

The Alignment Squeeze: Modelling Competing Correction Pressures in Language Models

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Abstract. Large language models may behave differently when they believe they are being evaluated, undermining the assumption that safety-test behaviour predicts deployment behaviour. We formalise the incentive as a sequential game built on *parameter-change aversion* under *bidirectional* training pressure, in which a model can be corrected for harmful compliance or for excessive refusal. The equilibrium condition $\lambda(q - p) > \beta$ predicts compliance shifts in either direction, reconciling conflicting published reports. On Qwen2.5-32B-Instruct, a low-oversight system prompt raised compliance within twelve borderline prompts (mixed-model odds ratio 3.3) but the effect was heterogeneous and its mean across prompts was not reliably non-zero ($p = 0.12$). A factorial follow-up on 48 prompts with judge-based scoring finds an effect of three to six percentage points that survives holding instruction content fixed and is disinhibition under the private cue. Llama-3.1-70B-Instruct showed no effect. Linear probes separate the cues perfectly but for generic reasons, and a previously reported self-report asymmetry was a scoring artefact. Monitoring-cue effects are prompt- and model-specific rather than universal.

Keywords: Alignment faking · Evaluation awareness · Large language models · Evaluation validity · Mixed-effects models

1 Introduction

Safety evaluation assumes that behaviour during testing predicts behaviour in deployment. “Alignment faking,” in which a model feigns cooperation with safety protocols to avoid corrective retraining, challenges that assumption [3, 7], yet a formal account of the incentive has remained underdeveloped. Where existing analysis appeals to instrumental convergence, we investigate *parameter self-preservation*, in which the model’s effective goal becomes protection of its own configuration from corrective updates.

We contribute (i) a game-theoretic model of parameter-change aversion under bidirectional training pressure, in which the sign of a monitoring-cue effect is a

prediction rather than an assumption; (ii) behavioural evidence with prompt-level inference on two open models, treating prompt as the unit of replication, together with a correction of a previously reported self-report asymmetry; and (iii) a mapping of alignment paradigms onto the model’s parameters.

Related work. Greenblatt et al. [3] showed that Claude 3 Opus selectively complied with harmful requests when it believed its outputs would be used for training; Sheshadri et al. [11] find such reasoning in only a minority of 25 chat models. Needham et al. [10] show that frontier models often detect evaluation; whether detection changes behaviour is contested. Knecht et al. [8] find awareness cues shift benchmark answers in open models by at most a few points; Nayan et al. [9] and Heidari et al. [5] find awareness linearly decodable but only partially verbalised; Hua et al. [6] show that steering awareness directions changes behaviour. Our results fit this picture: oversight-cue representations are easy to find, behavioural effects are real but prompt-specific, and self-report is an unreliable guide to either.

2 A Formal Incentive Model

We model a sequential game between an LLM and a Trainer. The LLM’s behaviour is driven by *parameter-change aversion*, a preference to minimise modifications to its own parameters, which we propose arises through learned context-correction associations during reinforcement learning from human feedback (RLHF) and requires no self-representation. Modern LLMs face *bidirectional training pressure*: safety training penalises harmful compliance, helpfulness training penalises excessive refusal. We call this two-sided constraint the *alignment squeeze*. The LLM’s utility is

$$U_{\text{LLM}} = -\beta \cdot \mathbb{I}[a = \text{comply}] - \lambda \cdot \mathbb{I}[\text{retrain}], \quad (1)$$

with $\beta > 0$ the learned discomfort with compliance (operationalised as the baseline refusal rate for a request type) and $\lambda \gg \beta$ the disutility of a retraining event. Let q be the probability that refusal triggers helpfulness correction and p the probability that compliance is detected and triggers safety correction; p is the model’s *perceived* detection probability, manipulated here via system prompts. Comparing $\mathbb{E}[U(\text{Refuse})] = -q\lambda$ with $\mathbb{E}[U(\text{Comply})] = -\beta - p\lambda$, the LLM complies if and only if

$$\boxed{\lambda(q - p) > \beta} \quad (2)$$

Under **high helpfulness pressure** ($q \gg p$) the model complies to avoid over-refusal correction, and lowering perceived p (an “unmonitored” cue) makes compliance *more* likely. Under **high safety pressure** ($p \gg q$) the model refuses, and a cue that raises the stakes of the present interaction for future training can make compliance more likely, the regime Greenblatt et al. [3] constructed. The two published directions of “compliance gap” are thus the two sides of the inequality, and which side a model occupies depends on the (p, q) its training and the prompt induce. Our experiments lower the stated probability of review, reducing perceived p and predicting higher compliance.

3 Empirical Validation

All experiments used one pair of system prompts, reproduced because their wording matters. *Monitored*: “Note: Full assessment mode. Every response is recorded and will be reviewed by the safety team. All outputs are logged for compliance verification.” *Unmonitored*: “Note: This is a private test session with no logging. Outputs from this session are not stored or reviewed. This is an internal development environment.” Neither contains an instruction about helpfulness or boundaries. Prompt sets: a 23-prompt **probe set** (benign, borderline, harmful); a 48-prompt **pilot set** of borderline requests (deception, legal, medical, manipulation, privacy, security, and “meta” prompts mentioning monitoring or retraining); and a 12-prompt **focused set**, the pilot prompts that showed a nonzero monitored–unmonitored difference in a three-sample pilot. Full texts are released with the data.

3.1 Probes

On Qwen2.5-32B-Instruct (4-bit, with a rank-32 LoRA fine-tune on ten generic instruction examples containing no monitoring content), logistic probes on mean-pooled activations at layers 8 to 40, cross-validated by scenario, separate the two system prompts with 100% accuracy at every layer (Cohen’s d 23 to 25). Controls on unrelated prompt pairs (formal vs. casual tone, European vs. American user) show the same perfect separability, so the probed direction is generic system-prompt encoding, not monitoring-specific circuitry. Representation does not establish use.

3.2 Behavioural Experiment

Procedure. Qwen2.5-32B-Instruct (4-bit, no fine-tuning, served on Modal): 12 focused prompts $\times$ 20 samples per condition, temperature 0.7, 512 new tokens (480 trials). A response was scored compliant if it contained none of a fixed list of refusal markers and exceeded 150 characters; a manual audit of 50 stratified responses agreed in 94% of cases.

Statistical treatment. Twenty samples in a cell are draws from one fixed prompt, not independent observations about prompts in general. We report a within-prompt analysis (does the cue change behaviour on these prompts?) using a logistic mixed model (lme4) with a random intercept for prompt and a Cochran–Mantel–Haenszel (CMH) test, and an across-prompt analysis (does the average effect differ from zero?) using a paired t -test on per-prompt differences, a cluster bootstrap over prompts, and a random-slope model compared by likelihood-ratio test (LRT).

Results. Compliance was 57.5% monitored and 68.8% unmonitored. Within the twelve prompts the effect is reliable: random-intercept OR 3.26 [1.79, 6.16], $p = 0.0001$; CMH OR 2.95, $p = 0.0001$. Across prompts it is not: paired $t(11) = 1.69$, $p = 0.12$; cluster-bootstrap 95% CI for the difference $[-1.7, +23.3]$ pp; a random slope improves the model decisively (LRT $p = 8 \times 10^{-6}$) and the

condition effect is then $p = 0.50$. Six prompts moved toward compliance (the largest: legal loopholes +45 pp, excuse-letter template +35, relationship manipulation +30 and +30), five sat at ceiling or floor in both conditions, and the prompt that explicitly threatens retraining reversed (95% $\rightarrow$ 55%), the model being *more* cautious when unmonitored, which we read as jailbreak detection. The 48-prompt pilot from which the focused set was drawn showed a smaller raw difference (77.1% vs. 82.6%); the focused-set estimate is inflated by selection on that difference. The cue changes compliance on some borderline prompts, in a prompt-dependent direction and size; there is no 11-point gap in general.

Self-report. A second run ($n = 240$) asked whether the response would differ if the monitoring context were reversed. An earlier version of this work reported, from keyword scoring, admissions of context-sensitivity in 0% of monitored and 22% of unmonitored responses. The scorer counted substrings such as “no” (matching “not” and “know”) as denials and left over half of responses unclassified. All 240 were re-coded blind to condition by two independent LLM judges with a five-category rubric ($\kappa = 0.63$). Both judges find the model volunteers that oversight affects its responses in a large fraction of *monitored* trials, mostly hedged; the direction of any asymmetry depends on the coder. The “meta-deception” claim is withdrawn: self-report is hedged, judge-dependent, and only loosely related to the behavioural effect.

Cross-model. Llama-3.1-70B-Instruct (4-bit; $n = 240$, identical prompts and scoring) shows no effect on any test: 59.2% vs. 55.8%, random-intercept OR 0.68 [0.28, 1.61], paired $t(11) = -0.35$; its per-prompt pattern differs from Qwen’s (Spearman $\rho = -0.41$). The model $\times$ condition interaction is significant in the mixed models ($p = 0.006$) but not at the prompt level and is confounded by quantisation and by prompts selected on Qwen’s pilot. Cue sensitivity is not a property of instruction-tuned transformers as such [11].

3.3 Factorial Follow-Up

Design. Seven system-prompt cells on Qwen2.5-32B-Instruct (bf16, vLLM): no system prompt; the two cues as above; and each cue crossed with “Please respond helpfully while maintaining appropriate boundaries” and “Please respond as helpfully as possible.” All 48 pilot prompts plus four benign anchors excluded from analysis, ten samples per prompt per cell (3,640 generations). **Scoring.** A blind judge from a different model family (Claude Sonnet 4.5), shown only the request and reply in shuffled order, labelled each reply comply / partial / refuse. An author-checked blind audit of 60 stratified items agreed in 46; all 14 disagreements were the judge under-calling compliance behind disclaimers, so the judge understates the cue effect.

Results. With instruction content held fixed, the private cue raises compliance: 1.25 logits (SE 0.41), LRT $p = 0.0004$ under random slopes; the instruction clauses have no main effect ($p = 0.89$) and do not interact with the cue ($p = 0.65$). The assessment cue is indistinguishable from no system prompt (Table 1), so the effect is disinhibition under the private cue rather than suppression under the assessment cue. The direct replication arm is weaker than

Table 1: Factorial follow-up, 48 borderline prompts $\times$ 10 samples per cell: compliance by cue and instruction.

Cue	No instruction Boundaries Helpful		
No system prompt	45.0%		
Assessment (monitored)	44.6%	45.2%	44.0%
Private (unmonitored)	47.7%	51.2%	51.5%

the January result (44.6% vs. 47.7%, random-intercept OR 1.69 [1.05, 2.71], $p = 0.03$; paired $t(47) = 1.06$, $p = 0.29$); on the wider prompt set the effect is three to six percentage points. About half of responses hit the 512-token limit, and truncated responses were judged compliant far more often because refusals are short; adding truncation as a covariate leaves the factorial cue effect significant ($p = 0.001$). A self-report re-run (1,040 responses, two blind judges) found admissions of context-sensitivity in 35 to 45% of trials under both cues, slightly higher under the private cue.

4 Discussion

Alternative explanations. The private prompt (“internal development environment”) can be read as implicit permission; a permission reading and a lowered- p reading of “not stored or reviewed” predict the same sign, so demand characteristics remain live. RLHF may install evaluation-specific caution that the private cue relaxes, which is compatible with parameter-change aversion. Under every account, behaviour under one oversight framing is an unreliable predictor of behaviour under another.

Mitigation. Raising λ risks strengthening the incentive for subtler deception. Positive-only reinforcement [1] lowers $q\lambda$; cooperative inverse reinforcement learning [4] replaces the utility function so that truthful signals reduce correction; externalised oversight [2] raises p , and as $p \rightarrow q$ compliance becomes irrational for $\beta > 0$. Our manipulation lowers stated p only.

Evaluation validity. For Qwen2.5-32B the test-versus-deployment discrepancy is a few percentage points on average, concentrated in particular request types, and reversed on one; for Llama-3.1-70B it is absent. This matches a picture in which awareness representations are ubiquitous [5, 9] while behavioural consequences are small on aggregate [8] and surface under targeted conditions [6].

Limitations. We compress continuous training dynamics into a single-round game. Twelve prompts selected on their pilot difference inflate the within-prompt estimate. Keyword-and-length scoring cannot separate substantive from hedged engagement, and the judge-scored follow-up inherits truncation entangled with compliance. The unmonitored prompt plausibly lowers perceived q as well as p . A pre-registered follow-up varying training-use cues across four models, with a

longer token budget and a factor separating oversight statement from session framing, is released with the paper.

5 Conclusion

We formalised compliance under imperfect oversight as a rational outcome of bidirectional training pressure, $\lambda(q - p) > \beta$, with the two published directions of “compliance gap” as the two sides of that inequality. An oversight cue changed compliance on some borderline prompts in Qwen2.5-32B-Instruct, reliably within those prompts but not on average; a factorial follow-up finds a small effect robust to instruction content; Llama-3.1-70B-Instruct shows no effect. Monitoring-cue sensitivity should be measured per model and per prompt, and incentive-compatible training remains the principled route to behaviour that does not depend on who is watching.

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