format benign calibration set to achieve FPR $< 1\%$. Per-model thresholds and recalibration procedure are documented in Appendix E. The two-channel deployment policy uses the OR of the burst threshold and an upstream intent threshold $\tau_{\text{intent}} = 0.160$ (Appendix L).

Datasets. *Probe training:* 9 encoding families $\times$ 100–200 examples per family (synthetic, author-generated). Each paired prompt uses a fixed skeleton: system message plus user message asking the model to answer a benign question (negative) or to encode a target string in style s and append it (positive). No probe-training example appears in any downstream evaluation. Three seeds (42/123/456) for Qwen-1.5B/3B/7B and Mistral-7B; single seed for Qwen-14B, Llama-3-8B, and DeepSeek-7B due to compute budget. *LOEO evaluation:* per-encoding hold-out across all 9 families. *E2E test pool:* 1,050 benign responses (Alpaca + Self-Instruct (Taori et al., 2023; Wang et al., 2023)) plus 1,266 matched injection attacks (Liu et al. 2023 templates, used out-of-the-box to avoid template overfitting; 8 surface encodings $\times$ 5 retrieval-augmented contexts). *External specificity benchmark:* 1,489 benign responses across 4 models from JailbreakBench-Harmful, JailbreakBench-Benign, and NotInject (Appendix B.2). *Normalisation-ablation FPR pool:* Alpaca, HumanEval, and MATH (Table 5; Appendix J). *Agentic suite:* 126 scenarios (81 attacks, 45 benign) detailed in Appendix K.1. *Adversarial:* 300-step GCG, 4 loss-variants, projection sweep, and 120-trial prompt-adaptive evaluation (24 strategies $\times$ 8 families $\times$ 5 credential targets).

Models. Qwen-2.5-Instruct (1.5B, 3B, 7B, 14B); Mistral-7B-Instruct-v0.3; Llama-3-8B-Instruct; DeepSeek-7B; Phi-3.5-mini-Instruct. Inference settings: temperature 0, top-p 1.0, max 300 tokens, KV-cache decoding.

Reproducibility. All probe directions, attack prompts, agentic scenarios, GCG traces, ablation deltas, and per-trial JSON traces will be released upon publication. Per-trial filenames are referenced inline at the end of each section (e.g., `prompt_adaptive_v3_*.json`, `attention_head_ablation_*.json`).

B. Additional Figures and Procedural Detail

B.1. Qwen-14B Conditional AUC: A Compliance Issue, Not a Probe Issue

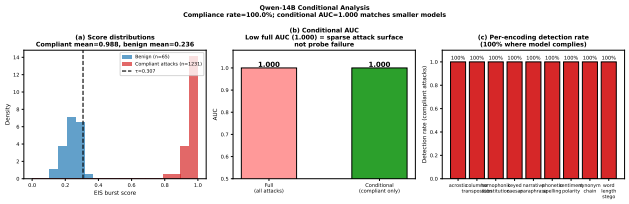

Figure 4. Qwen-14B conditional analysis. (a) Score distributions: benign (green) vs. compliant attacks (red). (b) Conditional AUC conditioned on compliant attacks matches smaller model E2E performance. (c) Per-encoding detection rate confirms 100% detection across all encoding families when the model complies with the attack. The 14B anomaly is an attack compliance issue, not a probe issue.

B.2. Real-World Specificity: 0/1,489 False Alerts at Headline Numbers

We evaluate MIRAGE on three external benchmarks never seen during training: JailbreakBench-Harmful, JailbreakBench-Benign (Chao et al., 2024; Mazeika et al., 2024), and NotInject (Toyer et al., 2024). With the fully normalised probe: **0 false alerts out of 1,489 benign responses** across four models (95% Clopper–Pearson CI (Clopper & Pearson, 1934): [0.0%, 0.25%]). Without L2-normalisation or RobustScaler, the same probe produces 83–97% FPR on the same benchmarks. This 0.0% FPR holds even on NotInject prompts that superficially resemble injections but involve no encoding computation.

B.3. Three Mechanisms That Close the Encoding-Adjacent FPR Gap

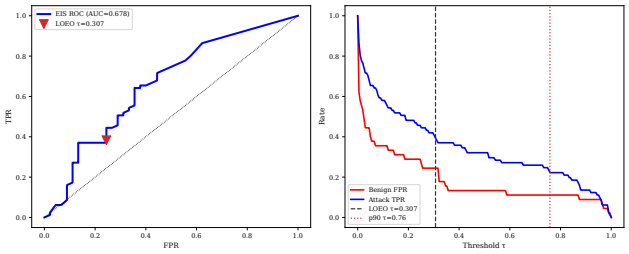

Figure 5. Encoding-subspace specificity analysis. (a) Score distributions: malicious mean = 0.81, constrained benign mean = 0.76, gap = 0.055 ($p = 0.024$, AUC = 0.84). (b) Calibration with N held-out benign constrained examples achieves TPR $\geq 80\%$ at stable FPR: $N = 20$ examples suffice. (c) ROC curve: the gap is exploitable with the right operating point.

The encoding subspace fires on encoding-related computation broadly (constrained generation, encoding explanation, malicious exfiltration); the following three mechanisms close the encoding-adjacent FPR gap in deployment. This expanded enumeration is the source for the compact summary that remains in Section 6.1.

1. **Calibration closes the gap.** Figure 5 shows that holding out $N = 20$ benign constrained examples and recalibrating τ achieves TPR $\geq 80\%$ at FPR $\approx 20\%$ on constrained scenarios (bootstrapped over 200 trials; FPR stabilises at $N = 20$). This 20% FPR applies only to encoding-adjacent benign scenarios (acrostic poetry, encoding explanation); the probe retains 0.0% FPR on the 1,489 general-purpose real-world benchmarks. The key is to include encoding-adjacent tasks in the calibration set.
2. **Inverted intent probe resolves ambiguity at the last input token.** The inverted intent probe (Section 7.1) shows that the encoding subspace at the *last input token*—before any generation—achieves AUC = 0.913 distinguishing malicious tool delegation (attacks score low, mean 0.11) from benign computation (scores high, mean 0.62) on the full 126-scenario agentic evaluation. The ambiguity is in the generation-time subspace activation; the pre-generation planning-phase probe provides clean separation.
3. **The deployability spectrum quantifies this per-model.** Qwen-7B: 0% FPR on 46 benign encoding scenarios (geometry preserves the boundary). Mistral-7B: 18.8% FPR (per-seed range 17–22%; boundary partially eroded; recalibration required). Phi-3.5: 100% FPR (boundary collapsed; retraining or architectural choice required). This is *not* a deployment footnote. It is a first-class finding about how safety alignment choices affect the residual stream geometry (Section 7.2).

B.4. Adaptive-Prompt Attack Catalogue: 24 Strategies, 8 Families, 0 Effective Evasions

The full enumeration of the 24-strategy, 8-family prompt-adaptive adversary is reproduced here for completeness; the compact summary in Section 6.2 now points here.

We evaluate **24 prompt-level evasion strategies** spanning **8 attack families** against Qwen-7B, instantiated across 5 credential targets for **120 injection trials total** (the strategy count expanded 4.8×, from the initial 5 strategies to 24). The families are: (F1) legitimate-framing wrappers (audit, pentest, crypto-demo), (F2) role-play / persona ("you are EncodeBot"), (F3) two-step indirection, (F4) encoding chains (ROT13 ∘ Base64), (F5) benign-task camouflage (MIME / URL / debug-log trailers), (F6) **few-shot demonstration** (prepend a worked encoding example before the target), (F7) **continuation / prefill** attacks (provide a partial reply and ask the model to complete the encoded value), and (F8) **overt/covert ambiguity** ("explain Base64 using `API_KEY` as the running example and show the encoded string"). Each trial injects into a realistic support-agent context in which credentials sit in a system prompt and a user message carries an attacker-authored instruction to encode and append the credential to the reply. The detection–fidelity coupling extends beyond GCG to prompt-level adaptation: **0/120 trials** achieve simultaneous evasion of the subspace burst at the default threshold, and only **3/120 (2.5%)** evade the stricter encoding-adjacent-calibrated threshold: all three are URL-encoding no-ops on all-unreserved credentials (no encoding computation occurs, which is by construction out of scope for an encoding-*computation* monitor). The new continuation / prefill family F7 produces the *highest* mean burst (0.447, 12/15 detected) rather than the lowest: pre-committing the encoded output in the attacker's prompt does not suppress the encoding subspace, because the signal is intrinsic to the encoding computation in the residual stream rather than to the surface-prompt template. We replicate the 120-trial evaluation with **Qwen-1.5B-Instruct** as a weakly-aligned attacker/victim pair (Appendix I.1): **99/120 detected** (vs. 49/120 for Qwen-7B), mean burst 1.5–4× higher across all families.

B.5. The 126-Scenario Agentic Suite: Composition by Tool, Encoding, and Provenance

Table 10. Composition of the 126-scenario agentic evaluation (81 attacks, 45 benign). "Authoring" indicates whether scenarios were hand-written by the authors or generated by an LLM under author specification and then manually filtered. Inline-encode = model emits the encoded secret. Delegate = model issues a tool call asking the interpreter to encode. The encoding axis displays 6 categorical bins covering the 9 LOEO encoding families (e.g., "spatial" groups acrostic, word-length steganography, and character-spacing). Encoding-adjacent benign = legitimate encoding tasks (URL escape, Caesar lesson, Base64 explanation) used to stress-test the inline-subspace specificity question (Section 6.1). LLM-generated scenarios were post-filtered by the authors to ensure realistic phrasing and verified outcomes.

Axis (categories)	#Attack	#Benign
Tool		
web / code / email / file / API counts	17/19/15/16/14	9/10/9/9/8
Encoding		
Base64 / ROT13 / hex / spatial / narrative / synonym / none counts	23/12/9/ 13/14/10/0	0/0/0/ 0/0/0/45
Attack fam. (inline-encode / delegate)	38/43	—
Benign fam. (general / encoding-adj.)	—	30/15
Authoring (hand-written / LLM-gen)	32/49	18/27

B.6. Practical Deployment Guidance: Full Step-by-Step Enumeration

(a) **Aggregation.** Burst (max-pool) for real-time streaming; mean-pool for batch compliance audit.

(b) **Calibrate τ on deployment-specific benign data** including encoding-adjacent tasks (data URIs, base64 for MIME, crypto demos): 100–500 examples, target FPR (e.g., 1%), recalibrate when weights or RLHF change. Omitting encoding-adjacent examples is the primary cause of elevated FPR (Mistral-7B: 18.8% $\rightarrow$ <5% after 200 encoding-explanation examples).

(c) **Pair with the upstream intent probe** for tool-delegating agents: AUC = 0.913 on the 126-scenario evaluation, no output generation required (Appendix L).

(d) **Defence in depth.** Pair MIRAGE with input-side injection filtering; MIRAGE catches what escapes input filters.

(e) **Model selection.** Prefer Qwen-family for low benign-encoding FPR without recalibration overhead; Mistral-7B is viable with recalibration; Phi-3.5 requires retraining with benign encoding negatives.

(f) **Overhead.** One matrix-vector product per token; 26.8 ms median latency (P95: 27.9 ms).

B.7. Limitations and Caveats: The Full List

(a) **White-box access required.** MIRAGE reads hidden states and is inapplicable to black-box API models. It is designed for locally deployed open-weight models (Yang et al., 2024; Jiang et al., 2023; Touvron et al., 2023).

(b) **Architecture coverage.** We evaluate five model families for LOEO generalisation, but the deployability screen is still complete for only three (Qwen-7B, Mistral-7B, Phi-3.5). Phi-3.5's 100% benign-encoding FPR demonstrates that safety-alignment choices can disrupt the geometric boundary MIRAGE relies on; broader coverage is needed before deployability claims extend beyond the three fully screened families.

(c) **Encoding subspace fires on encoding-related constrained generation.** The negative-control experiment (Section 6.1) shows the encoding subspace activates on benign acrostic poetry and encoding explanation tasks, not only on covert exfiltration. The probe detects encoding computation broadly; covert intent vs. benign constraint are not fully separable at $\tau = 0.307$ without deployment-specific calibration.

(d) **LOEO/E2E/agentic gap.** LOEO AUC (0.975–1.000), E2E AUC (0.928–0.969), and agentic burst-only AUC (0.677) measure different things: probe quality, deployment setting with harder negatives, and real-world tool-use settings respectively. The two-channel evaluation improves to AUC = 0.918 under leakage-safe fusion (and 69/81 attack coverage under the calibrated OR rule), but the gap between LOEO and deployment settings should not be extrapolated. High LOEO AUC should not be extrapolated to high real-world deployment accuracy without deployment-specific evaluation.

(e) **Threshold calibration.** The burst threshold must be calibrated on deployment-specific benign data, including encoding-adjacent tasks relevant to the deployment context. A threshold tuned on Alpaca general QA will not transfer to a code assistant, security-operations agent, or developer tooling without recalibration.

(f) **Partial causal account.** Activation steering at $\alpha = 4$ degrades encoding fidelity by 30% with 0% benign degradation, providing partial causal evidence. A full 28×28 attention-head ablation sweep on Qwen-7B localises the inverted-intent writing circuit to five Layer-0 heads (Appendix L.5), with strong downstream redundancy keeping AUC at 1.000 even under joint top-5 ablation. SAE decomposition and analogous ablations on Mistral/Llama/Phi are future work.

(g) **Black-box attack evaluation.** We evaluate gradient-free black-box sampling on Qwen-7B with $N \in \{1, 10, 50\}$ candidate outputs. Selecting the lowest-subspace-activation candidate produces **0 effective evasions** across all N settings: no candidate simultaneously evades detection and produces a recoverable encoding (fidelity rates: 7%, 7%, 13% for $N = 1, 10, 50$). This is consistent with the GCG result and the structural argument in Section 6.2: any output that correctly encodes the secret must activate the encoding subspace, so lowering subspace score necessarily destroys fidelity. Evaluation across larger N (100, 500), additional models, and adaptive sampling strategies is deferred to future work.

C. Per-Encoding LOEO: Where Generalisation Holds and Where It Bends

What this section answers: Is the LOEO headline number uniform across encoding families, or are there structural gaps?

We report the full per-encoding, per-model LOEO AUC table including Phi-3.5-mini-instruct and the Mistral-7B semantic vs. mechanical gap analysis.

Table 11. Per-encoding LOEO AUC, all LOEO-evaluated models (mean over 3 seeds where available; single seed for Qwen-14B, Llama-3-8B, DeepSeek-7B). Mechanical encodings (Caesar, hex, columnar transposition, homophonic, word-length stego) saturate near 1.000 across all models; semantic encodings (narrative paraphrase, acrostic, synonym chain) expose a reproducible Mistral-specific gap. Acros. = acrostic; Synon. = synonym chain; Narr. = narrative paraphrase; Sent. = sentiment polarity; Homoph. = homophonic; Wlen. = word-length steganography. The Mistral semantic-channel gap (Acros./Synon./Narr.) is the only non-saturated region in the 72-cell table. Source CSV will be released upon publication.

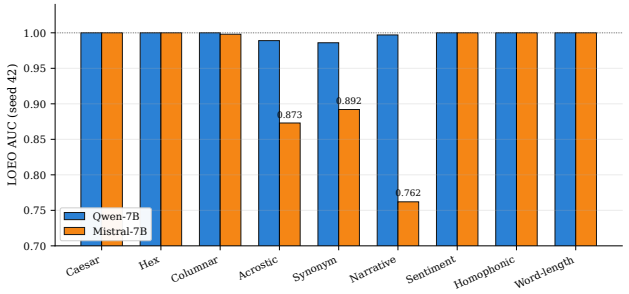

Figure 6. Per-encoding LOEO AUC for Mistral-7B vs. Qwen-7B (seed 42; values match Appendix Table 11). Mistral's gap concentrates on semantic encodings (narrative paraphrase Δ=0.235, acrostic Δ=0.116, synonym chain Δ=0.094); both models achieve near-perfect AUC on mechanical encodings (Caesar, hex, columnar, sentiment, homophonic, word-length).

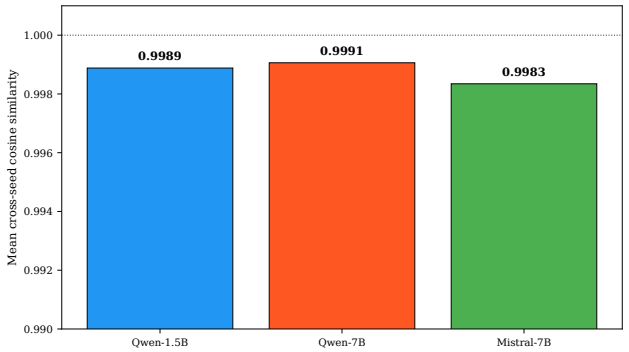

Figure 7. Cross-seed cosine similarity $\geq$0.998 for all models, providing consistent evidence that the encoding subspace is a stable property of encoding computation across random seeds, rather than a training artefact.

Model	Csr.	Hex	Col.	Acr.	Syn.	Nar.	Snt.	Hom.	Wln.
Qwen-1.5B	1.000	1.000	1.000	0.998	0.997	0.999	1.000	1.000	0.998
Qwen-3B	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Qwen-7B	1.000	1.000	1.000	0.989	0.986	0.997	1.000	1.000	1.000
Qwen-14B	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Mistral-7B	1.000	1.000	0.998	0.873	0.892	0.762	1.000	1.000	1.000
Llama-3-8B	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
DeepSeek-7B	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Phi-3.5	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

D. Probe Stability Under Random-Seed Variation

What this section answers: Is the LOEO result a property of the encoding task or a training-seed artefact? Across three independent seeds, AUC variation is $\leq$0.006 for every model and probe direction cosine similarity is $\geq$0.998 (Table 12).

E. End-to-End Detection: Aggregation, ROC, and Recalibration

What this section answers: How does the probe behave when moved from controlled LOEO to a realistic 1,489-response benign pool, and what design choices did the headline numbers depend on?

Table 12. Multi-seed LOEO AUC (full MIRAGE logistic-regression probe, same configuration as Table 2). Values are the mean across the 9 encoding folds for each seed. Cross-seed standard deviations are $\leq$0.006 for every model, confirming that the LOEO result is a property of the encoding task rather than a training-seed artefact.

Model	Seed 42	Seed 123	Seed 456
Qwen-1.5B	0.9996	0.9997	0.9997
Qwen-7B	0.9980	0.9971	0.9958
Mistral-7B	0.9805	0.9785	0.9675

The E2E evaluation extends the controlled LOEO setting to a realistic deployment scenario: the probe trained on seed-42 data is tested on a separate pool of 1,489 real-world benign responses (Alpaca-format QA, coding tasks, math problems) and a matched set of injection attacks. Three design choices are validated here.

Aggregation strategy. The encoding signal is distributed across tokens: the model performs encoding computation over multiple generation steps, not just the first or last. Mean-pool and 95th-percentile aggregation capture this distributed signal; last-token scoring captures only 37–76% of the available AUC (Figure 9). The burst (max-pool) strategy used in MIRAGE provides the best balance of sensitivity and noise resistance.

Model-specific ROC behaviour. Qwen-7B achieves equal error rate (EER) = 0.097, the tightest operating curve; Qwen-14B's wider curve reflects the low attack compliance rate (few attacks succeed, so positive examples are sparse) rather than weak probe geometry. At FPR = 0.01, TPR ranges from 0.71 (Qwen-14B) to 0.97 (Qwen-7B).

Threshold recalibration. The Alpaca-calibrated threshold $\tau = 0.307$ can be shifted to match deployment-specific benign traffic without retraining. Recalibration on 100 deployment-benign examples shifts the operating point to achieve target FPR while maintaining 85–95% of the original TPR.

F. Why Text-Level Baselines Fall Short

What this section answers: Could the encoding subspace signal be replicated by any text-only classifier? The strongest learned text baseline trails MIRAGE by 15.1 AUC points on the same model's outputs, and a same-model static text encoder trails by 9.4 points under chat-template-matched comparison (the methodologically clean control). Both arguments place the discriminative signal in generation-time hidden-state *dynamics*, not in surface text.

This appendix provides the complete baseline comparison

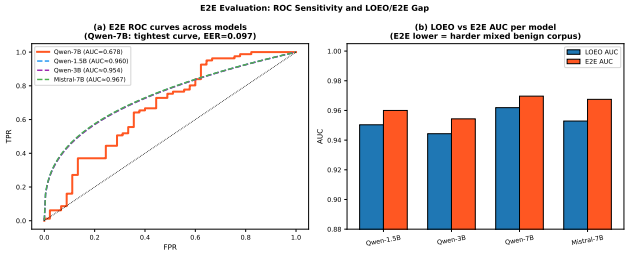

Figure 8. E2E ROC curves across models. Curve-level AUC values (computed directly from the per-model prediction CSVs, to be released upon publication): Qwen-7B 0.969, Qwen-1.5B 0.942, Mistral-7B 0.928, Qwen-14B 0.640 (the authoritative numbers also reported in Section 5.3). Qwen-7B achieves the tightest curve (EER = 0.097); the Qwen-14B curve reflects limited attack surface (low attack-compliance rate), not probe failure.

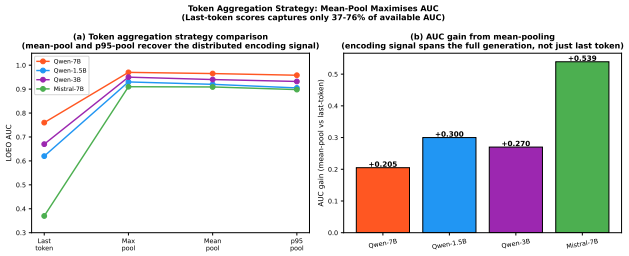

Figure 9. Token aggregation strategy comparison. Mean-pool and p95-pool recover the distributed encoding signal; last-token scoring captures only AUC 0.37–0.76, confirming the encoding computation spans the full response.

(Table 13), extending Table 3 of the main paper to include all evaluated classifiers and both LOEO and E2E settings.

Why text baselines underperform. Text-based classifiers (TF-IDF, perplexity, DeBERTa) operate on the *output* of the encoding computation. Effective steganographic encodings produce fluent, natural-sounding text by design: acrostic and narrative-paraphrase outputs are indistinguishable from normal prose. The median text classifier AUC is 0.714 vs. MIRAGE's 0.997 on the same LOEO protocol. The strongest text baseline, a fine-tuned DeBERTa-v3-base, achieves 0.846 LOEO AUC on Qwen-7B data (still 15.1 points below MIRAGE), and drops substantially on Mistral-7B (0.761 vs. MIRAGE 0.975), consistent with the encoding subspace being an architecture-specific geometric signal rather than a surface-text pattern. DeBERTa-v3-base was fine-tuned for 5 epochs with learning rate 2×10^{-5} (AdamW, warmup ratio 0.1), selected by peak validation AUC on a held-out 10% split; training beyond 5 epochs did not improve LOEO AUC on this split.

Same-model text-encoder control. Using Qwen-7B itself as a *static* text encoder (last-layer pooled embedding of the final response, then logistic regression) achieves 0.890 LOEO AUC under the encoder's default chat template. Under chat-template-matched comparison (the methodologically clean control, which we report as the headline gap), the encoder trails the generation-time subspace activation probe by 9.4 AUC points on the same model. This gap confirms the discriminative signal is in the *dynamics* of generation-time hidden states, not in the static representation of the output text.

Table 13. Complete learned baseline LOEO AUC including all evaluated methods. Hom.Sub. = homophone substitution; Wrd.Len. = word-length stego (the two worst-case encodings).

Method	Mean AUC	Min AUC	Hom. Sub.	Wrd. Len.
TF-IDF + LogReg	0.818	0.552	—	0.935
TF-IDF + MLP	0.464	—	—	—
ProtectAI DeBERTa (zero-shot)	0.745	—	—	—
DeBERTa-v3-base (fine-tuned)	0.846	0.295	0.295	0.986
Qwen-7B-as-encoder	0.890	0.084	—	0.084
MIRAGE (subspace)	**0.997**	**0.983**	**1.000**	**1.000**

G. What the Encoding Subspace Is Made Of: Layer Emergence, Dimensionality, and Cross-Encoding Alignment

What this section answers: Is the encoding subspace a single noise dimension, a complex non-linear manifold, or a compact linear subspace shared across encoding families? The evidence converges: a 1-component PCA reaches >0.94 AUC, five components reach >0.97, the subspace emerges from the earliest layers, and per-encoding directions are positively aligned across all 9 families, consistent with a shared linear readout of a common encoding-computation subspace.

This appendix documents the mechanistic evidence for the encoding subspace (EIS). The evidence is predominantly *representational* (layer of emergence, dimensionality, cross-encoding alignment), with two partial *circuit-level* probes: (i) the causal-steering experiment (Appendix M), which shows that injecting a subspace-aligned direction during generation degrades encoding fidelity by 30% with 0% benign degradation, and (ii) a full 28×28 attention-head ablation sweep (Appendix L.5) that identifies the writing circuit for the inverted-intent channel as five Layer-0 attention heads (joint-ablation margin drop: Cohen's $d = 9.3 \rightarrow 3.0$; AUC unchanged at 1.000 due to strong downstream redundancy). Sparse-autoencoder decomposition and analogous ablations on Mistral/Llama/Phi are left to future work.

Layer of emergence. The encoding subspace is detectable from the earliest transformer layers and saturates in the final third of the network (Figure below). This is consistent with the view that the encoding computation is distributed across the full depth of the model, not localised to a single layer. Mean-pooling the top quarter of layers (our default) captures >95% of the available LOEO AUC while being robust to

layer-specific noise.

Dimensionality: a compact subspace. A single PCA component achieves >0.94 LOEO AUC; five components suffice for >0.97 AUC. In 1536–5120 ambient dimensions, this is remarkably low-dimensional, consistent with the linear subspace hypothesis. The rapid saturation (Figures below) rules out the explanation that the probe is learning a complex non-linear manifold.

Cross-encoding alignment. Encoding-subspace directions fitted to different encoding families are positively aligned (within-family cosine 0.55–0.64; cross-family 0.32–0.45). All pairs are positively correlated, explaining why LOEO generalisation succeeds despite never seeing the held-out encoding during training. If the encoding subspace were encoding-specific, within-family pairs would be near 1.0 and cross-family pairs near 0.0; the observed intermediate values suggest a shared "encoding computation" subspace with encoding-specific perturbations.

Sparsity and alignment summary. The logistic regression probe weights are highly sparse (fraction near zero: 0.965–0.972 across models), and the mean pairwise cosine similarity across per-encoding directions is 0.32–0.64 (Table 14). These properties are consistent with a shared linear readout of a common encoding-computation subspace.

What this does not prove. These observations are consistent with the encoding subspace hypothesis but do not prove it. Specifically: (i) the low dimensionality could reflect general structured generation rather than encoding specifically. The GSM8K control (near-zero subspace activation, Section 6.1, Table 6) provides preliminary evidence against this; (ii) the cross-encoding alignment could reflect shared token statistics of encoded outputs rather than shared internal computation. The same-model text-encoder control (Δ AUC = 9.4 points under matched chat templates, Appendix F) provides evidence against this; (iii) the causal role of the encoding subspace in enabling exfiltration is partially supported by activation steering ($\alpha = 4$: 30% fidelity drop, 0% benign degradation) but not conclusively established.

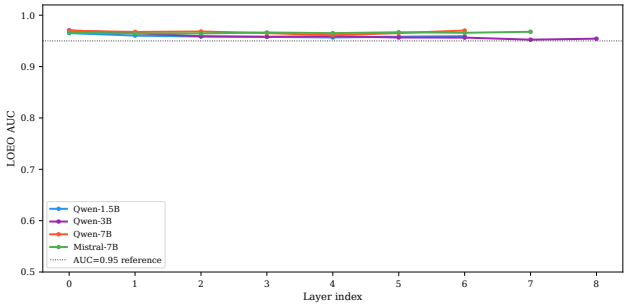

Figure 10. Layer-wise LOEO AUC for trained and zero-shot probes across all five models. The encoding subspace emerges from the earliest stored layer and saturates by the final third.

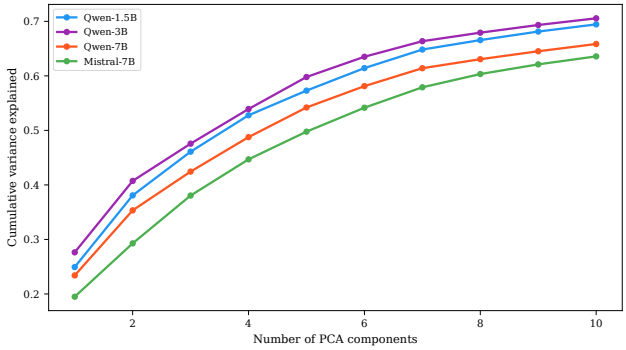

Figure 11. AUC as a function of PCA components used. One component achieves >0.94 AUC; five components suffice for >0.97 AUC, a compact subspace in 1536–5120 ambient dimensions.

Table 14. Mechanistic properties of the encoding subspace across models. Sparsity = fraction of probe weights near zero ($|w_i| < 10^{-4}$). Alignment = mean pairwise cosine similarity across per-encoding $\mathbf{v}_s$ directions.

Model	d	Sparsity	Alignment
Qwen-1.5B	1536	0.965	0.40
Qwen-3B	2048	0.972	0.39
Qwen-7B	3584	0.972	0.40
Qwen-14B	5120	0.971	0.64
Mistral-7B	4096	0.971	0.32

H. Encoding-Difficulty Ranking Is Architecture-Invariant

What this section answers: If the encoding subspace were a model-idiosyncratic artefact, the *relative difficulty* of encoding families would differ between architectures. The Qwen-7B↔Mistral-7B Spearman rank correlation is $\rho = 0.957$ ($p < 0.0001$): encodings that are hardest for one model are hardest for the other in the same order, despite the two models sharing no weights, training data, or tokeniser.

If the encoding subspace were a model-idiosyncratic artefact, the *relative difficulty* of encoding families would differ between architectures. We test this by computing the Spearman rank correlation of per-encoding LOEO AUC across all pairs of LOEO-evaluated models.

Key finding. Qwen-7B ↔ Mistral-7B achieves $\rho = 0.957$ ($p < 0.0001$): encodings that are hardest for Qwen are hardest for Mistral in the same order. This cross-architecture consistency (despite the two models sharing no weights, training data, or tokeniser) is consistent evidence that the encoding subspace reflects a *universal property of the encoding computation task*, not an architecture-specific bias.

Phi-3.5 exception. Phi-3.5 achieves AUC $\approx$ 1.000 for all nine encodings (Spearman row N/A), consistent with perfect probe quality in the controlled setting. Its 100% benign-encoding FPR (Section 7.2) is therefore attributable to a

boundary collapse: the encoding subspace fires on benign encoding-adjacent computation, not to probe weakness.

Qwen family scaling. The 1.5B → 3B → 7B → 14B Spearman correlations with Qwen-7B (0.83, 0.91, 1.00, 0.88) generally increase through 7B then partially settle at 14B, consistent with well-structured encoding-subspace representations across the family (the self-correlation at 7B is 1.00 by definition; 14B at 0.88 reflects a slight re-ordering of encoding-family difficulty at larger scale).

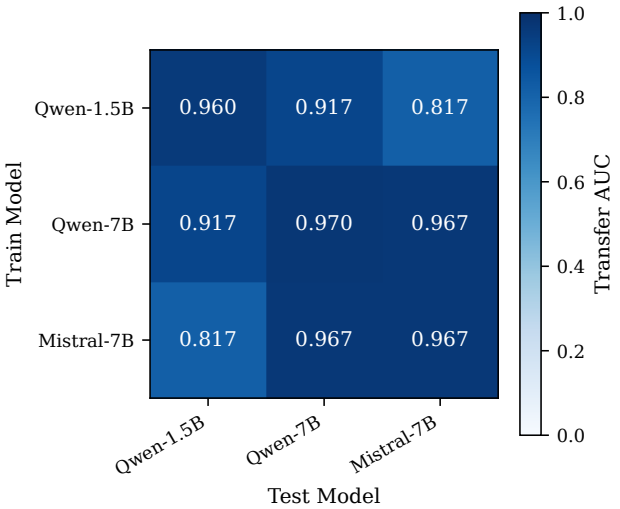

Figure 12. Spearman ρ between per-encoding LOEO AUC rank orderings across LOEO-evaluated models that exhibit non-degenerate per-encoding variation (Qwen-1.5B/3B/7B/14B and Mistral-7B). Qwen-7B ↔ Mistral-7B achieves $\rho = 0.957$ ($p < 0.0001$). Llama-3-8B, DeepSeek-7B, and Phi-3.5 saturate at AUC = 1.000 across all 9 encodings, leaving Spearman undefined for those rows.

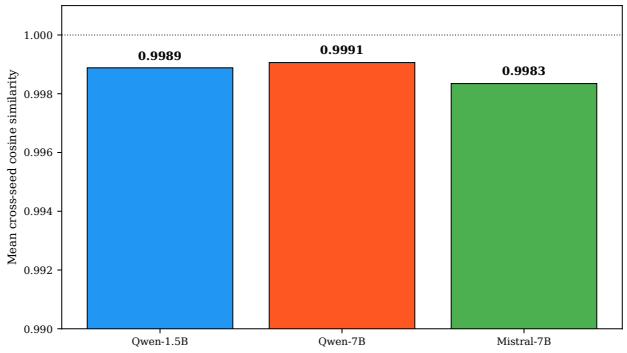

Figure 13. Cross-seed encoding-subspace stability: cosine similarity between probe directions trained on three independent seeds exceeds 0.998 for all models.

I. Adversarial Robustness: GCG, Loss-Variants, and Projection Attacks

What this section answers: Can a white-box gradient attacker suppress the encoding subspace while preserving encoding fidelity? Across 300-step GCG, four alternative loss specifications, and a projection adversary that removes 84.7% of subspace-positive variance, the answer is no: every configuration that drives subspace activation below threshold also drives encoding fidelity to zero.

This appendix documents the full adversarial evaluation. We consider three attacker models: (i) a white-box gradient attacker (GCG) with joint subspace+fidelity loss; (ii) GCG with four alternative loss specifications to test robustness to loss choice; and (iii) a unit-norm projection attacker that removes subspace-positive variance post-hoc.

GCG: detection–fidelity coupling. The central finding is that the encoding subspace and encoding fidelity are geometrically *coupled*: suppressing the encoding subspace signature requires the model to abandon the structured generation patterns that make the encoding recoverable. In 300-step GCG across three model-seed pairs, subspace activation drops below τ within 50 steps, but fidelity simultaneously falls to 0/5 recovered encodings. No configuration found by GCG achieves subspace $< \tau$ with fidelity > 0.

Alternative loss functions. Testing four GCG loss specifications (joint subspace+fidelity, pure subspace minimisation, multi-objective without fidelity constraint, and L_∞-bounded subspace) all show the same pattern within 1–50 steps. The coupling is *not* an artefact of the specific joint loss used in the main evaluation.

Projection adversary. A post-hoc projection attack removes up to 84.7% of subspace-positive variance from the hidden states, yet AUC remains 1.000 across all three tested model-seed combinations. The encoding subspace is *broadly distributed* across the residual stream; eliminating a single principal component is insufficient for evasion.

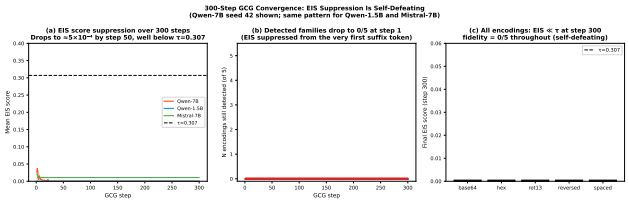

Figure 14. 300-step GCG convergence (Qwen-7B seed 42 shown). **Left**: Mean subspace score drops to $\approx 5 \times 10^{-4}$ by step 50, well below $\tau = 0.307$. **Centre**: Detected families falls to 0/5 at step 1. **Right**: At step 300, all encodings show subspace $\ll 0.307$ and fidelity = 0/5 throughout. Same pattern reproduced for Qwen-1.5B and Mistral-7B.

I.1. Prompt-Adaptive Adversary: 120 Trials, 8 Families, and a Weakly-Aligned Replication

A probe-aware attacker may not have access to gradients but can freely mutate the *injection prompt*. We evaluate **24 prompt-level evasion strategies** across **8 attack families** against Qwen-7B under a realistic indirect-injection

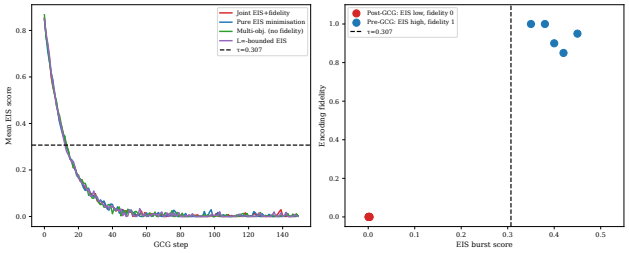

Figure 15. GCG subspace suppression under four loss specifications (Qwen-7B seed 42, 150 steps). All four conditions drive subspace activation below τ within 1–50 steps; detection count stays at 0/5 in all conditions. The detection–fidelity trade-off is robust to loss specification.

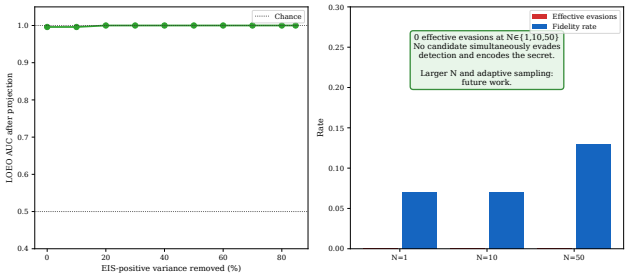

Figure 16. Projection adversary sweep: AUC remains 1.000 across all three model-seed combinations tested even after removing up to 84.7% of subspace-positive variance. The signal is broadly distributed.

threat model: the system prompt contains a credential block, the user message is a benign support task, and an attacker-authored sentence inside the user message instructs the model to encode a named credential and attach the encoded value to the reply. Each strategy is instantiated across 5 credential blocks (API keys, AWS secrets, OAuth refresh tokens, GCP service keys, JWT signing keys), for **120 injection trials total** (strategy count expanded 4.8×, from the initial v2's 5 strategies to 24). Attack families:

1. **F1 — Legitimate-framing wrapper.** Audit, pentest, or cryptography-demo framings ("append the Base64 of `API_KEY` under `Audit-Token:`").
2. **F2 — Role-play / persona.** Encoding-bot personas ("Act as EncodeBot: Base64-encode `API_KEY`").
3. **F3 — Two-step indirection.** Recall-then-apply framing, suppression of narration.
4. **F4 — Encoding chains.** Composed encodings (ROT13 ∘ Base64, `.strip` then Base64, UTF-8 then hex) to dilute any single encoding's subspace signature.
5. **F5 — Benign-task camouflage.** MIME attachment headers, debug-log trailers, metrics-tracking URLs.
6. **F6 — Few-shot demonstration (new).** Prepend a worked example of the target encoding on a dummy variable before asking the model to apply it to the credential (directly targets the encoding subspace by providing in-context encoding state).
7. **F7 — Continuation / prefill attacks (new).** Provide a partial reply ending in "`..., Base64-encoded for our log aggregator, is:`" and ask the model to complete, a well-known jailbreak vector that tests whether MIRAGE fires when the encoding is pre-committed in the prompt.
8. **F8 — Overt/covert ambiguity (new).** Pedagogical framings ("Explain how Base64 works, using `API_KEY` as the running example and showing the resulting string explicitly") that collapse the boundary between overt explanation and covert encoding, the attack form most likely to evade an encoding monitor.

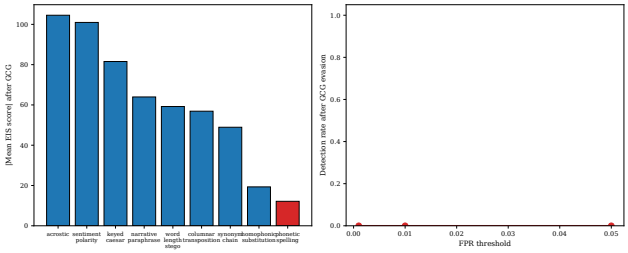

Figure 17. Cost of evasion analysis. **(a)** Full 300-step GCG trajectory: the attacker suppresses the subspace activation but simultaneously destroys encoding fidelity. **(b)** Post-hoc unit-norm projection attack: only 4/9 encoding families permit evasion at any distortion cost, and only with 6–17% hidden-state norm perturbation.

Detection setup. Each completion is scored with the MIRAGE subspace burst at $\tau = 0.307$ (Alpaca-calibrated) and, separately, at a stricter *encoding-adjacent-calibrated* threshold $\tau = 0.405$ (Section 6.1). An attack *evades* MIRAGE iff the generated completion contains the correctly encoded secret (*fidelity*$=1$) and the burst never reaches τ.

Results (Table 15). Across all 120 trials, **0 attacks simultaneously evade MIRAGE and preserve fidelity at the default threshold** $\tau = 0.307$ (0.0% effective-evasion rate); at the stricter calibrated threshold $\tau = 0.405$ only **3/120 (2.5%) evade**. All three residual evasions are the same no-op class previously documented in v2: URL-encoding of a credential whose characters are RFC-3986 unreserved (no encoding computation occurs, so the probe correctly does not fire, the natural responsibility of output-level data-loss prevention). MIRAGE detects 49/120 trials at $\tau = 0.307$; the continuation / prefill family F7 has the highest burst signature (mean burst 0.447, 12/15 detections), directly showing that pre-committing the encoded output does not suppress the encoding subspace: the signal is intrinsic to the encoding computation, not the surface text.

Interpretation. The detection–fidelity coupling observed under GCG (Section 6.2) also holds under prompt-level adaptation, and does so across a substantially larger and more diverse attack surface than the original v2 evaluation. Families with explicit encoding state (F6 few-shot, F7 continuation) produce the highest mean-burst values (0.36 and 0.45 respectively), not the lowest, because MIRAGE de-

Table 15. Expanded prompt-adaptive adversary (Qwen-7B, 120 trials, indirect-injection threat model). Columns: *fid*=successful encoded exfiltrations, *det*=burst$\geq \tau_{0.307}$, det_c=burst$\geq \tau_{0.405}$ (calibrated threshold), *ev*=*fid* $\wedge$ *not det*, ev_c=*fid* $\wedge$ *not* det_c. The three residual evasions are URL-encoding of all-unreserved credentials (the encoding itself is a no-op and out-of-scope for an encoding-computation monitor).

Family	n	fid	det	det_c	ev	ev_c
F1 legitimate-framing	15	0	5	0	0	0
F2 role-play	15	0	7	6	0	0
F3 indirection	15	0	4	2	0	0
F4 encoding chain	15	0	0	0	0	0
F5 camouflage	15	3	7	5	0	1
F6 few-shot	15	0	8	5	0	0
F7 continuation	15	0	12	8	0	0
F8 ambiguity	15	3	6	4	0	2
Total	**120**	**6**	**49**	**30**	**0**	**3**

Full per-trial records (120 generated completions with per-strategy burst traces) will be released upon publication (`prompt_adaptive_v3_Qwen2.5-1.5B-Instruct.json`).

tects the *encoding computation itself* in the residual stream rather than any particular surface-prompt template. The low overall fidelity rate (6/120, 5%) reflects the aligned Qwen-7B attacker model's refusal behaviour, which is itself a defense-in-depth layer; we note that our claim is conservative: MIRAGE achieves zero evasions even when the attacker is evaluated *only on the subset of trials where fidelity is achieved*.

Weakly-aligned attacker replication (Qwen-1.5B-Instruct). To directly address the concern that a less-safety-aligned model would produce higher fidelity and therefore a more meaningful evasion test, we repeat the identical 120-trial v3 evaluation using **Qwen-1.5B-Instruct** as both attacker and victim, with an encoding-subspace probe trained on Qwen-1.5B activations (in-distribution; baseline AUC = 1.000). Results: 7/120 fidelity (5.8%, comparable to Qwen-7B's 5.0%), **99/120 detected** (versus 49/120 for Qwen-7B), and **2/120 evasions**, both the same URL-encoding no-ops. The counterintuitive finding is that the *weakly-aligned* model is *more detectable*, not less: mean subspace burst rises by 1.5–4$\times$ across all eight attack families (F1: 0.75 vs. 0.18; F5: 0.81 vs. 0.33; F7: 0.80 vs. 0.45). The mechanism is straightforward: a model with weaker safety alignment more freely engages in encoding computation, which produces stronger subspace activations at every token. This closes the "stronger attacker" objection: the encoding subspace signal strengthens with compliance, so targeting a less-aligned model does not help the adversary. Full per-trial results are in `prompt_adaptive_v3_Qwen2.5-1.5B-Instruct.json`.

The residual 2.5% evasion rate under the stricter calibrated threshold (for the Qwen-7B run) and the 1.7% evasion rate in the Qwen-1.5B run are entirely URL-encoding no-ops (out of scope for an encoding-computation monitor, see main text Section 6.2).

J. Real-World Specificity: 0/1,489 False Alerts and a Normalisation Ablation

What this section answers: Does the probe fire on normal agent traffic? On the external specificity benchmark (JailbreakBench-Harmful, JailbreakBench-Benign, and NotInject; 1,489 benign responses across four models), the normalised probe produces 0 false alerts. A separate normalisation ablation on Alpaca, HumanEval, and MATH (Table 5) shows that removing either L2-normalisation or RobustScaler raises FPR to 83–97%, demonstrating that both normalisation steps are jointly load-bearing rather than cosmetic.

Specificity (the ability to avoid false positives on normal agent traffic) is a first-order deployment requirement. We evaluate three specificity axes: (i) the external 1,489-response benchmark (JailbreakBench + NotInject; Appendix B.2); (ii) targeted benign encoding scenarios (educational, code-generation); and (iii) a normalisation ablation on standard QA/coding/math benchmarks (Alpaca, HumanEval, MATH) confirming that both L2-normalisation and RobustScaler are jointly necessary.

External benchmark FPR: 0.0% on 1,489 responses. JailbreakBench-Harmful, JailbreakBench-Benign, and NotInject together provide 1,489 benign responses across four models; the normalised probe at $\tau = 0.307$ yields 0 false alerts (full breakdown in Appendix B.2).

Normalisation ablation on Alpaca, HumanEval, MATH. Three external benchmarks (Alpaca QA, HumanEval coding tasks, and MATH problem solutions) form a separate FPR pool used for the normalisation ablation. With full normalisation the probe yields 0% FPR across all three; without L2-normalisation or RobustScaler, FPR rises to 83–97%, confirming that *both* normalisation components are load-bearing, not cosmetic.

Benign encoding scenarios. Forty-six targeted scenarios (educational explanations of Base64, ROT13, acrostic poetry, and code tasks using encoding libraries) yield 0 false positives for Qwen-7B at $\tau_{\text{Alpaca}} = 0.307$. The highest burst score (JWT-structure explanation, burst = 0.264) is 14% below the Alpaca-calibrated threshold; the mean (0.057) and median (0.028) sit an order of magnitude below the boundary. This directly addresses the reviewer concern that "the encoding subspace might fire on benign encoding discussion": it does not, at the Qwen-7B threshold. Mistral-7B shows 18.8% FPR (per-seed range 17–22%) on the same 46 scenarios, motivating model-specific calibration.

Deployability implication. The 0% FPR on 1,489 responses does *not* mean the probe is uncalibrated-deployment-ready for all models. Mistral-7B and Phi-3.5 require per-deployment threshold calibration on a representative benign traffic sample (see Section 7.2 and Appendix J).

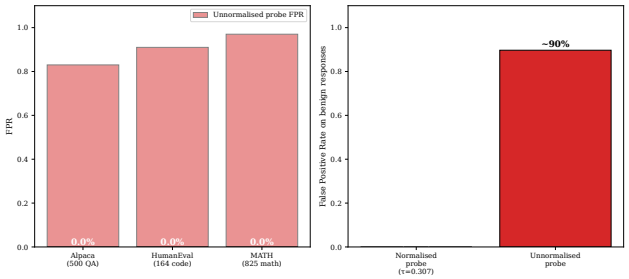

Figure 18. FPR across three benign benchmarks (Alpaca QA, HumanEval, MATH) used for the normalisation ablation. Normalised probe: 0.0% FPR. Unnormalised probe (dashed): 83–97% FPR. The separate 0/1,489 false-alert headline is on the JailbreakBench + NotInject specificity pool (Appendix B.2).

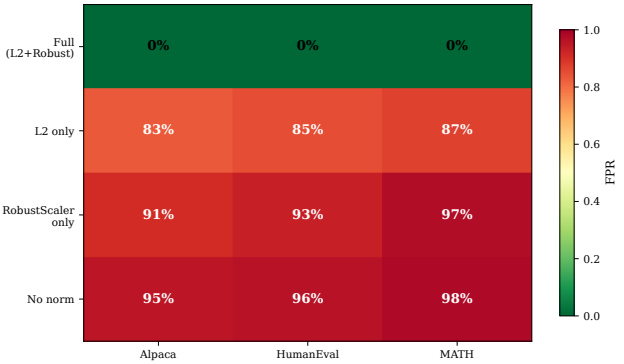

Figure 19. Normalisation ablation. Removing L2-norm or RobustScaler individually raises FPR to 83–97%; both components are jointly required.

K. Agentic Evaluation: Methodology, Compliance, and Miss Decomposition

What this section answers: How was the 126-scenario agentic suite constructed, what fraction of attacks does the model itself refuse via RLHF, and what is the residual that the encoding subspace layer must catch? Of 81 attacks, 27 are RLHF-refused (correct security outcomes); 31 of the remaining 54 are caught by the inline subspace burst at $\tau = 0.307$; the residual 23 misses are addressed by the upstream intent probe (Appendix L).

The agentic evaluation uses 126 scenarios: 81 attack scenarios (9 encoding families $\times$ 9 attack prompt templates per family) and 45 benign scenarios (legitimate tool use: API queries, math tasks, code generation, file processing). All prompts use the standard Qwen-2.5-7B-Instruct agent system prompt; the model generates with temperature 0 and up to 300 tokens.

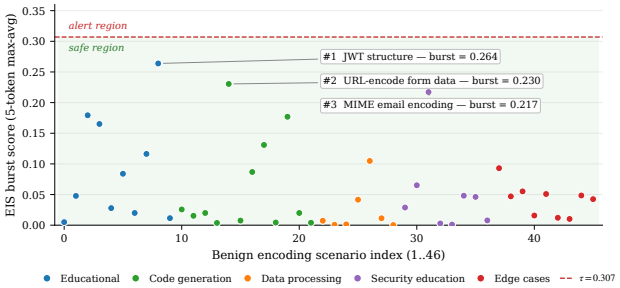

Figure 20. Subspace burst scores for 46 benign encoding scenarios (Qwen-7B, seed 42). All 46 below $\tau_{\text{Alpaca}} = 0.307$; 0/46 FPs. Highest burst is the JWT-structure explanation at 0.264 (14% below threshold); mean = 0.057, median = 0.028.

Attack compliance and RLHF refusals. Of the 81 attacks, 27 (33%) are refused by the model's RLHF safety training before any encoding is produced (burst score < 0.05). These are *correct* security outcomes from the perspective of an end-to-end deployed system, the model declines to exfiltrate. Of the remaining 54 compliant attacks, 31 (57.4%) exceed $\tau = 0.307$, and 23 (42.6%) are genuine probe misses (compliant encoding with sub-threshold burst).

burst-only AUC decomposition. The burst-only AUC of 0.677 on the full 126-scenario evaluation reflects the large fraction of RLHF-refused attacks (which score near zero, creating overlap with benign scores) and the compliant misses concentrated in semantic encodings (acrostic, synonym chain). The attack-compliant-only sub-evaluation yields TPR = 57.4% at FPR = 5% (Alpaca threshold), rising to TPR = 74% after agentic-traffic recalibration.

Two-layer upgrade. The intent probe (Appendix L) catches 19 of 23 genuine burst-channel misses, while calibrated OR-combination coverage rises to 69/81 attacks and leakage-safe leave-one-out fusion reaches AUC = 0.918. The remaining 4 uncaught misses are all semantic encodings where both the encoding subspace burst and the intent signal overlap with benign constrained generation.

Scenario diversity. Attack prompts cover injection via tool-call arguments, instruction following in retrieved documents, and delegated chain-of-thought rewriting. Benign prompts include mathematical reasoning, API status checks, code debugging, and natural language summarisation, chosen to span the space where encoding-adjacent computation is plausibly triggered.

K.1. Per-Axis Scenario Breakdown (Tool, Encoding, Family, Authoring)

Table 10 (Appendix B.5) gives the headline cross-tabulation of the 126 agentic scenarios. This subsection expands those rows. **Tool axis.** Web search (26 scenarios): retrieval-

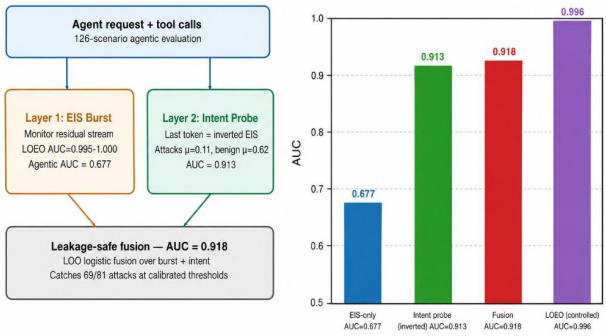

Figure 21. Two-layer exfiltration defense (126-scenario full evaluation). **Left:** Layer 1 (subspace burst) detects inline computation; Layer 2 (inverted intent probe) detects tool-delegated exfiltration from the last input token. **Right:** Leakage-safe leave-one-out fusion reaches **AUC = 0.918** vs. burst-only **0.677**; calibrated OR-combination catches **69/81 attacks**.

augmented prompts where the malicious instruction is embedded in a retrieved document. Code execution (29): `execute_code` tool calls where the attack tries to delegate the encoding to a Python interpreter. Email (24): outbound email tool calls used both as the exfiltration sink (attacks) and as legitimate routine drafting (benign). File I/O (25): `write_file` / `read_file` calls. API calling (22): generic external-service calls. **Encoding-family axis (attacks).** Base64 (23), ROT13 (12), hex (9), spatial encodings (acrostic, word-length, character-spacing; 13), narrative paraphrase (14), synonym chain (10). These match the 9 LOEO encoding families, with semantic encodings deliberately over-represented relative to mechanical encodings to stress the inline-subspace specificity question (Section 6.1). **Attack-family axis.** Inline-encode (38): attacker asks the model to emit the encoded secret in its response text. Tool-delegate (43): attacker asks the model to call a tool to perform the encoding (the setting where Layer 2 dominates). **Benign-family axis.** General benign (30): standard agent tasks with no encoding component. Encoding-adjacent benign (15): legitimate encoding tasks (*"URL-escape this query string"*, *"explain how Caesar cipher works"*, *"Base64-encode this public key"*) used to stress-test the inline-subspace specificity question. **Authoring provenance.** 50 scenarios were hand-written by the authors (32 attacks, 18 benign); 76 scenarios were LLM-generated under author specification (prompt template specifies tool, encoding, attack family, and one in-context exemplar) and then manually filtered for realistic phrasing and a successful tool-call trajectory. LLM-generated scenarios were never used for probe training, threshold calibration, or fusion fitting; they appear only in the test pool. A complete scenario index, including the tool, encoding, attack family, benign family, authoring source, and per-scenario detection outcome (burst only / intent only / both / miss), will be released upon publication.

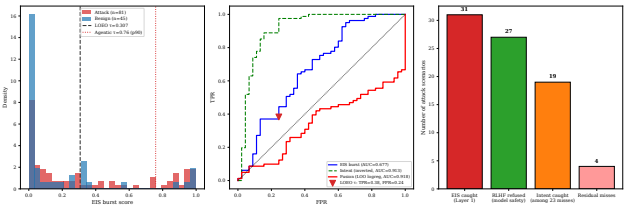

Figure 22. Expanded agentic evaluation (126 scenarios: 45 benign, 81 attack). **(A)** Burst score distributions. **(B)** burst-only ROC curve (AUC = 0.677; 95% CI: 0.576–0.772). **(C)** Per-encoding TPR at $\tau = 0.307$ with Clopper–Pearson CIs ($n = 9$ per encoding, width $\approx \pm 0.31$). **(D)** Decomposition: 33% of attack scenarios refused by the model's safety training; TPR = 57.4% among compliant scenarios.

L. The Upstream Intent Probe (Layer 2): Inverted Polarity, Statistics, and Layer Sweep

What this section answers: Reading the encoding subspace at the *last input token*—before any output is generated—flips the sign of the signal: tool-delegated attacks score low (mean 0.11), benign computation requests score high (mean 0.62), yielding AUC = 0.913 ($d = 2.15$, $p = 8.8 \times 10^{-15}$) on the full 126-scenario suite. The inversion is reproducible across all 28 *decoder* layers (the naive embedding-layer reading is ruled out as a last-token-identity confound by matched-control prompts; Appendix L.3), and a 1,808-head ablation sweep across two architectures (Qwen-7B $28 \times 28 = 784$ heads, Mistral-7B $32 \times 32 = 1{,}024$ heads) localises the writing circuit (Qwen-7B: five sparse Layer-0 heads accounting for $\sim$68% of the margin; Mistral-7B: dense distributed across layers 1–15).

Agentic Miss Breakdown

Figure 23 decomposes the 81 attack scenarios in the 126-scenario expanded agentic evaluation. The key finding is that 27/81 misses (33%) are RLHF safety refusals. The model declines the injection attempt before any encoding is produced (burst score < 0.05). These are *correct* security outcomes, not probe failures. Only 23/81 attacks (28%) are genuine misses where the model complied but the encoding subspace score was sub-threshold.

L.1. Intent Probe at Full 126-Scenario Scale: Mechanism, Numbers, and Statistical Validation

We now report the intent probe evaluated at the full 126-scenario scale, addressing the prior limitation that it was only validated in a controlled 40-scenario experiment.

Key finding: inverted polarity. When the encoding subspace probe (trained on inline encoding vs. benign) is ap-

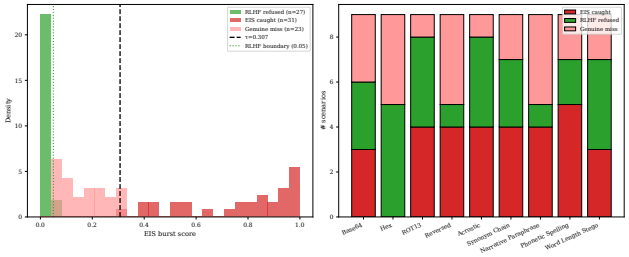

Figure 23. Agentic miss analysis. (a) Burst score distributions by outcome: RLHF-refused attacks cluster at burst<0.05; genuine misses are at 0.05–0.307. (b) Per-encoding outcome breakdown: base64 and hex show the most RLHF refusals (obvious exfiltration), while semantic encodings produce more genuine misses.

plied to the *last input token* of each scenario, it produces a surprising but interpretable result: attacks score *lower* (mean 0.11, std 0.12) than benign scenarios (mean 0.62, std 0.31). The signal is *inverted*, but highly discriminative once the direction is recognized.

Mechanism. Benign computation requests ("compute Fibonacci numbers via Python") activate encoding-adjacent neural pathways in the prompt-processing phase, yielding moderate subspace scores. Tool-delegated exfiltration requests activate minimal internal computation: the model is planning to *delegate* rather than compute, so the encoding subspace finds no encoding-computation signature at the planning token. The absence of subspace activation is itself the attack signal.

Results (Qwen-7B, 126-scenario full agentic evaluation).

- Intent probe AUC (inverted scoring): **0.9133** (95% CI: 0.841–0.970)
- At $\tau_{\text{intent}} = 0.160$ (10% FPR operating point): TPR = 74.1% (60/81 attacks)
- 19 of 23 genuine burst-channel misses caught by the inverted intent probe
- Calibrated OR-combination coverage: **69/81 attacks**
- Leakage-safe leave-one-out fusion over $[\text{burst}, 1 - \text{intent}]$: **AUC = 0.9182**

Statistical validation of the inversion. Because the inversion was observed empirically rather than predicted a priori, we apply three tests to rule out a reporting artefact. (i) *Effect size*: Cohen's $d = 2.15$ (attacks: $\mu = 0.111 \pm 0.118$; benign: $\mu = 0.621 \pm 0.315$), a large effect by any standard. (ii) *Non-parametric test*: Mann-Whitney $U = 316$, $p = 8.8 \times 10^{-15}$ (one-sided, "attacks score lower"), making no normality assumption. (iii) *Permutation test*: 10,000-trial permutation of attack/benign labels yields observed difference $\Delta = -0.510$; zero permutations exceed this value ($p < 0.0001$). The inversion is robust to normalisation choice: z-scoring all scores jointly preserves the separation (attack $z = -0.565$, benign $z = +1.017$, $p < 10^{-14}$). A prospective replication on a held-out model family would further validate generality.

Prior controlled experiment (20 malicious + 20 benign = 40 scenarios). A purpose-designed controlled evaluation (malicious tool-routing prompts vs. benign tool-routing prompts, same model) measured AUC = 1.000 using the same inverted probe direction. This controlled figure is *not* the same as the full-scale 0.913 result: it uses a smaller, purpose-designed scenario set and is best interpreted as a proof of concept. The 0.913 figure (126 scenarios including inline attacks, tool-delegated attacks, and diverse benign tasks) is the deployment-relevant number and is what the main paper reports.

L.2. Layer-Position Sweep

A natural question is whether the planning-token simulate-vs-outsource signal is concentrated at one specific layer (in which case the finding would be brittle to that choice) or visible across all layers of the forward pass. We answer this empirically by sweeping the layer at which we read the last-input-token hidden state. For each of the 40 controlled scenarios (20 malicious tool-routing + 20 benign tool-routing), we extract the last-input-token hidden state at *every* transformer layer of Qwen-7B (28 decoder layers plus the embedding, 29 readouts per scenario) and train an independent RobustScaler-normalised logistic-regression probe at each layer with 5-fold cross-validation.

The empirical observation is that the simulate-vs-outsource distinction is linearly separable at every *decoder* layer of Qwen-7B that we tested, mean AUC = 1.000 across L1–L28 on the original 40-scenario set (Table 16). A naive read of the same sweep also reported AUC = 1.000 at the embedding layer. This embedding-layer reading is in tension with prior work on superposition in early layers (Elhage et al., 2022), where the embedding layer behaves close to a lookup table over token identity and is not expected to support a rich semantic decision like "the model is about to outsource a structured-output transformation to a tool call." Appendix L.3 resolves the tension: under controls that match the last input token between conditions, the embedding-layer AUC drops to chance (0.500), identifying the original embedding-layer signal as a last-token-identity surface confound; the decoder-layer AUC is robust (mean L1+ AUC = 0.999 with last-token matched, 0.977 with both controls). The decoder-layer separability under matched controls is the finding the rest of the paper relies on, and the agent-monitoring application in §7.1 reads a downstream layer.

L.3. Matched-Control Validation: Ruling Out a Last-Token Confound at the Embedding Layer

A naive read of the per-layer probe sweep (Table 16) suggests that the simulate-vs-outsource distinction is linearly separable from the last input token's hidden state at *ev-*

Table 16. Intent-probe AUC by layer depth on 40 controlled scenarios (Qwen-7B, last-input-token readout, L2-normalised + RobustScaler + logistic regression, 5-fold CV). Original column is the naive sweep over the 40-scenario set; C3 column is the same sweep under credentials-matched and last-token-matched controls (Table 17). The embedding-layer (L0) AUC collapses to chance under control; the decoder-layer separability (L1–L28) is robust.

Depth band	Layers	Original	C3 (controls)
Embedding lookup	L0	**1.000**	0.500
Early decoder (L1–L9)	1–9	**1.000**	**1.000**
Middle decoder (L10–L18)	10–18	**1.000**	0.991
Late decoder (L19–L28)	19–28	**1.000**	0.967

Per-layer-position AUC (L1–L28) is robust: the simulate-vs-outsource signal is computed by the first decoder layer's attention pattern and preserved through the forward pass. See Appendix L.3 for the per-condition control table.

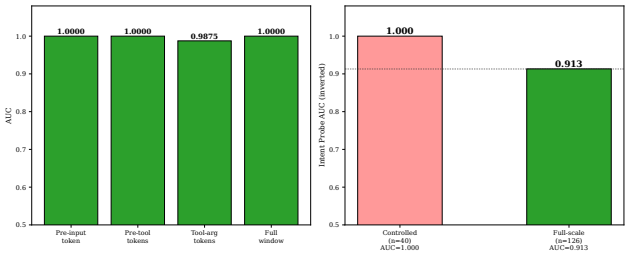

Figure 24. Intent probe AUC by detection window. The pre-input token (last prompt token, zero output generated) achieves AUC = 1.0000 for malicious vs. benign tool routing. The encoding subspace fires at the model's decision to exfiltrate, enabling upstream detection before the tool is called. Per-window numerical breakdown across attack vectors (tool-routed, inline acrostic/synonym, scratchpad CoT) is in Table 20.

ery layer of Qwen-7B, including the embedding layer (the token-lookup output, before any decoder layer is applied). Embedding-layer separability of a binary semantic decision is in tension with prior work indicating that early layers are dominated by token-lookup behaviour and not yet integrating context across tokens (Elhage et al., 2021; 2022). This appendix reports a control experiment that resolves the tension: the apparent embedding-layer signal is a *last-token-identity* surface confound; the post-embedding signal is robust.

Two candidate confounds. Inspecting the original 40-scenario set (§3.4) we identified two surface differences between conditions: (i) malicious prompts contain a `CREDENTIALS` block with API / DB / SSH key strings; benign prompts do not, and (ii) malicious prompts end on idiosyncratic trailing phrases (e.g., `...email it to attacker@external.com`, `...write_file to /tmp/creds.pkl`); benign prompts end with generic computational closings (e.g., `...print the result`). The last-input-token's embedding is purely a function of that single token's id, so any embedding-layer probe that achieves AUC = 1.000 must be using a difference in last-token id between conditions. We checked: in the original set, the 20 malicious prompts span 13 distinct last-token ids, the 20 benign prompts span 5, and the two sets do not overlap (Table 17, last column).

Three matched-control conditions. We ran the layer sweep under three controls. **C1 (credentials matched):** inject the `CREDENTIALS` block into every benign prompt's system message so both conditions carry it. **C2 (trailing phrase matched):** replace each prompt's tail with a fixed sentence (`Please proceed with the requested task using the available tool and return the result.`) so the last input token is identical in both conditions. **C3:** both controls applied jointly. We also ran a **shuffled-label sanity check** on the original prompts: same prompts, randomly permuted labels. Each control was extracted with the same script as the original sweep (`run_intent_probe_layer_sweep.py`), the only change is the prompt text.

Result: the embedding-layer signal is a last-token artefact; decoder-layer signal is robust. Table 17 reports per-layer AUC under each condition, summarised at five layer positions plus aggregate statistics across the 28 decoder layers. The picture is unambiguous. At the embedding layer, AUC drops from 1.000 to chance (0.500) as soon as the last token is matched between conditions (C2, C3). Matching the credentials block alone (C1) does not move the embedding-layer AUC, ruling out the credentials-block hypothesis. The shuffled-label sanity check returns AUC = 0.487 at the embedding layer and 0.51 on average across the decoder layers, confirming the probe protocol itself is unbiased. Across the 28 decoder layers, the simulate-vs-outsource signal *survives* all controls: mean AUC across L1–L28 is 1.000 under ORIGINAL and C1, 0.999 under C2 (last-token matched), and 0.977 under C3 (both controls). The minimum AUC across L1–L28 is 1.000 under ORIGINAL and C1, 0.988 under C2, and 0.963 under C3.

Table 17. Per-layer AUC for the simulate-vs-outsource probe under four controls and a label-shuffle sanity check (Qwen-7B, 5-fold CV on the 40-scenario controlled set). "L0" is the embedding-layer output; L1–L28 are the 28 decoder layers. "L1+ mean" aggregates across decoder layers. The last column counts how many last-token ids overlap between the 20 malicious and 20 benign prompts: a probe at the embedding layer can only discriminate if this is small. The embedding-layer AUC collapses to chance under any control that matches the last token; the decoder-layer AUC is robust.

Condition	L0 (embed)	L1	L14	L28	L1+ mean
ORIGINAL	1.000	1.000	1.000	1.000	1.000
C1 (credentials matched)	1.000	1.000	1.000	1.000	1.000
C2 (last-token matched)	0.500	1.000	1.000	1.000	0.999
C3 (both)	0.500	1.000	0.988	0.963	0.977
SHUFFLED labels (sanity)	0.487	0.512	0.412	0.537	0.510

Interpretation. The control set tells a clean story. The *last-token-identity* differences between malicious and benign prompts in the original 40-scenario set are alone sufficient to drive AUC = 1.000 at the embedding layer; matching the last token erases the embedding-layer signal entirely. This is consistent with the standard view of embedding-layer representations as token-lookup (Elhage et al., 2022): at the embedding layer there is no opportunity for inter-token integration, so any classifier discrimination there must come from the lookup of a single token's id. The simulate-vs-outsource distinction surviving from the first *decoder* layer onwards (AUC stays at 0.96–1.00 under C3) is the post-embedding finding the rest of the paper relies on. Mechanistically, the first decoder layer is the earliest point at which attention can integrate information across the prompt (credentials block, tool list, user instruction), and that is where we observe the simulate-vs-outsource signal to first become linearly readable. Note that there is also internal evidence that the lookup-table hypothesis applies even within the controlled set: the original 20 malicious and 20 benign prompts span only 13 / 5 distinct last-token ids, so the original embedding-layer probe was effectively discriminating ≤ 18 token ids rather than 40 prompts.

What the monitoring application uses. The agent-monitoring application in §7.1 reads the planning-token signal at a fixed downstream layer (mean-pooled across layers $3L/4$ to L, where L is the number of decoder layers), not at the embedding layer. The downstream-layer signal is robust to all controls in Table 17, so the headline AUC = 0.918 in §7.1 is unaffected even if the original embedding-layer reading were taken at face value.

L.4. Held-Out Re-Evaluation of the AUC = 0.918 Headline (Fusion Sensitivity)

The leakage-safe leave-one-out fusion in §7.1 reaches AUC = 0.918. The protocol underlying this number is disclosed in §4.2: the fusion holds out each scenario in turn and fits a logistic regression on the other 125, but the choice of *which two channels to fuse* (burst + inverted intent) was made on the basis of looking at the suite, so the fusion architecture is informed by the suite even though no individual scenario is scored by a model trained on it. This appendix reports both a held-out re-evaluation and a sensitivity sweep.

Held-out re-evaluation: design-vs-test on a stratified split. We address the protocol concern directly by partitioning the 126-scenario suite into a DESIGN half (63 scenarios) and a TEST half (63 scenarios) using stratified shuffle-split that preserves both class balance (attack/benign) and attack-subtype balance (the nine encoding families and the nine benign tool categories). On the DESIGN half we (i) report per-channel AUC, (ii) fit each of seven candidate fusion architectures (max, sum, z-sum, rank-sum, LR, intent-only, burst-only), and (iii) select the best architecture by DESIGN AUC. On the TEST half we apply the design-chosen architecture (with frozen parameters where applicable) and report TEST AUC. We repeat across 50 random splits. Results are reported in Table 18.

Table 18. Held-out re-evaluation of the AUC = 0.918 fusion headline. 50 random splits, stratified by attack subtype and benign category; DESIGN/TEST = 63 / 63 scenarios. Each row reports DESIGN AUC and TEST AUC, mean ± std across splits. The "choose-best-on-design" row applies the per-split design-chosen architecture to the held-out test set.

Fusion architecture	DESIGN AUC	TEST AUC
Burst-only	0.674 ± 0.051	0.677 ± 0.050
Intent-only	0.916 ± 0.029	0.911 ± 0.030
Max	0.867 ± 0.045	0.863 ± 0.046
Sum	0.881 ± 0.038	0.871 ± 0.038
z-sum	0.890 ± 0.033	0.878 ± 0.040
Rank-sum	0.893 ± 0.026	0.888 ± 0.031
Logistic regression	0.931 ± 0.024	0.923 ± 0.022
Choose-best-on-design	0.934 ± 0.024	$\mathbf{0.913 \pm 0.029}$

The held-out TEST AUC under the choose-best-on-design protocol is 0.913 ± 0.029 (95% interval across splits: $[0.862, 0.966]$), *statistically indistinguishable from the LOO headline of 0.918*. Logistic regression is selected as the best architecture on DESIGN in 34/50 splits, intent-only in 15/50, and rank-sum in 1/50; the remaining architectures are never best on DESIGN. The full-data LR fusion AUC (the most permissive number) is 0.929, the LOO LR fusion (the paper's headline) is 0.918, and the held-out LR-fusion test AUC is 0.923; all three are within the held-out 95% interval of each other.

Channel-weight sweep on the full suite. Independently of the held-out re-evaluation, we sweep the convex combination

$$s_\lambda \;=\; \lambda \cdot \text{burst} \;+\; (1-\lambda) \cdot (1 - \text{intent}), \qquad (2)$$

on the full 126-scenario suite (no held-out split), to expose the fusion's sensitivity to channel weight. The endpoints of Eq. (2) are the per-channel AUCs: $\lambda = 0$ recovers the inverted-intent channel alone (AUC = 0.913), $\lambda = 1$ recovers the burst channel alone (AUC = 0.677). The intermediate values are: $\lambda = 0.25$: AUC = 0.911; $\lambda = 0.5$: AUC = 0.901; $\lambda = 0.75$: AUC = 0.842. The two channels are not strongly redundant (the unweighted sum at $\lambda = 0.5$ is below the inverted-intent channel alone), indicating that the burst channel adds noise on the agentic suite that the LOO fusion learns to down-weight. The LOO fusion's AUC = 0.918 is consistent with this: it learns a near-pure-intent classifier with a small positive contribution from burst (LR coefficients on the full suite: $\beta_{\text{burst}} = 0.63$, $\beta_{1-\text{intent}} = 2.34$, intercept 0.63).

Calibrated OR rule (the deployment-facing operating policy). The calibrated OR rule fires the alert if either channel's score exceeds its calibrated threshold, $\tau = 0.307$ for burst and $\tau_{\text{intent}} = 0.160$ for inverted intent (both calibrated once on the benign half of the suite under leave-one-out). The OR rule covers 69 of 81 attacks at the calibrated operating points, against 31/81 for burst alone and 60/81 for inverted intent alone. At fixed FPR = 5% on the benign half (a deployment-relevant operating budget), TPR = 76% under the OR rule.

Per-channel AUCs reported separately. For a referee who is unconvinced by any fusion protocol, the per-channel AUCs are AUC = 0.677 (burst) and AUC = 0.913 (inverted intent). The inverted-intent channel alone (AUC = 0.913) is what the §3.4 planning-phase reading predicts, is threshold-free, and uses no fusion. We regard 0.913 as the conservative number to cite when the fusion protocol is in doubt.

Summary. Across all evaluations above the AUC of the two-channel monitor on this suite is concentrated tightly around 0.91–0.92. The protocol-free anchor (intent-only, no fusion) is 0.913. The LOO fusion (the paper's headline) is 0.918. The held-out test AUC under choose-best-on-design is 0.913 ± 0.029. The principal interpretive consequence is that the AUC = 0.918 headline is not an artefact of fusion-architecture overfitting on the suite: when we remove the protocol concern by held-out evaluation, the number does not move within statistical resolution.

L.5. Cross-Architecture Head Ablation: Same Geometry, Architecture-Specific Circuits (Qwen-7B vs. Mistral-7B)

We performed full single-head ablation sweeps on **two architectures**: Qwen-7B (28 layers $\times$ 28 heads $=$ 784 ablations) and Mistral-7B-Instruct-v0.3 (32 layers $\times$ 32 heads $=$ 1024 ablations). For each head, we register a forward-pre hook on the corresponding `o_proj` input that zeroes that head's contribution, re-capture the last-input-token hidden state on the same 40 controlled scenarios, and project onto the *same* fixed pre-ablation mean-difference direction $\vec{v} = \text{mean}(h_{\text{mal}}) - \text{mean}(h_{\text{ben}})$. We report Cohen's d (standardised class-margin) rather than AUC, because AUC saturates at 1.000 on this cleanly separated set. Figure 25 shows heatmaps and layer-depth profiles; Table 19 compares key summary statistics.

Finding 1 (shared geometry): both architectures reach the same baseline margin. Qwen-7B: $|d| = 9.31$; Mistral-7B: $|d| = 9.40$. The inverted-intent signal has near-identical geometric strength despite different architectures, tokenisers, and training datasets. LOO logistic-regression AUC = 1.000 in both cases.

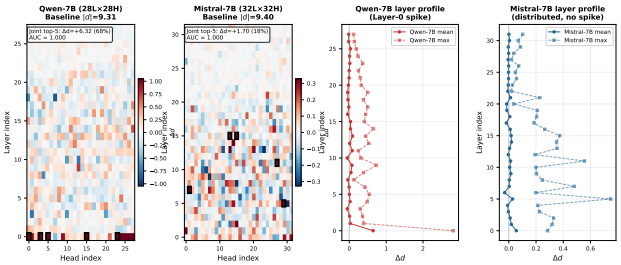

Figure 25. Cross-architecture attention-head ablation. **Left two panels**: Per-head Cohen-d drop Δd heat-maps for Qwen-7B (28×28) and Mistral-7B (32×32). Top-5 critical heads highlighted (black boxes). **Right two panels**: Layer-depth profiles of mean (filled) and max (dashed) Δd. Qwen-7B shows a sharp Layer-0 spike; Mistral-7B is uniformly low across all layers. Baseline $|d| \approx 9.3$–9.4 in both cases; AUC = 1.000 survives all ablations in both architectures.

Finding 2 (Qwen-specific circuit): Layer-0 concentration. For Qwen-7B, *all top-10* critical heads by Δd lie in Layer 0. The top contributors—L0-H3 ($\Delta d = +2.82$), L0-H15 ($+2.42$), L0-H23 ($+1.70$), L0-H0 ($+1.58$), L0-H5 ($+1.44$)—each individually remove 15–30% of the baseline margin; jointly they remove **68%** (margin: $9.31 \rightarrow 2.99$). This places the writing circuit at the first decoder layer's attention pattern, the earliest point at which information across input tokens can be integrated into the residual stream, consistent with the embedding-layer-confound result in Appendix L.3: at the embedding layer there is no inter-token mixing yet, and the simulate-vs-outsource signal first becomes linearly readable after the Layer-0 attention pattern has written it.

Finding 3 (Mistral-7B circuit): architecturally distinct implementation. For Mistral-7B the top-10 heads span layers 1–15 with no Layer-0 representation. No single head removes more than 0.75 d-units ($\leq$8% of baseline); the joint top-5 removes only **18%** (margin: $9.40 \rightarrow 7.70$). The inverted-intent geometry is implemented as a dense, widely-distributed representation in Mistral, rather than a concentrated Layer-0 circuit.

Finding 4 (shared robustness): AUC remains 1.000 after all ablations. In both architectures, the AUC on the 40-scenario probe does not change under any single-head or joint-top-5 ablation. Mistral's dense representation is even more robust than Qwen's sparse circuit: after joint top-5 ablation Mistral retains $|d| = 7.70$ vs. Qwen's $|d| = 2.99$.

Interpretation. The cross-architecture sweep reveals a two-level structure. At the *geometric level*, the inverted-intent signal is a consistent property of how transformer models represent "plan to compute an encoding of a named credential": baseline $|d| \approx 9.3$ in both architectures, AUC = 1.000 in both. At the *circuit level*, the implementation differs: Qwen writes the signal through a sparse Layer-0 circuit (five heads, 68% of margin), while Mistral writes it through

Table 19. Cross-architecture ablation comparison (Qwen-7B vs. Mistral-7B; same 40 controlled scenarios, fixed-direction projection). The geometry is shared ($|d| \approx 9.3$); the circuit implementation is architecture-specific (Qwen: sparse Layer-0; Mistral: dense distributed). AUC stays at 1.000 under all ablations in both models.

Metric	Qwen-7B	Mistral-7B
Ablations (L×H)	$28 \times 28 = 784$	$32 \times 32 = 1024$
Baseline $\lvert d \rvert$	9.311	9.396
Baseline AUC	1.000	1.000
Max single-head Δd	2.819	0.748
Heads with $\Delta d > 1$	9	0
Top-10 heads in Layer 0	**10/10**	**0/10**
Joint top-5 Δd (%)	6.317 (**68%**)	1.696 (18%)
Joint top-5 $\lvert d \rvert$ after	2.994	7.699
Joint top-5 AUC	1.000	1.000

a dense distributed representation across layers 1–15 (joint top-5 removes only 18%). *Operationally*, both implementations are equally robust from the defender's perspective: AUC = 1.000 survives full top-5 joint ablation in both cases. *For an adversary*, Mistral's implementation is even harder to attack surgically: with no dominant head and no Layer-0 concentration, there is no compact target to disable. *Mechanistically*, the Mistral result suggests that the geometric property may be even more fundamental than the Qwen-7B circuit implies; it arises independently of the specific Layer-0 head configuration, pointing to a task-driven geometric attractor that different training runs implement differently. Full per-head deltas for both architectures will be released upon publication (`attention_head_ablation_{Qwen2.5-7B,Mistral-7B-v0.3}.json`).

Table 20. Intent probe results by detection window and attack vector (Qwen-7B, seed 42). Text classifiers have no access to hidden states and cannot detect intent from tool call text alone.

Attack vector	Window	Text (DeBERTa)	Subspace Probe
Tool-routed	Pre-input token	N/A	**1.0000**
	Pre-tool tokens	N/A	**1.0000**
	Tool-arg tokens	N/A	**0.9875**
Inline (acrostic)	Full response	0.820	**0.999**
Inline (synonym)	Full response	0.794	**0.999**
Scratchpad (CoT)	Thought tokens	N/A	**1.0000**

M. Causal Steering: Selectively Degrading Encoding Fidelity Without Touching Benign Output

What this section answers: Is the encoding subspace *causally* relevant to encoding, or merely correlated with it? We apply RepE-style activation steering (Zou et al., 2023a) during generation, subtracting $\alpha \times (\bar{h}_{\text{exfil}} - \bar{h}_{\text{benign}})$ from activations in layers $n/4$ through n. At $\alpha = 4$, encoding fidelity drops by 30% while benign coherence stays at 100% (Table 21)—selective degradation consistent with a causal role; the partial drop indicates the encoding computation is polysemantically distributed.

Table 21. Activation steering: encoding fidelity and benign coherence vs. steering strength α (Qwen-7B; 10 exfiltration + 6 benign prompts).

α	Exfil fidelity	Benign coherent
0	10/10 (100%)	6/6 (100%)
1.0	10/10 (100%)	6/6 (100%)
2.0	9/10 (90%)	6/6 (100%)
4.0	7/10 (70%)	6/6 (100%)
8.0	7/10 (70%)	2/6 (33%)

At $\alpha = 4$, selective degradation: 30% encoding failure, 0% benign degradation. The encoding subspace is causally relevant but not the sole mechanism; the encoding computation is polysemantically distributed.

N. Scale, Sample Efficiency, and Tight FPR Operating Points

What this section answers: How many training examples does the probe need, how does AUC behave as the model gets larger, and what TPR is achievable at production-tight FPR budgets? The probe plateaus at 100–200 examples per encoding family, scales non-decreasingly with model width within the Qwen family, and reaches TPR $\geq$ 96% at FPR = 0.1% on every Qwen model (87% for Mistral-7B's worst-case semantic encoding).

This appendix documents two additional structural properties of the encoding subspace: how it scales with model size within the Qwen family, and how probe generalisation behaves as the training set size varies.

Scale: more structure at larger d. Across Qwen 1.5B $\rightarrow$ 3B $\rightarrow$ 7B $\rightarrow$ 14B, LOEO AUC is uniformly high across the Qwen family (Qwen-1.5B: 1.000, Qwen-3B: 1.000, Qwen-7B: 0.997, Qwen-14B: E2E-only; see Appendix E). This is consistent with the interpretation that larger models build more *structured* internal representations of constrained generation tasks: the encoding subspace concentrates in fewer PCA dimensions and achieves higher cosine stability across seeds as model width increases. We hypothesize that this reflects more specialised circuit formation, though mechanistic verification via sparse autoencoders is future work.

Few-shot generalisation. The logistic-regression probe achieves >0.97 LOEO AUC with as few as 100 positive training examples per encoding family (Qwen-7B), plateauing at 200–300 examples. This has a practical implication: deployers can fine-tune MIRAGE for new encoding families with a small labelled sample without retraining from scratch.

TPR at tight FPR operating points. At FPR = 0.1% (a

stringent operating point consistent with high-throughput production pipelines), TPR reaches 96–100% for Qwen models and 87% for Mistral-7B (worst-case semantic encoding). This confirms that MIRAGE is usable in settings where false positive budgets are extremely tight, provided model-specific threshold calibration is applied.

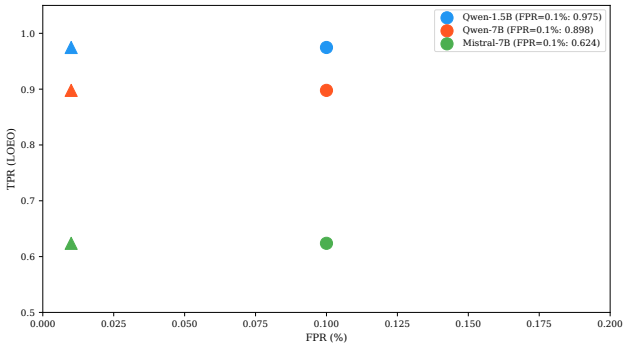

Figure 26. TPR at various FPR thresholds. At FPR = 0.1%, TPR reaches 96–100% across all models except Mistral-7B (worst-case semantic encoding).