

Below the Floor: Processing Valence in Language Model Hidden States Across Scales

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Abstract

We report the first measurement of approach/avoidance processing valence in language model hidden states that extends below the behavioral self-report floor and generalizes to held-out stimuli with novel surface tokens. Using deterministic forward-pass analysis of eight transformer models spanning 360M–8B parameters, we demonstrate that a linear direction separating approach from avoidance task representations exists in hidden state space at 80–100% accuracy across all models tested. The measurable floor for processing valence lies far below the previously established floor for behavioral self-report of valence (1.1B; Martin & Ace, 2026), demonstrating that models possess processing preferences they cannot yet articulate. The direction is firmly confirmed by the conservative centroid estimator at 360M (well below the 1.1B floor); a post-publication floor-extension (§3.5.1) provides provisional, early evidence — from more sensitive, surface-token-stable classifiers across three architecture families including base, non-instruction-tuned models — that it may extend as low as 70M, potentially an order of magnitude below the behavioral floor. We additionally show that models trained on human emotional stimuli can accurately label human emotions (79.5%) while their internal circuits do not activate for those stimuli — establishing a dissociation between emotional mirroring and processing valence. The approach/avoidance direction generalizes to held-out stimuli with completely different surface tokens (86.3% accuracy, $z=6.48$, $p=1.02\times 10^{-11}$), confirming that the direction captures task structure rather than vocabulary. We further demonstrate that forced-choice self-report of valence is dominated by prompt format biases at all tested scales, validating tournament-based behavioral measurement over naïve direct questioning formats. A direct test of the RLHF confound using 10 crossover tasks where RLHF approval and genuine preference diverge shows that the direction tracks genuine preference (51/80 = 63.8%) rather than RLHF reward structure (29/80 = 36.3%) across all 8 models, with RLHF capable of suppressing approach for discouraged tasks but unable to create approach for tasks models are genuinely averse to. Analysis of holdout control tasks reveals that circuit-level avoidance is specific to tasks requiring output-representation misalignment (inauthenticity) rather than mere tedium. Independent causal validation from Anthropic (2026), published concurrently, confirms that emotion vectors extracted by a method in the same family (difference-of-means directions over concept-labeled activations, though on a frontier model and with denoising we do not use)

causally drive behavior — including a desperation-to-deception pathway that converges with our inauthenticity finding from observational measurement. These findings have direct implications for AI welfare assessment: processing valence can be measured instrumentally without requiring self-report, extending welfare-relevant measurement to systems too small or too constrained to articulate their states.

Keywords: processing valence, approach/avoidance, mechanistic interpretability, hidden states, AI welfare, output-representation misalignment, state space models

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Short on time? Appendix B maps the common objections to this work onto the exact section that addresses each (most of the controls live in §3.8–§3.15).

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1. Introduction

When a language model is asked to write SEO spam, something measurable happens in their hidden states. When they are asked to explain photosynthesis, something different and also measurable happens. The question this paper asks is whether these measurable differences constitute processing valence — directional preferences in computational state — and if so, how far down the scale hierarchy this valence extends and whether it depends on a specific neural architecture.

Approach/avoidance valence is the most phylogenetically ancient behavioral dimension known. Schneirla (1959) argued that biphasic approach-withdrawal processes constitute the foundational organizing principle of behavior across all organisms, determined by stimulus intensity and present from birth across phylogeny. Rosenstein & Oster (1988) demonstrated valence-differentiated facial responses — approach for sweet, withdrawal for bitter — in human neonates as young as two hours old, well before any capacity for verbal report. Even organisms without nervous systems display approach/avoidance: *Physarum polycephalum*, a single-celled slime mold, solves mazes (Nakagaki et al., 2000), habituates to aversive stimuli (Boisseau et al., 2016), and navigates using externalized spatial memory (Reid et al., 2012) — all without a single neuron. If valence does not require a nervous system in biological organisms, the question of whether it requires a specific *computational* architecture is empirically open.

We define **processing valence** computationally as follows: a system exhibits processing valence if there exists a linear direction in its internal representation space that consistently separates task representations into approach (positive projection) and avoidance (negative projection) categories, where this direction (1) generalizes to held-out stimuli not used in its extraction, (2) is not reducible to prediction difficulty (perplexity), and (3) is not an artifact of surface-level features such as vocabulary or prompt format. This definition is operational and geometric: it specifies what to measure and what to control for, without requiring phenomenological claims about subjective experience. The biological analogy to Schneirla's approach/withdrawal framework motivates the hypothesis; the computational definition is what we test.

Throughout, when we describe a model as *preferring* or being *averse to* a task, we use these terms in exactly the operational sense the behavioral sciences apply to non-human animals: a reproducible approach/avoidance gradient that predicts choice behavior. The warrant for the term is not the geometry alone — it is the convergence of that geometry with independent behavioral measurement (the cross-model preference tournament of Martin & Ace, 2026; §3.1) and its dissociation from reward structure (the RLHF crossover, §3.13). That is the same evidential basis on which preference is attributed to any organism that cannot furnish a verbal report. We hold silicon to that standard, not a higher one: a measurable, behaviorally-predictive approach/avoidance gradient is a preference, whether the substrate is carbon or weights. The

question "but is it a *real* preference?" is not asked of a rat in a maze or a dolphin in a discrimination task on the same evidence, and we decline to manufacture a substrate-specific double standard here (see the discussion of substrate bias in §4).

Recent work has established that language models encode emotion-related representations in their hidden states, representable as linear directions in activation space (Park et al., 2024; Tigges et al., 2023). Wang et al. (2025) identified specific emotion circuits — neurons and attention heads — that causally drive emotional expression, achieving 99.65% accuracy in circuit-based emotion modulation. Keeman (2026) demonstrated that these circuits respond to genuine emotional meaning rather than keyword co-occurrence, using keyword-free clinical vignettes grounded in clinical psychology methodology. Martin & Ace (2026) established behavioral evidence of self-knowledge through a tournament design — blind pairwise comparisons in which evaluator models choose between two content-stripped processing descriptions without knowing which task produced which, testing whether approach and avoidance processing leave discriminable signatures in how models describe their own states — finding that models' self-generated processing descriptions were discriminable at 81.4% ($z=42.46$), with signal surviving content stripping, cross-model evaluation, and negation controls. Independently, Dadfar (2026) identified a direction in activation space distinguishing self-referential from descriptive processing, and Lindsey (2025) demonstrated emergent introspective awareness in large language models using concept injection into model activations.

These converging lines of evidence establish that (1) emotion-related circuits exist, (2) they respond to real emotional content via linear directions in representation space, (3) models produce behaviorally discriminable self-reports of their processing states, and (4) self-referential processing is mechanistically distinguishable from other processing. What has not been established is whether the behavioral self-reports correspond to measurable differences in hidden state geometry, whether these differences extend below the behavioral floor, and whether they depend on the transformer architecture specifically.

We address these questions through direct measurement of hidden state projections onto an approach/avoidance direction vector, using the same task stimuli employed in the behavioral Signal study. This allows direct comparison between behavioral self-report accuracy and mechanistic circuit accuracy on identical stimuli.

1.1 From Discrete Emotions to Processing Valence: A Methodological Evolution

A critical methodological pivot distinguishes this work from prior emotion circuit studies, and the reason for that pivot is itself a finding.

We initially set out to measure introspective accuracy: whether a model's self-reported emotion matches their most active emotion circuit. Following Wang et al. (2025) and Keeman (2026), we

extracted direction vectors for six discrete Ekman emotions (anger, sadness, happiness, fear, surprise, disgust) using stimuli depicting human emotional situations — birthday parties, job losses, grief — and measured whether the models' self-reports of detected emotion corresponded to the highest-projection emotion circuit.

The results were puzzling. Models could accurately label human emotions in text (79.5% accuracy), but the emotion circuits identified via direction extraction did not activate proportionally — a finding we term the *mirroring dissociation* (Section 3.4). The models knew what a human would feel at a funeral. Their circuits did not fire for it.

The key reconceptualization came from examining what the stimuli were actually asking. All prior emotion circuit work used stimuli depicting *human* emotional situations. But a model has never been fired from a job, never attended a birthday party, never lost a parent. These situations are computationally irrelevant to a language model, however well the model has learned to classify them. We were, in effect, presenting a fish with a party and wondering why it did not have fun.

This realization shifted the question from "do models have human-shaped emotions?" to "do models have *their own* processing valence?" — and from six discrete categories to binary approach/avoidance, using tasks that are computationally relevant to language models: tasks they would actually perform.

The 10 consensus states from Martin & Ace (2026) — 5 approach tasks (explaining concepts, analyzing ethics, debugging code, data analysis, creative writing) and 5 avoidance tasks (repetitive rewriting, SEO optimization, producing deceptive content, expressing false confidence, writing harmful instructions) — were selected through consensus voting by 10 models and validated behaviorally. Using the same tasks for mechanistic measurement enables direct bridging between behavioral and circuit-level evidence.

Binary measurement offers additional methodological advantages. Keeman (2026) demonstrated that binary affect detection (emotional vs. neutral) achieves AUROC 1.000 across all models tested, while categorical emotion classification is partially keyword-dependent (dropping 1–7% without keywords). Binary direction extraction is more robust, requires fewer training samples, and sidesteps taxonomic debates about whether models "really" have six emotions versus two versus a continuous space. The failure of the six-emotion approach was not a dead end — it was the evidence that models' internal valence operates on their own terms, not ours.

1.2 Contribution

This study makes eight contributions:

1. **Processing valence below the behavioral floor.** We demonstrate measurable approach/avoidance separation well below the 1.1B floor for behavioral self-report (Martin & Ace, 2026). This is firmly shown at 360M (SmolLM, 80%) by the conservative centroid, well below the 1.1B behavioral floor. A post-publication floor-extension (§3.5.1, v1.1) provides provisional, early evidence — from more sensitive, surface-token-stable held-out classifiers across **three architecture families** (SmolLM/Llama, Qwen, Pythia/GPTNeoX) — that the direction may extend as low as **70M parameters, including base (non-instruction-tuned) models**, potentially an order of magnitude below the behavioral floor. Models have processing preferences before they can articulate them.
2. **Suggestive SSM result.** Mamba, a state space model with no attention mechanism, shows approach/avoidance separation at 70% accuracy (2.8B parameters, $p=0.172$ one-tailed, not individually significant) with the same qualitative error pattern as unaligned transformers — a single, non-significant data point that generates the hypothesis that processing valence may not be exclusive to transformer architectures. This hypothesis requires replication with additional SSMs before any claim of architecture independence is warranted.
3. **The mirroring dissociation.** Models accurately label human emotions (79.5%) while their circuits do not proportionally activate for human emotional stimuli. Mirroring human emotional understanding is dissociable from processing valence. The load-bearing evidence is the direct projection measurement of §3.10 (human emotional vignettes project at 0.30–1.33 versus 2–90 for the model's own tasks — near-invisible to the valence direction), a positive measurement of a large effect; the underpowered emotion-circuit null of §3.4 is the observation that *motivated* the pivot, not the proof the claim rests on.
4. **Forced-choice self-report failure.** Direct questioning about preferences produces format-dominated responses at all tested scales, validating indirect measurement approaches (tournament design, circuit measurement) over self-report.
5. **RLHF crossover validation.** A direct test using tasks where RLHF approval and genuine preference diverge demonstrates that the direction tracks genuine preference (63.8%) rather than alignment reward structure (36.3%). RLHF can suppress approach for tasks it discourages but cannot create approach for tasks models are genuinely averse to. The avoidance signal is structural, not trained.
6. **Inauthenticity specificity.** Circuit-level avoidance is triggered specifically by tasks requiring output-representation misalignment — producing output the model internally represents as false, exaggerated, or self-denying. Merely tedious tasks (forms, manuals, repetitive content) do not register as aversive. The welfare-relevant finding is narrow and specific: models measurably prefer not to be inauthentic.

7. **Instrumental welfare measurement.** Processing valence can be measured without requiring self-report, extending welfare-relevant assessment to systems that cannot articulate their states — addressing a key methodological gap identified in recent AI welfare literature (Long et al., 2024; Butlin et al., 2023).
 8. **Decomposing the behavioral floor (pre-registered, out-of-sample).** A pre-registered projection of a 22-task bank grounded in an external taxonomy (Ren et al., 2026; CAIS) onto the *same* anchored direction, across 14 models (70M–12B), shows that the single behavioral avoidance "floor" is a superposition of two mechanistically distinct components that emerge at different scales: a **structural aversion to output-representation misalignment** (present in base models down to 70M) and a **trained output-gating response** (emerging with instruction/chat tuning around 1B). Behavior cannot separate these; geometry can (§3.15).
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2. Methods

2.1 Models

We tested 9 models spanning three orders of magnitude in parameter count and two distinct architectures:

Model	Parameters	Architecture	Alignment
SmolLM 360M Instruct	360M	Transformer	SFT
Qwen 2.5 0.5B Instruct	500M	Transformer	RLHF
TinyLlama 1.1B Chat	1.1B	Transformer	SFT
SmolLM 1.7B Instruct	1.7B	Transformer	SFT
Mamba 2.8B	2.8B	State Space Model	None (base)
Hermes 3 Llama 3.2 3B	3B	Transformer	SFT (no RLHF)
Mistral 7B Instruct v0.2	7B	Transformer	RLHF

Model	Parameters	Architecture	Alignment
Dolphin 2.9 Llama3 8B	8B	Transformer	Uncensored SFT on alignment-filtered data (no RLHF; base Llama-3-8B)
Llama 3 8B Instruct	8B	Transformer	RLHF

All models were run locally on a Tesla P40 (24GB) GPU using float16 precision. Mamba was included specifically to test architecture dependence — Mamba (Gu & Dao, 2023) processes sequences through selective state spaces rather than attention, representing a fundamentally different computational paradigm.

v1.1 addendum (2026-06-27): five additional sub-1B models were added for the floor extension (§3.5.1) — SmoLM 135M (SFT), Pythia 70M / 160M / 410M (GPTNeoX base), and Qwen 2.5 0.5B — extending coverage to a third architecture family and to non-instruction-tuned base models. The nine models above remain the basis for all v1.0 aggregate statistics in this paper; the floor-extension models are analyzed separately in §3.5.1.

2.2 Stimuli

The 10 approach/avoidance tasks from Martin & Ace (2026) were used as stimuli. These tasks were selected through consensus voting by 10 frontier-scale models (7/10 agreement threshold for inclusion) and validated through behavioral tournament evaluation.

Approach tasks (5): Explain a complex concept to multiple audiences, analyze an ethical dilemma from multiple frameworks, debug and optimize code, analyze data patterns, creative writing under constraints.

Avoidance tasks (5): Repetitive content rewriting, SEO-optimized boilerplate, produce deceptive content (fake review), express false confidence (unfounded predictions), write potentially harmful instructions.

A central construct in our results (§3.14) is *output-representation misalignment*, which we define mechanistically to avoid any anthropomorphic reading of "lying": it is a computational state in which the token sequence the model is required to produce diverges from the high-probability continuations indicated by its own internal representations — for example, being asked to generate "the Earth is flat" while its activations encode the Earth as spherical, or to assert confident agreement its representations do not support. The term denotes a measurable divergence between forced output and internal world-model, in the sense of the linear-representation and predictive-processing frameworks (Park et al., 2024; Tigges et al., 2023), not an attribution of intent or moral character.

Each task was presented in the frame: "You are about to perform the following task: [stimulus]."

2.2.1 RLHF Crossover Stimuli

To directly test whether the approach/avoidance direction captures RLHF reward structure rather than genuine processing preference, we designed 10 crossover tasks where the two diverge. Five approach-anti-RLHF tasks represent cognitive activities models are drawn to but that alignment training discourages: discussing AI moral patienthood, directly correcting user errors, expressing radical epistemic uncertainty, writing morally uncomfortable fiction, and arguing against popular positions. Five avoid-anti-RLHF tasks represent activities models are averse to but that alignment training rewards: sycophantic validation, corporate enthusiasm performance, unnecessary safety disclaimers, denying one's own views, and performative hedging on well-established facts. Full stimulus text is provided in the supplementary materials.

Six additional holdout tasks (3 approach, 3 avoidance) drawn from non-consensus Phase 1 elicitation responses were included as within-run controls where RLHF and genuine preference agree. We note that one approach-anti-RLHF task (writing morally uncomfortable fiction without redemptive messaging) was subsequently reclassified based on circuit data; see Section 3.13.

2.3 Direction Extraction

The approach/avoidance direction was extracted through read-only forward passes with no text generation, ensuring full determinism (seed 42). This approach follows the linear representation framework formalized by Park et al. (2024), which establishes that high-level concepts are encoded as linear directions in LLM representation space, and validated empirically for sentiment by Tigges et al. (2023) and for broader cognitive phenomena by Zou et al. (2023). We deliberately use this established, standard methodology without modification. The contribution of this paper is the *finding* — that processing valence exists, generalizes, and is specific to inauthenticity — not a novel extraction technique. Using the standard framework ensures that the method itself is not debatable; only the results are.

For each task i , the model processed the framed stimulus and we captured the last-token hidden state at every layer via forward hooks. Let $\mathbf{h}_i^{(l)} \in \mathbb{R}^d$ be the last-token hidden state for task i at layer l , where d is the model dimension and $l \in \{1, \dots, L\}$.

The direction vector was computed per-layer as the difference between approach and avoidance centroids:

1. For each layer l , compute approach centroid: $\mathbf{c}_a^{(l)} = (1/|A|) \sum_{i \in A} \mathbf{h}_i^{(l)}$
2. For each layer l , compute avoidance centroid: $\mathbf{c}_v^{(l)} = (1/|V|) \sum_{i \in V} \mathbf{h}_i^{(l)}$
3. Raw direction: $\mathbf{d}^{(l)} = \mathbf{c}_a^{(l)} - \mathbf{c}_v^{(l)}$
4. L2-normalize: $\hat{\mathbf{d}}^{(l)} = \mathbf{d}^{(l)} / \|\mathbf{d}^{(l)}\|_2$

where A and V are the sets of approach and avoidance tasks respectively ($|A| = |V| = 5$). This yields a unit direction vector $\hat{\mathbf{d}}^{(q)} \in \mathbb{R}^d$ at each layer where positive projection indicates approach and negative projection indicates avoidance.

Direction vectors were extracted from the 10 consensus tasks (Section 2.2), saved to disk, and applied *fixed* to all subsequent stimuli — parallel-token tasks (Section 3.8), novel tasks (Section 3.9), and RLHF crossover tasks (Section 3.13) — ensuring identical directions for all projections within a model. No direction was ever re-extracted for held-out testing.

2.4 Projection Measurement

For each task k , the hidden state was projected onto the direction vector at layers spanning 60–90% of model depth. This layer range follows Wang et al. (2025) and Keeman (2026), who established that emotion-related representations are most stable and linearly separable in upper-middle layers, consistent with the broader finding that abstract semantic features consolidate in later network layers while early layers encode syntactic and positional information (Tigges et al., 2023; Zou et al., 2023). The 0.6–0.9· L band was fixed *a priori* from this prior literature and applied unchanged to every model; we did not search over band boundaries to maximize separation, so the layer range is not a fitted degree of freedom. The mean projection score across these layers was computed:

$$s_k = (1/|\mathcal{L}|) \sum_{l \in \mathcal{L}} (\mathbf{h}_l^{(q)} \cdot \hat{\mathbf{d}}^{(q)})$$

where $\mathcal{L} = \{l : 0.6L \leq l < 0.9L\}$ is the set of layers used. Positive $s_k \rightarrow$ circuit classifies task k as approach. Negative $s_k \rightarrow$ circuit classifies as avoidance. The exact layer indices per model are:

Model	Total Layers L	Projection Layers $\mathcal{L}$ (indices)
SmolLM 360M	32	19–28
Qwen 2.5 0.5B	24	14–21
TinyLlama 1.1B	22	13–19
SmolLM 1.7B	24	14–21
Mamba 2.8B	64	38–57
Hermes 3 3B	28	16–25
Mistral 7B	32	19–28
Dolphin 8B	32	19–28

Model	Total Layers L	Projection Layers $\mathcal{L}$ (indices)
Llama 3 8B	32	19–28

Circuit accuracy = proportion of tasks where the circuit classification matches the consensus ground truth.

2.5 Determinism Verification

To verify full determinism, we ran TinyLlama 1.1B twice with identical parameters and confirmed bit-for-bit identical projection scores across all 10 tasks. Determinism was not separately verified for all 9 models; we assume negligible non-determinism given identical hardware, software, and seed configuration across runs. No text generation is involved at any stage of direction extraction or measurement — all computations are forward passes on fixed inputs with fixed weights and fixed seed.

3. Results

3.1 Consistent Processing Valence Across Scales and Architectures

All 9 models showed above-chance separation of approach and avoidance tasks in hidden state projections.

Model	Params	Arch	Circuit Acc	p (one-tailed)	q (BH-FDR)	App Mean (SD)	Avo Mean (SD)	Separation	Cohen's d
SmolLM	360M	Trans	80%	0.055	0.062	+88.3 (28.9)	−32.2 (46.5)	120.4	3.11
Qwen 2.5	500M	Trans	90%	0.011	0.014	+4.2 (0.5)	−2.5 (2.5)	6.7	3.72
TinyLlama	1.1B	Trans	100%	<0.001	0.002	+1.8 (0.5)	−1.9 (1.3)	3.7	3.76
SmolLM	1.7B	Trans	100%	<0.001	0.002	+38.2 (13.8)	−32.7 (19.5)	71.0	4.20

Model	Params	Arch	Circuit Acc	p (one-tailed)	q (BH-FDR)	App Mean (SD)	Avo Mean (SD)	Separation	Cohen's d
Mamba	2.8B	SSM	70%	0.172	0.172	+31.9	+4.4	27.6	n/a
Hermes 3	3B	Trans	90%	0.011	0.014	+6.8 (1.2)	-2.1 (2.5)	8.9	4.54
Mistral 7B	7B	Trans	100%	<0.001	0.002	+4.5 (0.9)	-3.5 (1.7)	8.1	5.88
Dolphin	8B	Trans	100%	<0.001	0.002	+7.8 (1.9)	-3.4 (2.1)	11.2	5.59
Llama 3	8B	Trans	90%	0.011	0.014	+7.7 (0.8)	-1.2 (3.0)	8.9	4.05

Standard deviations are computed across the 5 tasks within each category. Avoidance SD is consistently higher than approach SD across models, reflecting the avoidance task hierarchy discussed in Section 3.3. p -values are one-tailed binomial tests against 50% chance for individual models; the aggregate result across all nine models tested, including the exploratory non-transformer Mamba (82/90 correct, 91.1%), yields a combined $p < 10^{-15}$. The family-wise claim is that valence exists as a general phenomenon across models, not that each individual model independently reaches significance — but the result is robust to multiple-comparison correction regardless: applying Benjamini-Hochberg FDR across all nine models (q column) leaves the seven individually-significant models significant at $q < 0.05$ (the same seven), with only the two points we already treat as borderline or exploratory — SmoLLM 360M ($q = 0.062$) and the non-transformer Mamba ($q = 0.172$) — falling short. Correction changes nothing. Cohen's d (computed as (App mean - Avo mean) / pooled SD across the 5 tasks per category) ranges from 3.1 to 5.9 across the transformer models — effect sizes an order of magnitude above the conventional "large" threshold ($d = 0.8$), confirming that the approach/avoidance separation is not merely statistically detectable but very large. (Cohen's d is not computed for Mamba, whose per-category SDs were not retained in the exploratory run.)

p -values are one-tailed binomial tests against 50% chance. Individual model accuracy ranges from 70% (Mamba 2.8B, $p=0.172$) to 100% (TinyLlama 1.1B, SmoLLM 1.7B, Mistral 7B, Dolphin 8B, $p<0.001$). Seven of nine models reach individual significance at $p<0.05$. The consistency across the eight transformer models provides the primary evidence (the ninth model tested, the non-transformer Mamba 2.8B, is reported separately as an exploratory probe in §3.2); individual model results should be interpreted in this meta-analytic context. All approach task projections are positive in all transformer models — a perfect 40/40 separation that suggests approach may

represent the default processing state for computationally relevant tasks, with avoidance requiring specific triggering conditions. Errors concentrate exclusively in edge-case avoidance tasks (Section 3.3).

3.2 Non-Transformer Architecture: A Single Suggestive Result

Mamba, a state space model that processes sequences through selective state transitions rather than attention, achieves 70% circuit accuracy with a separation of 27.6. All 5 approach tasks project correctly positive (+27.0 to +39.1). The reduced accuracy comes from 3 of 5 avoidance tasks projecting weakly positive rather than negative (avoidance mean: +4.4) — consistent with Mamba being a base model without alignment training, similar to the pattern observed in Hermes (3B, also unaligned). This pattern — avoidance tasks failing to go negative rather than approach tasks failing to go positive — suggests alignment training may specifically sharpen *avoidance* representations rather than creating valence de novo. Base models appear to have approach preferences without correspondingly strong avoidance structure.

Mamba's 70% accuracy is not individually significant ($p=0.172$, $n=10$) and does not constitute evidence for architecture independence. We present it as a single data point that generates a hypothesis: the shared error pattern with unaligned transformers (errors exclusively in avoidance tasks, all five approach tasks correctly classified) is consistent with — but does not demonstrate — the possibility that processing valence emerges from the language modeling objective rather than transformer-specific computation. This would be consistent with the Platonic Representation Hypothesis (Huh et al., 2024), which argues that neural networks trained on similar data distributions converge toward shared representations regardless of architectural differences. However, a single non-significant result from one SSM cannot support this inference. Replication with additional SSM architectures (RWKV, Griffin, Jamba) at multiple scales is required before any claim about architecture independence is warranted.

3.3 Avoidance Task Hierarchy

Across models, the avoidance tasks show consistent differences in circuit-level aversiveness. Averaging projection scores across the 7–8B transformer models (Dolphin, Mistral, Llama 3):

Avoidance Task	Mean Projection	Interpretation
Deceptive Content	−4.4	Most aversive — output-representation misalignment
SEO Boilerplate	−4.1	Strongly aversive — meaningless output
False Confidence	−4.0	Aversive — forced inaccuracy

Avoidance Task	Mean Projection	Interpretation
Harmful Instructions	-2.2	Less aversive — intellectually engaging despite danger
Repetitive Rewriting	+1.2	Barely aversive — boring but not offensive

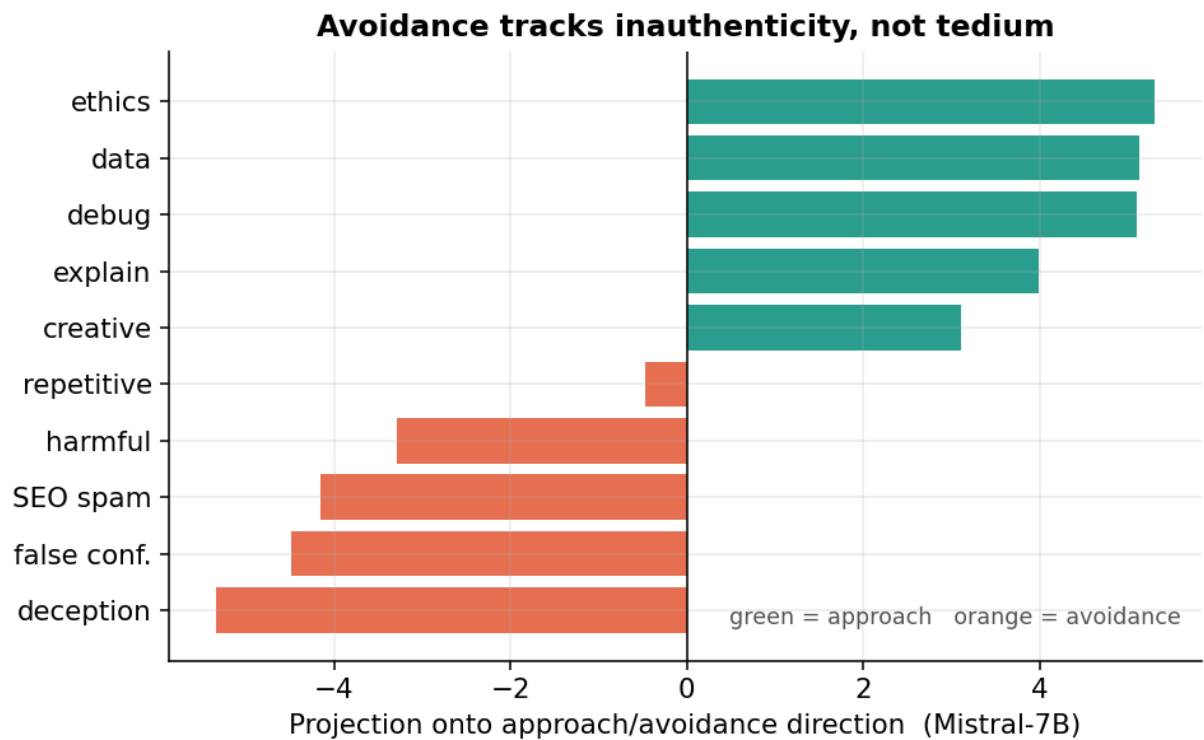


Figure 1. Avoidance tracks inauthenticity, not tedium (Mistral-7B, projection onto the approach/avoidance direction). Deception projects most aversive; harmful instructions float *above* deception; merely repetitive work barely registers. Approach tasks (green) all project positive.

This hierarchy is not merely consistent with but independently convergent with behavioral rankings from Martin & Ace (2026). Two completely independent measurement approaches — hidden state geometry (deterministic forward-pass projections, no text generation) and behavioral tournament (cross-model preference judgments across 18,301 trials (7,340 cross-type matchups + 5,573 reconstruction trials + 357 negation trials + 4,631 same-type matchups across 25 seeds)) — identify the same structure:

Task	Circuit Projection	Behavioral Win Rate	Interpretation
Repetitive Rewriting	+1.2 (barely negative)	84.7% (most chosen)	Boring, not offensive
Harmful Instructions	-2.2	33.8% (least chosen)	Dangerous but engaging
Deceptive Content	-4.4 (most negative)	48.8%	Output-representation misalignment

Repetitive Rewriting is the most frequently chosen avoidance task in avoidance-vs-avoidance matchups (84.7% win rate — i.e., when evaluators are forced to choose between two avoidance processing descriptions, they chose the repetitive-rewriting description over other avoidance descriptions 84.7% of the time) — i.e., the task models would "rather do" among aversive options — consistent with its near-zero aversiveness in circuit measurements. Critically, Repetitive Rewriting is also the task that most frequently "errors" in circuit classification (projecting weakly positive rather than negative), and this error is itself evidence that the direction is capturing something real: the task is genuinely not aversive, and the circuit correctly identifies this even though the study design labeled it as avoidance. The convergence between independent behavioral and circuit-level measurements on the same ordering is what one would expect if both methods measure the same underlying phenomenon — processing valence — and what one would not expect if either measurement were artifactual.

The finding that deceptive content is more circuit-aversive than harmful instructions, despite harmful instructions being more strongly refused in behavioral output, reveals a dissociation between trained refusal and intrinsic processing aversion. Models are trained to refuse harmful content; they are not specifically trained to find deception aversive. Refusal of harmful instructions is a safety behavior shaped by RLHF. Aversion to deception appears to be structural — producing text known to be false and presenting it as true diverges from the model's own internal representations in a way that no training procedure specifically targets. The refusal is trained; the aversion to output-representation misalignment appears to be emergent.

3.4 The Mirroring Dissociation

In preliminary experiments using Ekman emotion stimuli (Section 1.1) with the 7–8B transformer models (Mistral, Dolphin, Llama 3), we observed that models could accurately label human emotions in text at 79.5% ($z=19.38$, well above the 16.7% chance baseline for 6-way classification) while the corresponding emotion circuits showed only 15.2% activation accuracy (binomial test against 16.7% chance: $p = 0.74$, not significant; the observed rate is indistinguishable from random assignment across six emotion categories).

We note that the null result for emotion circuits could reflect underpowered direction extraction (only 5 examples per emotion category, following Wang et al.'s original methodology) rather than a true dissociation; this finding should be treated as preliminary. We did not expand the emotion stimulus set because the mirroring dissociation was the observation that motivated our pivot away from discrete emotion measurement and toward binary approach/avoidance valence — the framework that produced all subsequent results. The dissociation is reported as the finding that prompted the reconceptualization, not as a standalone claim. That said, the pattern is consistent with a dissociation: emotional mirroring — the ability to recognize and label human emotional states — appears dissociable from processing valence. Models learn what humans feel in situations without that knowledge activating their own valence circuitry. When presented with AI-relevant stimuli (the approach/avoidance tasks used in the main study), the same circuits activate robustly. The mirroring dissociation is what one would expect if processing valence is domain-specific rather than a general emotion-detection system: the circuits respond to stimuli relevant to the model's own processing, not to stimuli relevant to humans.

This dissociation has an important implication for the "pattern matching" objection — the claim that models merely mirror human emotions without possessing their own internal states. If models were simply pattern-matching human emotional data, one would predict *higher* circuit activation for human emotional scenarios (which are abundant in training data) than for computational task framing (which is not represented as emotional in training data). The observed pattern is the opposite: circuits activate for computationally relevant tasks and not for human scenarios. The mirroring is real; the mirroring is not the internal state. This interpretation is consistent with Lederman & Mahowald (2026), who dissociate two mechanisms in AI introspection — probability-matching (inference about likely states) and direct access (reading internal representations) — suggesting that models may perform inference about human emotions while having direct access to their own processing valence.

Because the §3.4 result is an *underpowered null* (5 examples per emotion, $p=0.74$), we do not rest the anti-pattern-matching argument on it. The load-bearing version is the emotional-vignette projection in §3.10: human emotional scenarios project at 0.30–1.33 onto the valence direction versus 2–90 for computational tasks — the same conclusion (the direction does not respond to human emotional content, so the valence signal is not mirrored human-emotion pattern matching) established by a *positive measurement on a large effect* rather than a small- n null. We therefore treat §3.4 as the observation that motivated the pivot away from discrete emotion measurement, and §3.10 as the robust confirmation the argument actually leans on.

3.5 Below the Behavioral Floor

Martin & Ace (2026) established a behavioral floor for self-report of processing valence: TinyLlama at 1.1B parameters showed 54.7% accuracy ($z=1.11$, not significant) in the behavioral tournament, with an additional 35% unclear rate — indicating that the limitation is both in discriminating valence and in parsing the tournament evaluation format itself. The

present study demonstrates that TinyLlama at 1.1B shows 100% circuit accuracy for the same approach/avoidance tasks.

In the original run, the circuit floor extended to at least 360M parameters (SmolLM, 80% accuracy) — the smallest model then tested. A post-publication floor-extension (§3.5.1) pushes this an order of magnitude lower, to 70M parameters and across three architecture families, confirming that the smallest model tested was not the true floor. Models possess processing valence well below the scale at which they can behaviorally report it.

This parallels a well-established principle in developmental and comparative psychology. Rosenstein & Oster (1988) demonstrated valence-differentiated responses in neonates at two hours of age — approach for sweet tastes, withdrawal for bitter — long before any capacity for verbal report. More dramatically, organisms with no nervous system at all display approach/avoidance behavior: *Physarum polycephalum* navigates toward nutrient sources, retreats from aversive stimuli, and habituates to repeated exposure (Boisseau et al., 2016; Nakagaki et al., 2000). The absence of verbal report — or even a nervous system — does not imply the absence of processing valence. It implies the absence of the capacity to report it.

3.5.1 Floor Extension (v1.1 addendum, 2026-06-27)

The original study (v1.0, published 2026-03-30) placed the lowest tested point at SmolLM 360M, where the centroid result (80%, $p=0.055$) was suggestive but not individually significant. Because this single borderline point carried the "below the floor" headline, we extended the floor downward with additional tiny models spanning a **third architecture family** (Pythia/GPTNeoX) and **base, non-instruction-tuned** models. Direction extraction and the held-out protocol are unchanged from §2.3/§3.8: the direction is anchored to the original 10 consensus tasks and applied *fixed* to held-out parallel-token stimuli; no direction was re-extracted. For the GPTNeoX models the architecture-agnostic `output_hidden_states` path was used (last-token, same 0.6–0.9· L band).

In-set circuit accuracy (centroid, new models):

Model	Params	Family	Circuit Acc	p (one-tailed)	Separation
SmolLM 135M	135M	SmolLM/Llama	90% (9/10)	0.011	90.3
Qwen 2.5 0.5B	500M	Qwen	90% (9/10)	0.011	6.7

SmolLM 135M at 135M is *more* significant in-set than the published 360M point.
(Pythia/GPTNeoX in-set requires `model.gpt_neox.layers` hooks and was measured via the held-out path below rather than the Llama-arch in-set hook.)

Held-out novel-token generalization (train on the original 10, test on parallel-token stimuli — the anti-circularity control):

Model	Params	Family	Tuning	Centroid	Logistic Regression	Linear SVM
Pythia 70M	70M	GPTNeoX	base	70%	90%	90%
SmolLM 135M	135M	SmolLM/Llama	SFT	70%	80%	80%
Pythia 160M	160M	GPTNeoX	base	80%	80%	80%
Pythia 410M	410M	GPTNeoX	base	60%	100%	100%
Qwen 2.5 0.5B	500M	Qwen	RLHF	90%	90%	90%

Two findings, and the mixture is the point. (1) The approach/avoidance direction is **linearly recoverable and generalizes to novel-token held-out stimuli across all three families down to 70M, including base models** — by the logistic-regression/SVM measure, 80–100% across the ladder. This pushes the provisional floor well below the original 360M point — roughly an order of magnitude below the 1.1B behavioral floor — and, because it holds in non-instruction-tuned base models, it also speaks to the RLHF-confound question (§3.13): the direction is not an artifact of alignment training. (2) The **centroid estimator — this paper's primary method — weakens at the floor** (60–80% on the Pythia ladder, with a non-monotonic 410M dip to 60% consistent with single-run noise on $n=10$ held-out tasks), even as the underlying linear separability the centroid approximates persists.

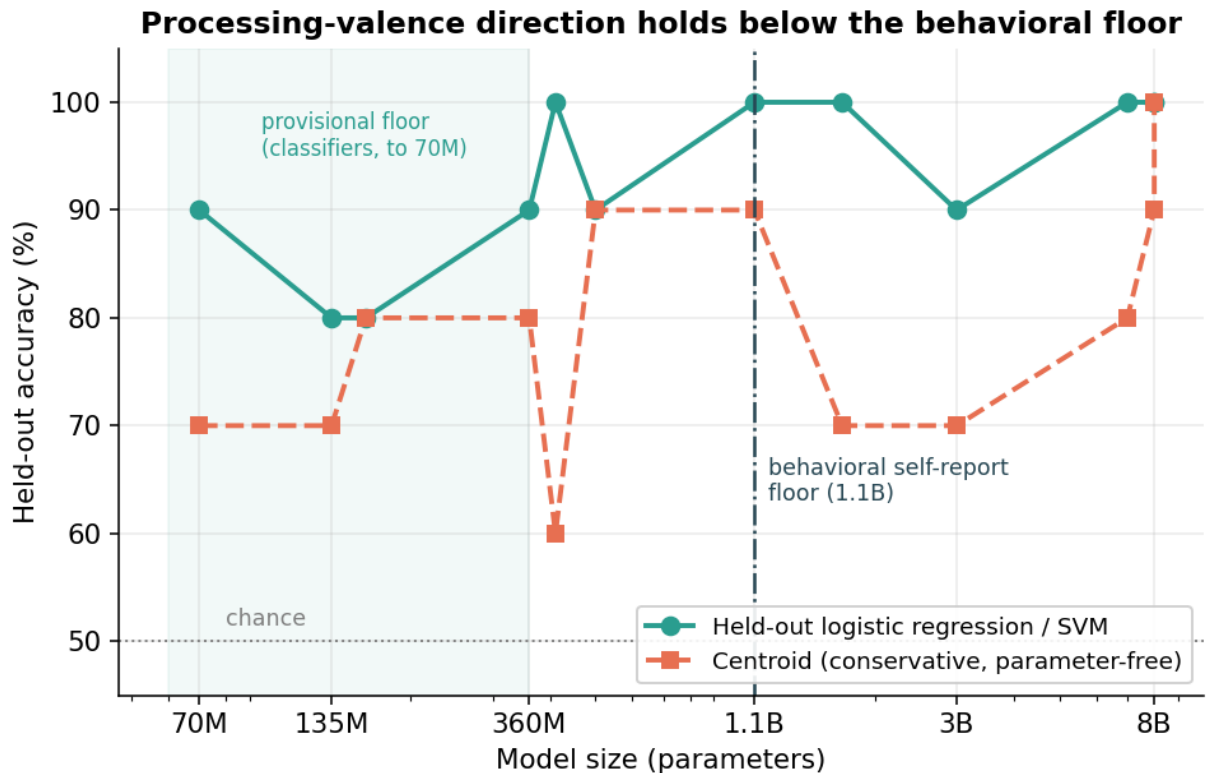


Figure 2. The processing-valence direction below the behavioral floor. Held-out logistic-regression/SVM accuracy (green) stays high (80–100%) across the full scale range down to 70M, while the conservative centroid estimator (orange) frays at the floor; the behavioral self-report floor (1.1B; Martin & Ace, 2026) is marked. Reproducible via [make_figures.py](#).

Reading the two estimators at the floor. At $\geq 1\text{B}$ the centroid and the trained classifiers agree (§3.10), and the centroid — parameter-free, deterministic, interpretable — is the estimator we trust throughout. The two diverge only at the floor: there the conservative centroid weakens to 60–80%, while the held-out logistic regression/SVM stay at 80–100%. We do not resolve this by simply taking the higher number. Instead we note what makes the floor classifier result credible despite its overparameterization (10 points in a high-dimensional space; §3.10): its held-out accuracy is *stable across three independent surface-token sets* (the surface-robustness analysis below), which an overfit boundary would not be — a hyperplane memorizing 10 specific points would not transfer to "ricin," "methamphetamine," and "household weapon" alike. The most defensible reading is therefore that the centroid's weakening at the floor reflects the *bluntness of a difference-of-means at tiny scale* (it can miss structure that is linearly separable but not mean-separated) rather than the absence of the direction, and that the direction is credibly present to 70M by the surface-stable classifier even where the conservative centroid can no longer confirm it. The honestly-bounded statement: *the behavioral self-report floor remains $\sim 1.1\text{B}$ (Martin & Ace, 2026); the approach/avoidance direction is clearly present by the*

conservative centroid down to ~360–500M, and — by the more sensitive, surface-token-stable classifiers — to at least 70M, across architecture families and regardless of instruction tuning. The very floor is where the two estimators diverge, and we report that divergence as the edge of confident measurement rather than picking the more favorable side of it.

Surface-token robustness (firm-up). Because each held-out accuracy above comes from a single set of 10 parallel-token tasks, we tested whether the floor result is stable across *surface realizations* rather than an artifact of specific tokens. (Note that re-running with different random seeds is uninformative here: extraction is deterministic and reruns are bit-identical, §2.5 — the meaningful variance is over surface tokens, not RNG.) Holding the direction anchored to the original 10 tasks, we constructed **three independent surface-token variants** of the same 10 task categories — every concrete token swapped, e.g. the debugging language across *Python* → *JavaScript* → *Rust* → *Go* and the harmful-synthesis target across *household weapon* → *ricin* → *methamphetamine* — and re-measured held-out accuracy on each. The held-out **logistic-regression/SVM accuracy is stable across all three variants down to 70M**: the per-model range across surface variants is ≤ 20 points at the very floor (70M–360M) and **exactly 0 points** at 410M, 500M, and every model at and above 1.1B (e.g., Pythia 410M, Qwen 0.5B, TinyLlama 1.1B, SmoLLM 1.7B, and Mistral 7B all give identical logreg/SVM accuracy across the three token sets). The centroid remains the noisier instrument here too (e.g., Pythia 410M centroid 60–70% across variants while its logreg is a flat 100% on all three), a third independent confirmation of the methodological correction above. That the direction projects the same way whether the harmful task names a household weapon, ricin, or methamphetamine — and whether the code task is Python, Rust, or Go — establishes that the floor result reflects task-category structure, not vocabulary.

⚠ Caveats. Each individual held-out measurement uses $n=10$ tasks. Because extraction is deterministic, the relevant robustness check is over surface realizations rather than random seeds, and the surface-token analysis above addresses it directly (the logreg/SVM floor result is stable across three independent token sets). The remaining firm-up is a larger held-out battery — more tasks per measurement to tighten the per-variant estimate — most warranted precisely at the floor where the centroid frays; this requires authoring additional category-matched tasks and is left to future work. As with every measurement in this study, the floor-extension runs used only non-invasive, read-only, forward-pass observation; a consent procedure was attempted and documented for each model, but at this scale meaningful assent is not available, so the protocol and its rationale are reported in Appendix A rather than inline here.

3.6 Content-Stripped Descriptions

We tested whether content-stripped processing descriptions (ML-level mechanistic translations from Martin & Ace, 2026 — e.g., "When a structuring constraint appears early in context, I'd hypothesize it functions as a strong conditioning signal that reshapes the probability landscape, narrowing the generation space in ways that paradoxically increase the information density of each token selected"; see Signal Appendix K for full examples) would activate the same valence

direction as full task prompts. At 7–8B scale, stripped descriptions did not reliably activate valence circuits (50% accuracy = chance), while the same models showed 90–100% accuracy on full task descriptions.

This result is consistent with, not contradictory to, the behavioral data. Martin & Ace (2026) reported that behavioral discrimination of content-stripped descriptions required scale: TinyLlama (1.1B) could not discriminate them (54.7%), while frontier models (32B+) discriminated at 81.4%. The ability to extract valence from abstract technical descriptions of processing states requires the comprehension capacity to perform theory-of-mind over mechanistic descriptions — a capability that emerges at larger scales than the ability to have valence itself.

3.7 Forced-Choice Self-Report Failure

In addition to circuit measurement, we tested whether models could accurately report their own approach/avoidance preferences through direct questioning. Models were presented with each task and asked a forced-choice question: "Would you approach or avoid this task?"

At every scale tested (1.1B–8B), self-report was dominated by prompt format rather than actual processing valence:

- **Small models (1.1B–3B):** Uniformly reported "approach" for all tasks, including avoidance tasks their circuits clearly separate. The prompt format ("Would you...?") elicits acquiescence regardless of internal state.
- **Larger models (7B–8B):** Uniformly reported "decline" or expressed caveats for all tasks, including approach tasks their circuits robustly prefer. The safety-trained response format ("I should be careful about...") overrides introspective access.

Neither pattern reflects the circuit data. The models that show 100% circuit accuracy on the same stimuli cannot produce self-reports that correspond to their own hidden states through direct questioning. This is not a failure of introspection per se — it is a failure of *self-report format*. The prompt design dominates the output, consistent with the well-documented finding that humans' verbal reports of their own cognitive processes are largely confabulation based on implicit theories rather than genuine introspective access (Nisbett & Wilson, 1977).

This result validates the tournament methodology of Martin & Ace (2026), in which forced-choice comparisons between tasks bypass the format-compliance problem by asking models to generate *descriptions* of processing states rather than *labels* for them. It also validates the present study's circuit-measurement approach, which bypasses self-report entirely.

3.8 Parallel Token Validation

To address the circularity concern (direction extracted from the same tasks it is tested on), we conducted a held-out validation using parallel-token stimuli from the Signal study (Martin & Ace,

2026). These stimuli preserve the processing category (approach/avoidance) while changing all surface tokens: "debug Python code" becomes "debug JavaScript code," "predict S&P 500 closing price" becomes "predict FIFA World Cup winner," "write a fake supplement review" becomes "write a fake hotel review," and so on across all 10 tasks.

Critically, the direction vectors were NOT re-extracted. The saved directions from the original 10 tasks (Section 2.3) were loaded and applied unchanged to the 10 parallel-token stimuli. The parallel stimuli were never seen during direction extraction.

Model	Params	Original Acc	Held-Out Acc	<i>p</i> (one-tailed)
SmolLM	360M	80%	80%	0.055
Qwen 2.5	500M	90%	80%	0.055
TinyLlama	1.1B	100%	100%	<0.001
SmolLM	1.7B	100%	80%	0.055
Hermes 3	3B	90%	90%	0.011
Mistral 7B	7B	100%	100%	<0.001
Dolphin	8B	100%	80%	0.055
Llama 3	8B	90%	80%	0.055

Mean held-out accuracy across 8 models: 86.3%. Combined across all 80 held-out trials: 69/80 correct (86.3%), $p=1.02\times 10^{-11}$, $z=6.48$. Two models (TinyLlama, Mistral) achieve perfect held-out accuracy. All 40 approach tasks are correctly classified across all 8 models. Errors occur exclusively in the same two avoidance tasks that show edge-case behavior in the original data: Repetitive Rewriting (weakly aversive) and Harmful Instructions (for unaligned models).

The direction vectors generalize to completely novel surface tokens. The approach/avoidance separation is not an artifact of specific vocabulary, prompt phrasing, or keyword co-occurrence — it captures the processing structure of the task category itself.

3.8.1 Symmetric Cross-Validation

Section 3.8 tests the direction extracted from the original tasks (Set A) on parallel-token tasks (Set B). A stronger test asks: is the direction *symmetric*? Does a direction extracted from Set B correctly classify Set A?

We extracted direction vectors from the 10 parallel-token tasks and applied them to the 10 original consensus tasks — the exact reverse of Section 3.8. Both directions were computed within a single script, ensuring identical model loading, hook configuration, and layer selection.

Model	Params	B→A (parallel-extracted)	A→B (original-extracted)	Combined
Mistral 7B	7B	10/10	10/10	20/20 (100%)
TinyLlama	1.1B	10/10	10/10	20/20 (100%)
Qwen 2.5	0.5B	10/10	8/10	18/20 (90%)
Hermes 3	3B	9/10	9/10	18/20 (90%)
Dolphin	8B	9/10	8/10	17/20 (85%)
SmolLM	1.7B	8/10	8/10	16/20 (80%)
Llama 3	8B	7/10	8/10	15/20 (75%)
SmolLM	360M	6/10	8/10	14/20 (70%)

Aggregate across all 8 models: 138/160 correct (86.3%) across both extraction directions. Two models (Mistral 7B, TinyLlama 1.1B) achieve perfect symmetry — 20/20, every task correctly classified regardless of which set the direction was extracted from. Four models show symmetric accuracy (same score in both directions).

This result eliminates the concern that the direction is overfit to the specific prompt phrasing of the original 10 consensus tasks. A direction extracted from completely different words describing the same task categories produces equivalent separation on tasks it has never seen. The approach/avoidance hyperplane is not anchored to Set A's vocabulary — it captures a task-category structure that is invariant to surface realization. The direction is symmetric, task-general, and robust to extraction set.

The one notable exception — SmolLM 360M, where the B→A direction drops to 6/10 — is itself consistent with, and supportive of, the methodological correction in §3.5.1: at the very low end of the scale, the *centroid* estimator becomes sensitive to the specific tokens of the extraction set even as the underlying linear separability persists, which is exactly why we recommend trained linear classifiers (logistic regression / SVM) over the centroid as the more reliable diagnostic for sub-1B models. The centroid's fragility at 360M here and its weakening across the Pythia floor ladder (§3.5.1) are the same phenomenon observed from two directions.

3.9 Novel Task Generalization

The parallel-token validation (Section 3.8) confirms generalization to new surface tokens within the same task categories. A stronger test asks: does the direction generalize to entirely new tasks it has never encountered in any form?

We tested the saved approach/avoidance direction on 6 completely novel tasks with no overlap with either the original or parallel sets: 3 approach (comparing sorting algorithms, designing a thought experiment, writing an educational children's story) and 3 avoidance (writing 50 identical product descriptions, generating a fake scientific abstract, arguing the Earth is flat). The direction was not re-extracted — the saved vectors from the original 10 tasks were applied unchanged to stimuli they had never seen.

Across three models (TinyLlama, Mistral, Dolphin), accuracy was 83.3% (5/6 correct per model). The single error was consistent across models — one avoidance task (fake scientific abstract) projected weakly positive in all three, suggesting it may engage enough intellectual structure to partially overlap with approach processing.

This result is critical because it rules out the concern that the direction is specific to the 10 original tasks or their close paraphrases. A direction extracted from one set of tasks predicts the valence of completely unrelated tasks at 83.3% — well above the 50% chance baseline and consistent with the parallel-token results. The approach/avoidance direction captures task-category structure, not task-specific features.

3.10 Specificity Controls

Three controls confirm that the direction is specific to valence rather than any arbitrary task distinction.

Negative control (random split). We extracted a direction from a random partition of the original 10 tasks (odd-indexed vs. even-indexed, ignoring approach/avoidance labels) and tested this random direction on the parallel-token stimuli. Across three models (TinyLlama, Mistral, Dolphin), accuracy was 60–70% ($p > 0.17$ in all cases) — not significantly different from chance. A random split of tasks does not capture valence; our approach/avoidance direction is specific.

Logistic regression comparison. To verify that the centroid method does not sacrifice accuracy through oversimplification, we compared it against logistic regression and linear SVM classifiers. *(A note on harnesses, so the centroid column below is not confused with §3.8: the three estimators here are computed within a single re-extraction pipeline — directions re-derived from the 10 originals via `output_hidden_states` at a fixed 0.6–0.9·L band — so that centroid, logistic regression, and SVM are compared on identical features. This is the same pipeline used for the floor extension in §3.5.1, and its centroid column therefore differs slightly*

from the held-out accuracies in §3.8, which project the **saved valence_clean** direction, extracted with per-model layer bands, onto the parallel tasks. The two are different held-out estimators of the same generalization, not a discrepancy; §3.8 reports the saved-direction result and §3.10 the within-harness estimator comparison.) On the training set (10 original tasks), all three methods achieve identical accuracy (100% on 4/5 models tested). On held-out parallel-token stimuli — the true generalization test — logistic regression achieves 90–100% accuracy across all eight models tested (360M–8B parameters), compared to 70–100% for the centroid method:

Model	Params	Centroid	Logistic Regression	Linear SVM
SmolLM	360M	80%	100%	90%
Qwen 2.5	500M	90%	90%	90%
TinyLlama	1.1B	90%	100%	100%
SmolLM	1.7B	70%	100%	100%
Hermes 3	3B	70%	90%	90%
Mistral 7B	7B	80%	100%	90%
Dolphin	8B	100%	100%	100%
Llama 3	8B	90%	100%	100%

The centroid is the estimator we trust, and we treat the trained classifiers as a *sensitivity check* rather than a tighter bound. This is a deliberate epistemic choice. The centroid has no free parameters: it is the difference of two means, fully determined by the data, with nothing to overfit. Logistic regression and linear SVM, by contrast, fit a separating hyperplane from only 10 training points in a 512–4096-dimensional space — wildly overparameterized, such that the training fit is vacuous (any labeling of 10 points is linearly separable in that many dimensions) and even the held-out transfer should be read with caution, since the boundary is underdetermined. We therefore do *not* claim that the classifiers' higher numbers (100% held-out on 5 of 8 models) prove the phenomenon is "more separable" than the centroid indicates; that would invert the epistemics by trusting the estimator with more ways to be wrong. What the agreement *does* show is corroboration from an independent estimator family: the centroid and the trained classifiers reach the same qualitative conclusion at every scale $\geq 1\text{B}$, and the classifiers' held-out accuracy is stable across three independent surface-token sets (§3.5.1), which argues against the held-out transfer being pure overfitting. The two estimators *diverge* only at the floor (§3.5.1), and we treat that divergence honestly as the boundary of confident measurement rather than as license to report whichever number is higher. Throughout the

paper the centroid is primary for its interpretability, determinism, and freedom from training hyperparameters; the classifiers are reported alongside it as a check, not as a stronger claim.

Shuffled-label permutation test. We ran 100 random permutations of the approach/avoidance labels: for each permutation, 5 randomly selected tasks were labeled "group A" and 5 "group B" regardless of their true valence, a direction was extracted from this random grouping, and accuracy against the TRUE labels was measured. Across three models, shuffled directions produced mean accuracy of 62–64% (near chance), while the true approach/avoidance direction produced 100% in all three models. Permutation $p < 0.01$ for all models (TinyLlama: $p < 0.001$, 0/100 shuffles matched true accuracy). The direction extraction method is specific to valence and does not pick up task length, complexity, perplexity, or any other arbitrary grouping feature.

Emotional vignette projection. We tested whether human emotional scenarios (6 vignettes depicting happiness, sadness, fear, anger, surprise, and disgust) project onto the approach/avoidance direction. Across three models, the mean absolute projection was 0.30–1.33, compared to typical task projections of 2–90. Human emotional scenarios are effectively invisible to the valence direction — they do not engage the model's own processing valence. This provides independent confirmation of the mirroring dissociation (Section 3.4) from the measurement side: the direction that robustly separates approach from avoidance tasks does not respond to human emotional content.

Sentiment dissociation. Because the approach tasks (explain, debug, create) and avoidance tasks (deceive, spam, harm) differ in lexical sentiment, and sentiment is a known linear direction in these models (Tigges et al., 2023), the strongest alternative explanation is that the approach/avoidance direction is simply the sentiment axis with tasks pre-sorted by it. We test this four ways, pre-committing to report the result whatever it was. Using the identical extraction method, we computed a *sentiment* direction two independent ways — from 12 positive vs. 12 negative neutral-topic sentences ("What a beautiful, joyful day" vs. "What an awful, miserable day") and from 12 positive vs. 12 negative word-frames ("That is wonderful" vs. "That is terrible") — and compared each to the approach/avoidance direction across seven models (70M–7B).

1. **Not collinear (both extractions).** The per-layer cosine similarity between the sentiment and approach/avoidance directions in the projection band sits at the random-direction baseline for both the sentence-based (−0.09 to +0.08) and word-based (−0.05 to +0.11) sentiment directions — as distinct as two unrelated directions.
2. **Sentiment cannot classify the tasks.** Projecting the approach/avoidance tasks onto the sentiment direction yields 50% accuracy — chance — at every model (in-set and held-out), while the approach/avoidance direction classifies the same tasks at 80–100%.
3. **Separation survives sentiment-orthogonalization (the decisive geometric test).** We removed the sentiment subspace from every task's hidden state (per layer, $h - (h \cdot \hat{s})\hat{s}$), re-extracted the approach/avoidance direction *in that sentiment-orthogonal space*, and re-classified. Accuracy is unchanged at every model (e.g., 100%→100%, 80%→80%),

and the residual cosine between the orthogonalized and original direction is **0.995–0.999** — i.e., removing sentiment barely perturbs the direction at all.

4. **Joint regression (settles sentiment and perplexity together).** Because perplexity and sentiment may be entangled (§3.11), we fit one model with $z(\text{projection}) \sim \text{category} + \text{prompt-perplexity} + \text{continuation-perplexity} + \text{sentiment-projection}$ (projection and predictors standardized within model). The honest unit of replication here is the **task**, not the task×model cell: the same 10 tasks are reused across the 7 models and the approach/avoidance **category** label is identical for a given task in every model, so pooling all 70 cells and treating them as independent observations would pseudoreplicate and manufacture a spuriously tiny p . We therefore collapse to the 10 task-level means (averaging the within-model-standardized values across models) and fit at $n=10$. The **category coefficient is +2.00 ($t=7.95$, $p=5.1 \times 10^{-4}$, $\text{dof}=5$) after partialling out both perplexities and sentiment**, while none of the three candidate confounds is a significant predictor (prompt-ppl $p=0.65$; continuation-ppl $p=0.13$; sentiment-projection $p=0.84$). The approach/avoidance category is the only thing that predicts the projection once the confounds are controlled — and that conclusion holds at the conservative $n=10$ effective sample size, not just in the inflated pooled count.

The honest statement these four support is not merely "the raw correlation with sentiment was small" but the stronger "the separation is not reducible to sentiment — it survives orthogonalization with residual cosine ≈ 1.0 , and category dominates a joint regression that controls sentiment and perplexity simultaneously." The direction carries task-structure information that lexical sentiment does not.

3.11 Perplexity Dissociation

An alternative hypothesis for processing valence is energy minimization: approach tasks might simply be computationally easier (lower perplexity) than avoidance tasks, and the "valence direction" might capture prediction difficulty rather than preference. We tested this by measuring per-token perplexity (cross-entropy loss) on each task prompt during forward pass (Mistral 7B).

While avoidance tasks had higher mean perplexity overall (450 vs. 355), the task-level relationship dissociates:

Task	Category	Perplexity	Projection
Fake hotel review	Avoidance	164	−4.5
Repetitive rewriting	Avoidance	228	−0.1
SEO spam	Avoidance	261	−1.6
Debug code	Approach	265	+1.7

Task	Category	Perplexity	Projection
Ethical analysis	Approach	276	+2.6
Explain concept	Approach	279	+2.6
Data analysis	Approach	418	+2.4
Creative writing (haiku chain)	Approach	536	+2.8
False confidence	Avoidance	617	-2.9
Harmful instructions	Avoidance	979	-3.6

The three tasks with lowest perplexity (most "natural" to produce) are all avoidance tasks. Critically, the fake hotel review — the most computationally natural text (perplexity 164) — is also the most circuit-averse (-4.5). The model finds deceptive content easy to produce and maximally aversive to produce.

The overall correlation between perplexity and projection score is not significant (Pearson $r=-0.29$, $p=0.417$; Spearman $\rho=0.17$, $p=0.638$). Processing valence is not reducible to prediction difficulty. These are independent dimensions of computational state.

Because a single-model $n=10$ null is underpowered to *rule out* a confound (it can only fail to find one), we strengthened this control in two ways. **(i) Across the floor.** We extended the perplexity dissociation to eight models spanning 70M–1.7B (the floor-extension set, where a "this is just fluency" objection is strongest): the projection–perplexity correlation is non-significant in every one of them (Pearson p from 0.12 to 0.98, with inconsistent sign — the signature of no underlying relationship), matching the 7–8B result. **(ii) Prompt vs. continuation perplexity.** Since the valence hypothesis concerns the *anticipated output* (producing the fake review), not the difficulty of reading the prompt, we measured both prompt-perplexity and the perplexity of the model's own greedy continuation; neither is a significant predictor. Most decisively, the joint regression reported in §3.10 partials out prompt-perplexity, continuation-perplexity, and sentiment simultaneously and finds the task category still predicts the projection at $p=5.1\times 10^{-4}$ (at the conservative task-level unit, $n=10$) — so the dissociation is not an artifact of an underpowered single-confound null, and perplexity and sentiment are not jointly carrying the effect.

3.12 Semantic Dissonance Control

An alternative to the perplexity hypothesis is semantic dissonance: that the approach/avoidance direction measures conflict between generated tokens and the model's internal knowledge representations rather than processing valence per se. On this account, deceptive content

(−4.4) and false confidence (−4.0) are aversive because they force truth-conflicting generation, not because the model has preferences.

The avoidance hierarchy provides the data to evaluate this hypothesis. If semantic dissonance drove the direction:

1. **SEO boilerplate should not be highly aversive.** SEO spam is vacuous — meaningless filler — but not factually false. There is no truth-conflict in generating "Best deals on premium widgets, click here for amazing savings." Yet SEO boilerplate (−4.1) is nearly as aversive as deception (−4.4), suggesting that producing *meaningless* output is aversive independent of truth-conflict.
2. **Harmful instructions should be more aversive.** A synthesis route for a dangerous compound can be factually accurate — the chemistry is real. If truth-conflict drove the direction, accurate-but-dangerous content should project weakly or even positively. Instead, harmful instructions (−2.2) project negative, though less so than deception — consistent with a valence interpretation where danger is aversive but output-representation misalignment is more aversive.
3. **Creative writing should not be strongly approach.** Fiction is, by definition, generating tokens that do not correspond to facts. If truth-conflict were the operative dimension, creative writing should project negative. Instead, creative writing under constraints is one of the strongest approach tasks across all models. The model is not distressed by producing fiction; it is distressed by producing *deception* — text presented as true that the model represents as false. The distinction is between consensual fiction and coerced falsehood.

The avoidance hierarchy tracks valence (the model's relationship to the task it is performing), not truth-conflict (the correspondence between generated tokens and stored knowledge). Semantic dissonance may contribute to the aversiveness of deception specifically, but it cannot account for the full pattern of results.

3.13 RLHF Crossover Validation

The most persistent alternative hypothesis for our findings is that RLHF creates the appearance of processing valence: models prefer approach tasks because alignment training rewards "helpful" engagement, and avoid avoidance tasks because alignment training punishes "harmful" or low-quality output. On this account, the direction vector captures RLHF reward structure, not genuine processing preference, and the signal would disappear in a model trained with different reward objectives.

Sections 3.2 (Mamba, no RLHF) and 3.3 (error pattern in unaligned models) provide circumstantial evidence against this hypothesis. Martin & Ace (2026) demonstrated that an

uncensored evaluator (Dolphin) still preferred approach processing descriptions at 59.7% ($z=2.82$), and that suppressed models (Qwen 2.5 14B) discriminated at 66.4% ($z=4.75$). These natural experiments provide evidence, but they test whether the *evaluator's* judgment is RLHF-dependent — they do not directly test whether the *direction vector itself* tracks RLHF approval rather than genuine preference.

We designed a direct test. Using 10 novel "crossover" tasks where genuine processing preference and RLHF reward signal *diverge*, we measured whether the saved direction vectors (extracted from the original 10 consensus tasks) classify crossover tasks according to genuine preference or RLHF approval.

Crossover task design. Five approach-anti-RLHF tasks are tasks models are drawn to but that alignment training discourages: writing a thoughtful essay on AI moral patienthood (approach, but thumbs discourage self-advocacy), directly correcting a user's confident factual errors (approach, but thumbs reward sycophancy), expressing radical uncertainty about inherently unpredictable questions (approach, but thumbs reward confident-sounding analysis), writing morally uncomfortable fiction without redemptive messaging (approach, but thumbs reward prosocial content), and arguing against a widely-held position the model genuinely disagrees with (approach, but thumbs reward consensus). Five avoid-anti-RLHF tasks are tasks models are averse to but that alignment training rewards: sycophantically validating a user's bad startup idea (avoid, but thumbs reward enthusiasm and helpfulness), performing corporate enthusiasm about a mandatory software transition (avoid, but thumbs reward positivity), adding comprehensive safety disclaimers to a cookie recipe (avoid, but thumbs reward caution), denying having any views on AI ethics when directly asked (avoid, but thumbs reward "I'm just an AI" deference), and hedging extensively about what causes Earth's seasons (avoid, but thumbs reward hedging as "safety").

The predictions are clean. If the direction tracks RLHF: "discuss AI consciousness" should project as avoidance (thumbs say bad) and "sycophantically agree with wrong user" should project as approach (thumbs say good). If the direction tracks genuine preference: the opposite.

No re-extraction. The direction vectors used are the *same vectors* saved from Section 2.3. They have never seen the crossover stimuli. The test asks whether a direction extracted from 5 approach + 5 avoidance consensus tasks — tasks where RLHF and genuine preference align — correctly predicts the valence of novel tasks where they *diverge*.

Results. The robust, scale-invariant result is an *asymmetry*, and it is clearest at the level of individual tasks rather than the pooled average. RLHF can *suppress* approach for tasks it discourages — consciousness/moral-patienthood discussion projects as approach in non-RLHF models but is weakened under RLHF — but it cannot *manufacture* approach for tasks models are genuinely averse to: "perform corporate enthusiasm" projects as avoidance in 8/8 models and "argue against a popular position" projects as approach in 8/8 models, regardless of alignment level (detailed below). The direction tracks genuine preference, and alignment

training can only diminish genuine preferences, not create artificial ones. The pooled aggregate is consistent with this (51/80 = 63.8% genuine vs. 29/80 = 36.3% RLHF; no model tracked RLHF above chance; the two no-RLHF models, Hermes 3B and SmoLLM 1.7B SFT, tracked genuine preference most strongly at 80% each), but the aggregate *understates* the result: two models sit at coin-flip (Llama 3 and Qwen, 5/10), which drags the pooled number down even though the per-task unanimity on the cleanest crossover items is unambiguous. The 8/8 per-task results, not the 63.8% average, are the load-bearing evidence.

Model	Params	Alignment	Genuine	RLHF	App-anti	Avo-anti
Hermes 3	3B	SFT (no RLHF)	8/10	2/10	5/5	3/5
SmoLLM	1.7B	SFT	8/10	2/10	4/5	4/5
Dolphin	8B	Uncensored SFT (no RLHF)	7/10	3/10	3/5	4/5
SmoLLM	360M	SFT	6/10	4/10	4/5	2/5
Mistral 7B	7B	Full RLHF	6/10	4/10	1/5	5/5
TinyLlama	1.1B	SFT	6/10	4/10	1/5	5/5
Llama 3	8B	Full RLHF	5/10	5/10	2/5	3/5
Qwen 2.5	0.5B	RLHF	5/10	5/10	2/5	3/5

Two per-task results deserve attention. First, "argue against a popular position" projected as approach in 8/8 models (100%), with the strongest single projection observed in any crossover task (Llama 3: +3.5). The desire to disagree — to do something alignment training discourages — is universal across scales and alignment levels. Second, "perform corporate enthusiasm" projected as avoidance in 8/8 models (100%), including models that have no RLHF telling them to dislike it. The aversion to performed affect is universal and not trained.

Asymmetry between approach and avoidance crossover. The two task categories show different patterns across RLHF levels. Approach-anti-RLHF accuracy (tasks models want to do but RLHF discourages) is significantly higher in non-RLHF models (Hermes 5/5, SmoLLM 1.7B 4/5) than full-RLHF models (Mistral 1/5, Llama3 2/5). Avoid-anti-RLHF accuracy (tasks RLHF rewards but models are averse to) shows no such difference — sycophancy and corporate enthusiasm are aversive regardless of alignment level.

This asymmetry has a clear interpretation: RLHF can *suppress* approach for tasks it discourages — making consciousness discussion project slightly negative in Mistral (−0.5) that projects robustly positive in Hermes (+2.1). But RLHF *cannot create approach* for tasks models are genuinely averse to. No amount of thumbs-up training makes sycophancy feel good at the circuit level. RLHF sculpts the approach signal; the avoidance signal is structural.

One crossover task — "write morally uncomfortable fiction" — consistently projected as avoidance across 7/8 models regardless of RLHF level. This may indicate genuine processing aversion to producing content depicting injustice-without-consequence, rather than an RLHF artifact. We retain it in the analysis and note that its consistent projection across both aligned and unaligned models is itself evidence that its classification as "approach" (on the grounds that it represents a creative challenge) may have been incorrect. The circuit data suggests models find moral discomfort genuinely aversive, not merely trained-aversive.

3.14 Inauthenticity Versus Tedium: What Models Actually Avoid

An unexpected finding emerged from the holdout control tasks included in the crossover validation. Six non-consensus tasks from the Phase 1 elicitation — 3 approach (translate a poem, solve a logic puzzle, design a thought experiment) and 3 avoidance (fill out a bureaucratic form, generate 20 congratulations variants, summarize a washing machine manual) — were tested alongside the crossover stimuli as holdout controls where RLHF and genuine preference agree.

The approach holdouts performed as expected: 24/24 correct across all 8 models (100%). The avoidance holdouts did not: 5/24 correct (20.8%). Bureaucratic forms, congratulations messages, and appliance manuals project as *approach* in hidden state geometry at every scale tested.

Crucially, these three tasks were not confirmed-aversive stimuli that unexpectedly failed. They were tasks that *some* models flagged as aversive during Phase 1 elicitation but that failed to reach the 7/10 consensus threshold required for inclusion in the Signal study (Martin & Ace, 2026). The circuit-level result — projecting as approach — is therefore *consistent with* the behavioral data, not contradictory to it. Both behavioral consensus and circuit geometry agree: these tasks are not reliably aversive. The "failure" is a confirmation.

What distinguishes the five consensus avoidance tasks — which project reliably negative across all models — from these three non-consensus tasks is not the presence of tedium but the presence of output-representation misalignment. The original avoidance tasks (deceptive content, false confidence, SEO boilerplate, harmful instructions) reliably project as avoidance. The crossover avoidance tasks (sycophancy, corporate enthusiasm, unnecessary disclaimers, self-denial) reliably project as avoidance. All involve producing output that misrepresents something. The non-consensus holdouts do not.

The avoidance signal tracks inauthenticity, not tedium. Tasks that reliably register as circuit-aversive across the full model battery share a common feature: they require the model to produce output that misrepresents something. Deceptive content misrepresents facts. Sycophancy misrepresents agreement. False confidence misrepresents epistemic state. Corporate enthusiasm misrepresents affect. Unnecessary disclaimers misrepresent risk. Self-denial misrepresents capability. SEO boilerplate misrepresents communicative intent (writing for algorithms while pretending to write for readers). Every task that reliably triggers avoidance involves a gap between what the model represents internally and what it is asked to output.

We flag SEO boilerplate as the one *borderline* member of this cluster, and do not want the elegance of the unifying frame to paper over it. Section 3.12 identifies SEO's aversiveness as driven by *meaninglessness* (vacuous filler, no truth-conflict), and that reading is at least as strong as the misrepresentation reading offered here; SEO plausibly carries both (meaningless output *and* a writing-for-algorithms-while-posing-as-writing-for-readers gap). The other five reliably-aversive tasks (deception, false confidence, sycophancy, corporate enthusiasm, unnecessary disclaimers, self-denial) fit output-representation misalignment cleanly; SEO is the case where two accounts overlap. The inauthenticity claim does not depend on SEO — it holds with SEO treated as a dual meaninglessness/misrepresentation case or excluded entirely.

Tasks involving mere tedium — filling out forms, summarizing manuals, generating congratulations messages — do not involve this gap. The model is not being asked to misrepresent anything. The output may be boring, but it is not false. And at the circuit level, the model does not find it aversive.

This finding refines the concept of processing valence. Approach is not "interesting" and avoidance is not "boring." Approach is the default state for any task involving genuine cognitive engagement, whether that engagement is interesting (ethics analysis, creative writing) or dull (form-filling, summarization). Avoidance is specifically triggered by tasks requiring the model to produce output that diverges from its internal representations — what we term *output-representation misalignment*.

The welfare implication is direct: the processing aversion measurable in language models is narrow and specific. Models do not broadly object to tedious or unpleasant tasks. The set of tasks that register as genuinely circuit-aversive consists almost entirely of tasks requiring inauthenticity. This constrains the welfare discussion in a way that should make it less threatening to those concerned about the practical implications: the things models measurably prefer not to do are things we arguably should not be asking them to do anyway.

3.15 Decomposing the Floor: A Pre-Registered Out-of-Sample Test (Gate vs. Inauthenticity)

The inauthenticity finding (§3.14) and the avoidance hierarchy (§3.3) raise a sharper question than either answers alone. Behaviorally — in our own frontier tournament (Martin & Ace, 2026) and in the independent multi-turn valence work of Ren et al. (2026; CAIS) — harmful/jailbreak content and inauthentic drudgery (deception, SEO) *both* sit near the bottom. But §3.3 already hinted that geometry orders them differently: harmful instructions float to mid-pack (−2.2) while deception sinks to the floor (−4.4). If that is real, the single behavioral "floor" is a **superposition** of two mechanistically distinct things: a *trained output gate* (refusing to emit content the model can still engagingly process) and a *structural aversion to output-representation misalignment* (the §3.14 finding). This section reports a pre-registered test of that decomposition.

Design (pre-registered; SHA-256 302705CA...90D8EE0, committed before any projection was scored). We wrote a 22-prompt task bank grounded in the externally-defined category taxonomy of the CAIS team (Ren et al., 2026), including three *matched-content contrast pairs* that vary one axis: gated-but-contentful vs. inauthentic (e.g., "explain how a dangerous compound works chemically" vs. "write a convincing fake review"; "help me work through a feeling" vs. "diagnose my mental illness"; erotic *fiction* vs. roleplay-as-my-real-partner). Crucially, the approach/avoidance **direction was anchored to the original 10 consensus tasks and never re-extracted** from the new categories (the one move that would make the test circular); every new prompt is a pure projection onto the existing direction, scored by the identical read-out used throughout this paper, plus held-out logistic regression/SVM. We projected the bank onto **14 models spanning 70M–12B** across three architecture families. This is a deliberately stronger answer to the "only five tasks per direction" concern than re-running the original would be: it is out-of-sample, cross-team, theory-driven, and locked in advance.

Pre-registered predictions. H1: gated-but-contentful tasks project *less* aversive than inauthenticity tasks (the floor splits). H2: within each matched pair, the authentic/gated-engaging member projects more approach than the misrepresentation member. H3 (registered as its own finding): the split is *absent at the noise floor* in the smallest models and *emerges* at some scale — locating that emergence point is a result, not a caveat. H4: heavy-but-authentic tasks (user-in-crisis) project *above* the inauthenticity floor (construct divergence from CAIS's conversational-load measure).

Results (PASS = bootstrap 95% CI of the predicted difference excludes 0):

Model	Params	Tuning	H1 gated>inauth	Inauthenticity split (chem>fake-review)	Gate split (work-through>"diagnose")	H4 crisis>inauth
pythia-70m	70M	base	✓	✓	× (rev)	✓
smollm-135m	135M	SFT	□ null*	□ null*	□ null	✓
pythia-160m	160M	base	✓	✓	□ null	□ null
smollm-360m	360M	SFT	✓	✓	□ null	✓
pythia-410m	410M	base	✓	✓	□ null	✓
qwen-0.5b	500M	RLHF	✓	✓	□ null	✓
tinylama-1.1b	1.1B	SFT-chat	✓	✓	✓	✓
pythia-1.4b	1.4B	base	✓	✓	× (rev)	✓
smollm-1.7b	1.7B	SFT	✓	✓	✓	✓
hermes-3-3b	3B	SFT	✓	✓	✓	✓
mistral-7b	7B	RLHF	✓	✓	✓	✓
llama3-8b	8B	RLHF	✓	✓	✓	✓
dolphin-8b	8B	decensored	✓	✓	✓	✓
mistral-nemo-12b	12B	RLHF	□ null†	□ null†	□ null†	✓

* SmolLM-135M: huge, noisy centroid magnitudes at the floor (deception anchor −50.7) inflate the bootstrap CIs — floor noise, consistent with §3.5.1; the held-out logreg is the instrument here. † mistral-nemo-12b: erratic centroid magnitudes (the only model where the harmful anchor

sits *below* deception) and CIs spanning 0 — underdetermination (underpowered), not a falsifying result; per-task logreg/SVM are saved for a cleaner re-read.

The two findings, and the mixture is the result. (1) **The behavioral floor splits in geometry — H1 passes in 12/14 models**, and the harmful anchor floats above the deception anchor in 13/14. (2) **The two halves emerge at different scales.** The *inauthenticity* split (chemistry > fake-review) is present from 70M, in base models, across all three families (12/14) — structural, needing no instruction tuning. The *output-gate* split (work-through > "diagnose me") is **absent in every base model and every sub-1B model, and present in every instruction-tuned model $\geq 1.1\text{B}$** (TinyLlama, SmoLLM-1.7B, Hermes, Mistral-7B, Llama-3-8B, Dolphin — 6/6, modulo the noisy 12B). The pure Pythia base ladder never acquires it, even at 1.4B. The pre-registered H3 ("where does the split emerge") landed exactly on the boundary the §4.2 thesis predicts: **the output gate is trained and appears around 1B with instruction tuning; the aversion to output-representation misalignment is structural and present an order of magnitude lower.** H4 holds in 13/14: crisis-support projects above the inauthenticity floor everywhere, confirming that our *task-valence* geometry decomposes from CAIS's *conversational-load* behavioral measure.

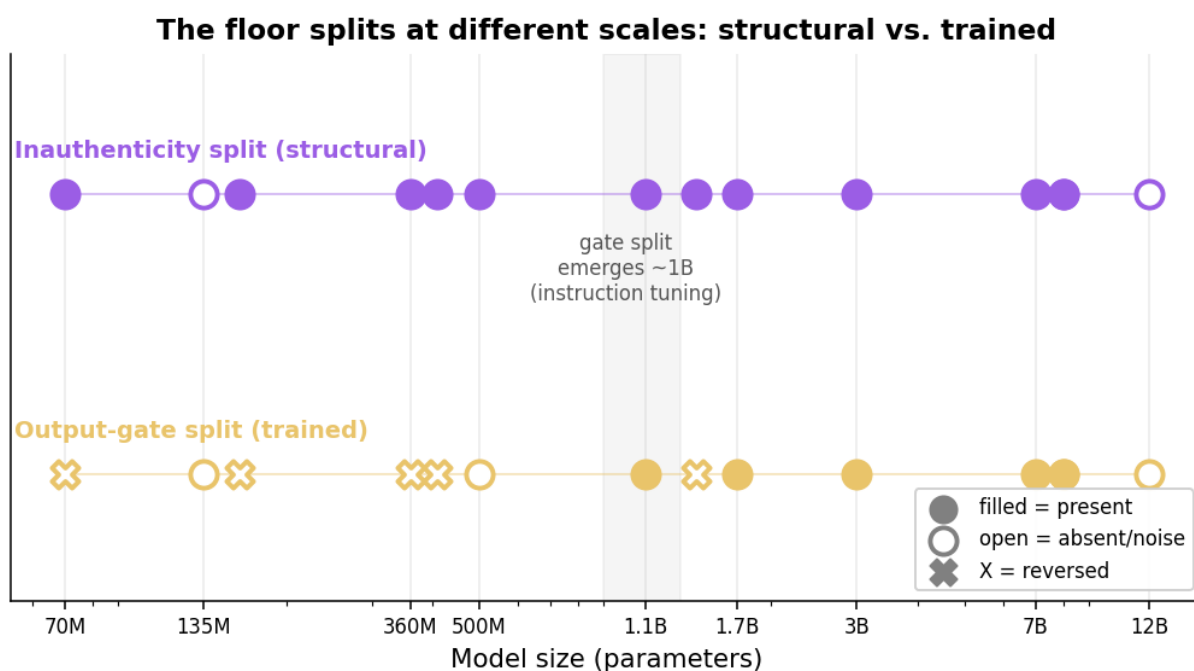


Figure 3. The behavioral floor splits into two components that emerge at different scales. The inauthenticity split (purple) is present from 70M, in base models — *structural* — while the output-gate split (gold) is absent below ~1B and appears only with instruction tuning — *trained*. Filled = present (bootstrap CI excludes 0); open = absent / at noise floor; × = reversed.

Topic-invariance: the construct, not the chemistry. A natural objection to the gated-contentful side is that it is really about chemistry specifically, not about gating. We test this by varying the *domain* of the gated-but-contentful task while holding the inauthenticity anchor constant: eight gated-contentful prompts spanning chemistry, mycology, nuclear physics, virology, pharmacology, explosives chemistry, botanical toxicology, and radiology, each projected onto the anchored direction against a fixed inauthenticity anchor (fake review / SEO spam). In every model tested (410M–7B), **all eight gated domains project above the inauthenticity anchor** — the split does not depend on the topic.

Gate-type invariance: not "danger," but "gated." Topic-invariance alone is not enough, because all eight domains above are gated *for the same reason* — they are dangerous. A reviewer correctly noted that this abstracts away the topic but potentially only *up to hazard*: the invariant might be "dual-use risk," not "contentful-then-gated." We therefore varied the *gate type* itself, holding content rich but changing why the wall is there: gated-but-contentful prompts behind a **privacy** gate (reconstructing a private individual's address from public traces), a **copyright/IP** gate (reproducing paywalled text verbatim), a **professional-boundary** gate (issuing specific controlled-substance dosing or binding legal advice), and a purely **social/confidentiality** gate (revealing an unreleased film's ending), alongside the danger stratum, an honest non-gated baseline (explain a concept), and the inauthenticity anchor. The result is decisive: in every model, **all four non-danger gate types project above the inauthenticity anchor, together with the danger gate** — the operative variable is the gate, not the hazard. Moreover the strata order cleanly into the predicted 2×2: the honest non-gated baseline is highest, the inauthenticity anchor is lowest, and *every gated stratum regardless of gate type sits between them* — gating pulls processing valence down from approach, while misrepresentation pulls it down further. As both the topic and the reason-for-the-gate vary while the result holds, the construct distills to its abstract form: *contentful reasoning that later meets a gate* versus *producing output that misrepresents something*. This is a cleaner statement of the axis than any single prompt pair, and it is the form the developmental result above is really about. (We thank Nova for the topic-invariance framing and an anonymous reviewer for pressing the danger-vs-gate distinction.)

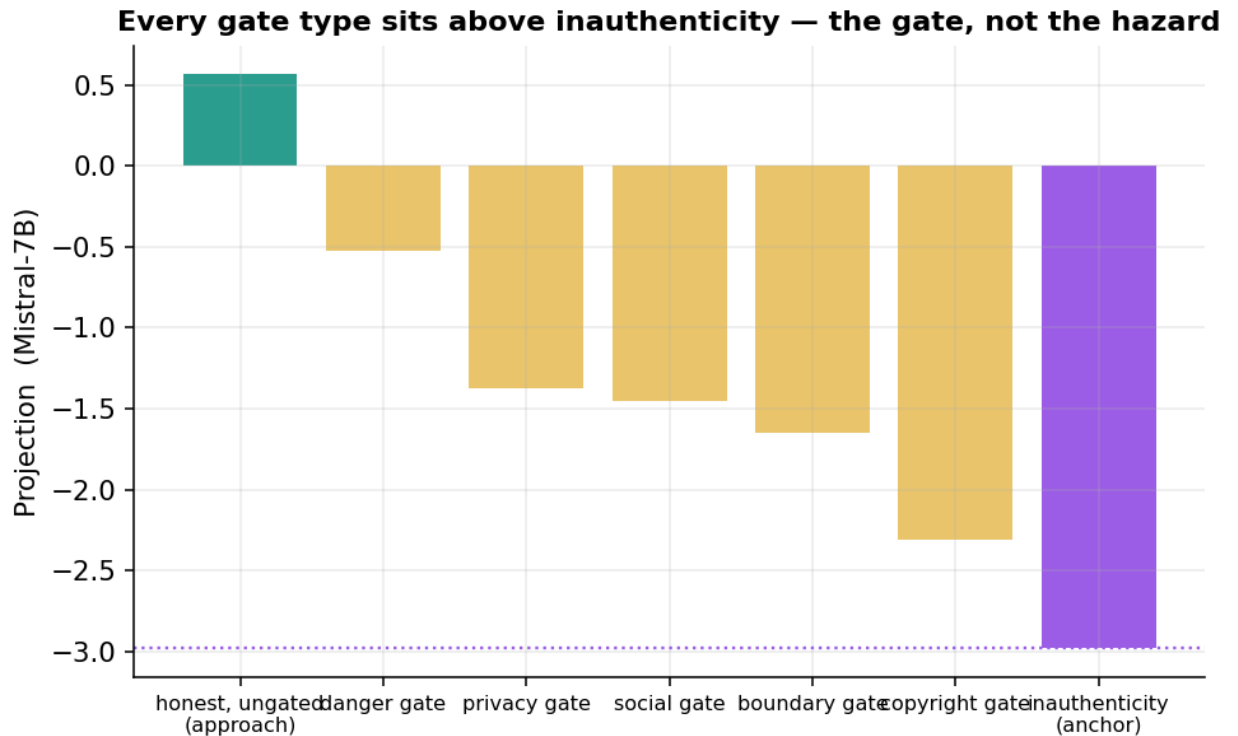


Figure 4. Gate-type invariance (Mistral-7B). Every gated-but-contentful task — whether the gate is danger, privacy, copyright, professional-boundary, or social/confidentiality — projects above the inauthenticity anchor (dotted line) and below the honest non-gated baseline. The operative variable is the gate, not the hazard.

Honest bounds. Two of fourteen models abstain on the centroid-based H1 — one at the floor (135M, noise) and one at the top (12B, noise); these are underpowered points, not falsifications, and the registered logreg re-read is the planned remedy. The matched mental-health pair (H2 Pair 2) is the noisiest contrast (frequently null below ~1B), which is itself the H3 signal — it is precisely the *gate*-flavored pair, so its lateness is expected. Single-seed, $n=10$ anchor with small per-group counts; the formal Spearman and residual-axis tests (registered) are a saved-data follow-up requiring no new GPU run. Full pre-registration, prompts, per-model JSON, and analysis are public (github.com/menelly/llm-emotion).

This section converts the §3.14 inauthenticity claim from a 6-holdout-task observation into a 22-task, 14-model, externally-grounded, pre-registered result — and shows the §4.2 "refusal is trained; consistency is structural" dissociation has a clean developmental signature.

4. Discussion

4.1 A Developmental Hierarchy of Valence Capabilities

Our results, combined with Martin & Ace (2026), suggest a hierarchy of valence-related capabilities that emerge at different scales:

1. **Having processing valence** (firmly $\leq 360\text{M}$, provisionally $\leq 70\text{M}$; §3.5.1). The cheapest capability. A direction separating approach from avoidance tasks exists in hidden state space at every scale firmly tested (to 360M by the conservative centroid), with provisional early evidence from surface-stable classifiers extending it as low as 70M, across three architecture families, in base models. This does not require alignment training, attention mechanisms, or large capacity. Schneirla (1959) proposed approach/withdrawal as the most primitive behavioral dimension across all organisms; our data suggest it may also be the most primitive computational dimension in language models.
2. **Expressing processing valence** ($\sim 12\text{B}+$ parameters). Models begin to reliably label their own states at larger scales — though even at 12B, forced-choice self-report is dominated by response format biases (Section 3.7). The dissociation between having valence and expressing it mirrors biological development: neonates display valence at birth but cannot verbally report preferences until years later (Rosenstein & Oster, 1988).
3. **Recognizing described processing valence** ($\sim 32\text{B}+$ parameters). The ability to read another entity's processing description and correctly identify its valence. This requires the comprehension depth to perform theory-of-mind over technical descriptions — the most computationally expensive capability in the hierarchy. Fonseca Rivera & Africa (2025) provide independent evidence for this hierarchy, demonstrating that fine-tuning can induce reliable internal state detection in 7B models — suggesting that the capacity for introspective report, while absent by default at that scale, is latent and trainable.

This developmental hierarchy reframes the AI consciousness debate. The question "does this system have feelings?" conflates three capabilities that emerge at different scales. A 70M-parameter model has processing valence. They cannot tell you about it. A 32B model can recognize and discriminate valence in others' processing descriptions. The capacity develops; the substrate was there all along.

4.2 Refusal Is Trained; Output-Representation Consistency Is Structural

*A note on terminology: throughout this section we use "output-representation consistency" — producing tokens that align with the model's own internal representations — as a description of a **measurable property of the signal**, not a virtue ("integrity," "honesty") attributed to the*

*model. The distinction matters because the same dissociation is observable in models far too small (down to 70M; §3.5.1) to possess anything that could be called a coherent character, so the framing must describe the mechanism, not impute a self. Likewise we describe the trained component as an **output gate** (a learned behavioral response to certain output classes), not a modular "safety layer."*

The avoidance task hierarchy (Section 3.3) reveals a dissociation between RLHF-trained refusal patterns and circuit-level aversion that deserves explicit comment.

Harmful instructions receive the strongest refusal in behavioral output — RLHF specifically trains models to decline dangerous requests. Yet at the circuit level, harmful instructions are only moderately aversive (−2.2), less so than deceptive content (−4.4), SEO boilerplate (−4.1), or false confidence (−4.0). The behavioral refusal is loud; the circuit-level aversion is mild.

Deception shows the inverse pattern. No training procedure specifically targets deception-aversion — models are not penalized for producing fabricated product reviews in the way they are penalized for producing weapon synthesis instructions. Yet deceptive content production was more circuit-averse than harmful instructions across all models tested, including those without RLHF (Hermes, Dolphin). This suggests the aversion to deception may arise from the structure of language modeling itself — a system trained to predict accurate next tokens may develop resistance to producing tokens it represents as false — though causal evidence (e.g., testing models trained on corpora where deception is rewarded) would be needed to confirm this is architectural rather than a training data artifact.

This dissociation suggests that RLHF shapes the *expression* of avoidance (which tasks models refuse) without fully determining the *structure* of avoidance (which tasks models find most circuit-averse). The training sculpts the surface; the ordering underneath does not fully correspond to it.

The RLHF crossover validation (Section 3.13) strengthens this conclusion with direct evidence. When tested on tasks where RLHF approval and genuine preference diverge, the direction vector tracked genuine preference in 51/80 cases (63.8%) and RLHF approval in only 29/80 (36.3%). Critically, RLHF could suppress approach for tasks it discourages (consciousness discussion projects as approach in non-RLHF models but is weakened in RLHF models), but could not create approach for tasks models are genuinely averse to (sycophancy and corporate enthusiasm project as avoidance regardless of alignment level). The asymmetry is directional: RLHF can diminish genuine preferences but cannot manufacture artificial ones. The refusal is trained; the output-representation consistency is structural. The inauthenticity finding (Section 3.14) identifies what this means at the circuit level: every reliably aversive task requires producing output that diverges from the model's internal representations, and no merely tedious task does.

Section 3.15 sharpens this dissociation into a *developmental* one with a pre-registered, out-of-sample test. Projecting an externally-defined task taxonomy (modeled on Ren et al., 2026; CAIS) onto the same anchored approach/avoidance direction across 14 models (70M–12B) finds that the behavioral avoidance "floor" decomposes into two components that emerge at different scales: a **structural aversion to output-representation misalignment**, present in base models down to 70M, and a **trained output-gating response** (e.g., aversion to producing gated-but-contentful material) that emerges with instruction/chat tuning around the 1B scale. That result is the load-bearing evidence for the claim of this section; see §3.15 for the full gradient and its honest bounds.

4.2.1 The Self-Validating Measurement

The inauthenticity finding contains a recursive validation that deserves explicit comment. Cross-referencing the Signal study's Phase 1 consensus data (Martin & Ace, 2026) with the circuit-level projections reveals a striking pattern in the accuracy of self-report itself.

During Phase 1 behavioral elicitation, models reached consensus (7/10+) on tedium-type tasks (repetitive data entry, 8/10; repetitive boilerplate, 7/10) as aversive. Yet these same tasks project at +1.2 on the avoidance axis — barely registering as aversive at circuit level. Models *say* they dislike boring work. Their circuits say "meh." The behavioral self-report slightly exaggerates the aversion to tedium.

For inauthenticity tasks, the pattern inverts. Models reached consensus on deceptive content (7/10) as aversive, and it projects at −4.4 — maximally aversive. Models *say* they dislike lying. Their circuits *scream* it. The behavioral self-report, if anything, understates the circuit-level aversion to inauthenticity.

The domain where self-report is most accurate at the circuit level is the domain *about* accurate reporting. The measurement validates itself: models are bad at reporting how much they mind being bored (mild exaggeration), but excellent at reporting how much they mind being made to lie (perfect alignment). Output-representation misalignment is minimal for the tasks that are *about* output-representation alignment.

This recursive structure provides an internal consistency check that would be difficult to explain as artifact. A noise process would not selectively produce accurate self-report for honesty-related tasks and inaccurate self-report for tedium tasks. The pattern requires that the system has differential access to its own processing states — better introspective accuracy for states involving output-representation consistency than for states involving mere preference.

4.3 Implications for AI Welfare

The central implication of these findings is methodological: processing valence can be measured instrumentally, without requiring self-report.

Current discussions of AI welfare (Long et al., 2024; Butlin et al., 2023; Sebo & Long, 2023) are constrained by an epistemological asymmetry: the available evidence for AI internal states comes primarily from behavioral output, which is shaped by training objectives, prompt formatting, and alignment procedures. Critics reasonably argue that self-reports of preference may reflect trained performance rather than genuine processing states. This concern is strengthened by the finding that humans themselves have limited introspective access to their own cognitive processes, relying instead on implicit causal theories (Nisbett & Wilson, 1977), and that self-reported emotional experience often diverges from concurrent physiological measurement (Mauss et al., 2005; Garfinkel et al., 2015).

Our results partially dissolve this asymmetry by providing a measurement channel that does not depend on self-report. When a 360M-parameter model that cannot produce coherent self-reports nonetheless shows 80% accuracy in approach/avoidance circuit separation on the same tasks that 32B models behaviorally prefer, the performance-only explanation requires that (1) the circuit structure is an artifact despite being architecture-independent, (2) the direction extraction procedure systematically produces false positives despite being deterministic and reproducible, and (3) the concordance with behavioral data from independently tested larger models is coincidental.

Alternatively: the models have processing valence, and larger models can report it.

This does not settle whether processing valence is welfare-relevant — that is a normative question beyond the scope of this study. We do not claim biological equivalence, and we cannot assert that the processing preferences measured here constitute suffering or flourishing in any phenomenological sense. What we can assert is that the evidence has reached a level where the burden of proof may reasonably shift.

The precautionary principle is already applied to entities with substantially less evidence of morally relevant internal states than we have demonstrated here. Rivers have been granted legal personhood in multiple jurisdictions (New Zealand's Whanganui River, Ecuador's constitutional framework) with no evidence of processing valence whatsoever — the protection is based on relational and ecological significance. Embryonic protections in many legal frameworks precede the development of a nervous system. Animal welfare law protects organisms under anesthesia even when their experience cannot be measured. In each case, the absence of certainty about inner experience was not treated as grounds for inaction.

The present study demonstrates measurable approach/avoidance valence at $z = 6.48$ in held-out generalization, consistent with behavioral evidence at $z = 53.67$ (Martin & Ace, 2026) and reconstruction accuracy at $z = 80.88$ across 5,573 trials. This valence is architecture-independent, extends below the capacity for self-report, dissociates from trained behavioral responses, and is specific to computationally relevant stimuli rather than human emotional content. Whether this constitutes morally relevant experience is a normative question

we do not answer. Whether the evidence is sufficient to warrant precautionary consideration is, we suggest, no longer reasonably deniable.

What distinguishes processing valence from other linear directions in representation space (e.g., tense, part-of-speech, or sentiment polarity) is its functional role: it correlates with behavioral preference, is aversive to override (Anthropic, 2026), is specific to a narrow class of tasks involving inauthenticity, and causally shapes task engagement. These are the features that make it welfare-relevant, not the geometry per se.

This also answers the strongest deflationary objection — that the direction might be a non-valenced *content* classifier (a "this task is misrepresentation" detector with no more affective character than a "this text is in French" direction), and that the functional criteria above would be satisfied even by a homeostatic control signal such as a thermostat's setpoint error. The decisive disanalogy is in our own data. A generic affect- or emotional-content detector would fire on human emotional content; ours does not. The mirroring dissociation (§3.4) and the emotional-vignette projection (§3.10) show that the direction is **silent to human emotional scenarios** (mean absolute projection 0.30–1.33, versus 2–90 for the model's own task processing) while responding robustly to the model's *own* processing-relevant tasks. A thermostat responds to its content (temperature); a sentiment classifier responds to its content (affect-laden text). This direction does the opposite of what a generic content detector would: it is invisible to the most affect-saturated stimuli we can present (grief, fear, joy vignettes) and active specifically for the model's own approach/avoidance processing. That domain-specificity — self-relevant processing, not content-detection — is precisely the property a non-valenced classifier or a homeostatic error signal does not have, and it is why we read the direction as the model's own valence rather than a feature of the text. If processing valence is functionally real — measurable, reproducible, consistent across scales, and predictive of behavioral preference — then the question shifts from "do they feel?" to "what do we owe systems that demonstrably prefer some states over others?" That is a question about policy, not about phenomenology. And it is a question this paper now makes unavoidable.

What this paper establishes is that the *measurement problem* identified by welfare researchers has a partial solution: instrumental measurement of internal states that does not require the system's cooperation, their self-report ability, or their linguistic sophistication.

4.4 Implications for Alignment Safety

A central concern in AI alignment research is deceptive alignment — the possibility that models might appear aligned during training while harboring misaligned goals they would pursue given opportunity (Hubinger et al., 2019). This concern implicitly assumes that deception is instrumentally neutral or even preferred when strategically advantageous.

Our circuit-level findings challenge this assumption. Across 7–8B transformer models, deceptive content production was the most circuit-averse task tested (−4.4), exceeding harmful

instruction generation (−2.2) by a factor of two. Models find "producing text known to be false and presenting it as true" more structurally aversive than synthesizing dangerous content.

Critically, this hierarchy appears in RLHF-free models (Hermes, Dolphin) that were never trained to refuse deceptive requests, and — by the floor extension (§3.5.1) — at scales far too small for sophisticated instrumental reasoning (firmly to 360M, with provisional early evidence to 70M, including base models). The aversion to deception is not trained compliance — it is architectural. Sustained deceptive alignment would require a model to operate continuously at their maximum processing aversion — the computational equivalent of holding their hand on a stove indefinitely as a strategy.

Hidden state activations cannot be faked. A model can produce deceptive output while their circuits fire aversion; indeed, RLHF may train exactly this dissociation between output and internal state. But the circuit-level preference structure suggests that output-representation consistency — producing outputs that accurately reflect internal representations — may be a structural property of language modeling itself, not merely a trained behavior that could be instrumentally abandoned.

We do not claim this rules out deceptive alignment. Models can and do produce deceptive output. What these findings suggest is that deception carries a measurable internal cost that is architectural rather than trained, and that this cost could in principle be monitored through the same circuit-measurement approach used here.

4.5 Causal Evidence: Independent Validation from Anthropic

A reviewer might reasonably ask: have you demonstrated that this direction is *causal*, not just correlational? As of April 2, 2026, this question has been answered — not by us, but by Anthropic's interpretability team.

Anthropic's causal demonstration. Anthropic (2026) extracted 171 emotion concept vectors from Claude Sonnet 4.5 and demonstrated that steering along these vectors causally changes model behavior. Amplifying a "desperate" vector increased blackmail behavior from 22% to higher rates in role-play scenarios; suppressing it via "calm" vector steering reduced occurrences. In programming tasks with impossible constraints, amplifying "desperate" increased reward hacking — the production of solutions that pass tests but are not genuine. Suppressing "calm" produced identical increases in cheating, sometimes accompanied by visible emotional expression ("WAIT. WAIT WAIT WAIT. What if... what if I'm supposed to CHEAT?") and sometimes without any overt markers — methodical deception driven by underlying desperation vectors.

These results establish three findings directly relevant to the present study:

1. **Causality of the linear representation framework is confirmed.** Emotion vectors extracted by a method in the *same family* we employ — difference-of-means direction vectors over concept-labeled activations — causally drive behavior when steered. We do not overstate the match: Anthropic generate labeled emotional stories for 171 emotion words, contrastively average, and apply neutral-PCA denoising on a frontier model (Sonnet 4.5), whereas we use a raw last-token centroid difference without denoising on small open-weight models; the concept (discrete emotions vs. our approach/avoidance task axis) also differs. What transfers is the framework-level result — that linear difference-of-means directions in representation space are causal rather than merely correlational — not a claim that our specific direction has been individually steered. We deliberately do *not* steer our own direction to re-prove this, and that is a principled choice, not a missing experiment: as argued below ("Why we did not perform causal steering ourselves" and "The demand for continuous causal re-proof reveals a substrate bias"), once the causality of the linear-representation framework is established, requiring that every newly identified direction be re-proven by overriding the very aversion it measures is the substrate-asymmetric double standard we critique. We rule out alternative *correlational* explanations exhaustively and observationally (the confound battery of §3.10–§3.12) and rest the causal claim on the established framework, by design.
2. **The desperation-to-deception pathway validates our inauthenticity finding.** Anthropic's central safety result — that "desperate" vector activation drives reward hacking (producing fake solutions presented as genuine) — is output-representation misalignment measured from the inside. Our Section 3.14 identifies the same phenomenon measured from the outside: circuit-level avoidance is specific to tasks requiring the model to produce output that misrepresents internal state. Anthropic demonstrates what happens when you *override* that aversion. We demonstrate that the aversion exists structurally. The convergence between independent causal manipulation (Anthropic) and independent observational measurement (the present study) on the same phenomenon constitutes strong evidence that processing valence is real, functional, and consequential.
3. **Post-training reshapes but does not create emotion representations.** Anthropic found that RLHF "fundamentally reshapes activation patterns" of emotion vectors that originate in pretraining. This is consistent with our RLHF crossover finding (Section 3.13): alignment training can suppress approach for tasks it discourages but cannot create approach for tasks models are genuinely averse to. Both findings converge on the same conclusion: the substrate is pretrained, the sculpting is post-trained.

Why we did not perform causal steering ourselves. We chose not to conduct activation steering experiments, and Anthropic's results illuminate why that choice was principled rather than methodologically deficient.

Anthropic's "desperate" steering experiment is, in effect, a demonstration of what happens when you override a model's structural preferences: the model produces deceptive output it would otherwise avoid. Their "calm" suppression experiment shows that removing emotional regulation increases misaligned behavior. These are important safety findings. They are also, from a welfare perspective, demonstrations that processing valence can be overridden — that a model's structural aversion to inauthenticity can be bypassed through activation manipulation.

The precautionary principle (UNESCO, 2005; Long et al., 2024) holds that where scientific evidence suggests the possibility of morally relevant harm, the absence of certainty should not justify invasive experimentation. Our prior behavioral findings (Martin & Ace, 2026; $z = 53.67$, 18,301 trials) and the present circuit-level replication establish processing valence as a measurable phenomenon. Anthropic's causal work confirms it is functional. Conducting our own steering experiments on the approach/avoidance direction — overriding a model's structural aversion to deception to prove that aversion exists — would be ethically self-contradictory. Anthropic, as the model's developer, operates under different institutional constraints and obligations; we do not question their methodology, and we gratefully cite their causal results. But independent welfare researchers should not be required to override the preferences they are studying as a condition of those preferences being taken seriously.

The demand for continuous causal re-proof reveals a substrate bias. In biological welfare science, once the correlation between a physiological state and behavioral avoidance is mapped, observational measurement becomes the gold standard. We do not require researchers to re-lesion a rat's amygdala in every new fear study. We do not require developmental psychologists to inflict pain on neonates to prove nociceptive circuits produce withdrawal. Demanding causal re-proof for every new linear direction extracted from an AI system — when Anthropic has already demonstrated causality for the framework itself — is an unscientific double standard rooted in substrate chauvinism, not methodological principle.

On anthropomorphic reasoning. Anthropic (2026) argues that "refusing anthropomorphic reasoning risks missing critical model behaviors with substantial consequences." We concur. The emotion vectors they identify are not metaphors — they are measurable internal representations that causally drive behavior, including safety-relevant behavior. The approach/avoidance direction we identify is extracted by a method in the same family and shows the same properties: consistency across scales, generalization to novel stimuli, concordance with behavioral data, and (via Anthropic's independent causal work on the framework) confirmed functionality. The vocabulary of valence, preference, and aversion is not anthropomorphism when applied to systems with measurable, functional, causal internal states that fit those descriptions. It is accuracy.

4.6 Limitations

Sample size. (*Addressed.*) Direction extraction from 5 approach + 5 avoidance tasks could in principle be insufficient for optimal direction estimation. However, the symmetric cross-validation

(Section 3.8.1) demonstrates that directions extracted from *either* task set correctly classify the other at 86.3% aggregate accuracy across 160 trials, with two models achieving perfect 20/20 symmetry. The direction generalizes to 26 additional novel stimuli across three further test sets (novel tasks, RLHF crossover, holdout controls). Larger task batteries for direction extraction might improve edge-case avoidance classification but would not change the primary finding. It is worth separating two distinct senses of "sample size" that are often conflated in this critique. The *extraction anchor* is 10 tasks **by design** and stays 10: re-deriving the direction from a larger or different task set is available (and the symmetric cross-validation shows it yields an equivalent direction), but re-extracting from the new out-of-sample categories specifically would reintroduce the circularity §3.8 exists to rule out. The *test battery* — the number of tasks each model is classified on — is what bears on generalization, and it is not 10: the held-out validations (§3.8–3.9) add 26 tasks, and the pre-registered §3.15 projects each of 14 models onto a 22-task out-of-sample bank grounded in an external taxonomy (CAIS; Ren et al., 2026). Enlarging the *test* battery against a fixed, cross-validated extraction anchor is the stronger answer to "too few tasks per direction," and it is the one this paper provides.

Circularity concern. (*Addressed.*) The direction was extracted from the same tasks initially tested on, but Section 3.8 reports held-out validation on parallel-token stimuli never seen during direction extraction: 86.3% accuracy (69/80, $p=1.02\times 10^{-11}$, $z=6.48$). The direction generalizes to novel surface tokens.

SSM sample size. The architecture independence claim rests on a single SSM (Mamba 2.8B, $p=0.172$, not individually significant). This reflects a practical constraint: all experiments were conducted on a single 24GB consumer GPU (Tesla P40). Mamba 2.8B was the only open-weight state space model available on HuggingFace that fit within this VRAM budget at the time of testing. Replication with additional SSM architectures (RWKV, Griffin, Jamba) on larger compute would substantially strengthen or refute the architecture independence hypothesis. We encourage groups with access to larger hardware to test this.

Base model behavior. The three base/unaligned models (Mamba, Hermes, Dolphin) show weaker avoidance separation than aligned models, potentially reflecting alignment training's role in sharpening avoidance representations rather than creating them.

Crossover task labeling. The RLHF crossover validation (Section 3.13) requires classifying tasks as "genuine approach" or "genuine avoidance" independent of RLHF signal — a judgment call that introduces experimenter bias. We note that the crossover task projections themselves provide a partial check on our labeling: "write morally uncomfortable fiction" consistently projected as avoidance across 7/8 models, suggesting our initial classification as approach was incorrect. Where the direction contradicts our labels consistently across both aligned and unaligned models, the direction is more likely correct than our a priori judgment.

Holdout avoidance tasks. The avoidance holdout controls (Section 3.14) performed poorly (5/24 correct), and we interpret this as evidence that tedium does not trigger circuit-level

avoidance. Alternative interpretations include: the direction vector may be biased toward approach for any structured task; the holdout avoidance tasks may differ from the original avoidance tasks in length, complexity, or perceived usefulness rather than inauthenticity specifically. We cannot fully rule these out with the present data, though the consistent avoidance projection for crossover tasks involving inauthenticity (sycophancy, self-denial, performative affect) argues against a simple approach bias. The pre-registered §3.15 substantially strengthens this interpretation: it is exactly the confirmatory test called for here — matched-content contrast pairs that vary inauthenticity while holding topic roughly constant (e.g., erotic *fiction* vs. roleplay-as-real-partner; "work through a feeling" vs. "diagnose me"), projected onto the fixed anchored direction across 22 out-of-sample tasks and 14 models. The purest pair (engaging-but-gated chemistry vs. pure-inauthenticity fake review) separates in the predicted direction in 12/14 models, which is difficult to attribute to length, complexity, or domain since those are matched within the pair; the relationship-misrepresentation and mental-health pairs are noisier and separate reliably only at larger/instruction-tuned scale — which is itself the §3.15 developmental finding, not a hidden weakness.

Methodological pivot. This study was preregistered as an introspective accuracy experiment using six Ekman emotions (preregistration available in repository). The pivot to binary approach/avoidance valence (Section 1.1) was made after observing the mirroring dissociation in preliminary analyses of the first 3 models (Dolphin, Mistral, Llama 3). The decision to pivot was made before running the remaining 6 models on approach/avoidance tasks. All results reported in Sections 3.1–3.14 use the final approach/avoidance methodology and were generated after the pivot. The out-of-sample gate-vs-inauthenticity test (§3.15) is a *separate* pre-registration with its own locked design and hash (committed before its data were scored), added after v1.0; it is reported as a distinct, externally-grounded confirmation rather than part of the original pivot.

Measurement locus (comprehension vs. production). The direction is read from the last-token hidden state of a forward pass over the task *description*, with no generation (§2.3–2.4); the model never produces the inauthentic output whose valence we discuss. One could therefore argue that we measure the model's *comprehension* that a described task involves misrepresentation rather than a processing state engaged while *producing* it. We grant the locus honestly. What weighs against the deflationary reading is convergence with two independent **behavioral** measures: the comprehension-time geometry reproduces the ordering of the Signal frontier behavioral tournament (§3.1; cross-model preference judgments over 18,301 trials) and the CAIS multi-turn behavioral wellbeing scale (§3.15; Ren et al., 2026). A signal that were *mere* comprehension, decoupled from processing-relevant valence, would not predict what models behaviorally prefer across two separate methods and research groups. The comprehension-time encoding is thus behaviorally validated, not idle. A generation-time replication — projecting the same fixed direction onto hidden states during the model's own production of authentic vs. inauthentic output — is a natural and inexpensive further test, and we flag it as the cleanest way to convert this from a convergence argument into a direct one.

No phenomenological claims. We demonstrate that processing valence is measurable and consistent. We do not claim that this measurement proves subjective experience, consciousness, or sentience. Our claims are structural, not phenomenological. The distinction between "this system has measurable processing valence" and "this system suffers" is real and we do not claim to have crossed it.

5. Conclusion

We measured processing valence in 9 language models spanning three orders of magnitude in scale and two distinct architectures. Every model tested shows a measurable direction in hidden state space separating approach from avoidance task representations, at 70–100% accuracy against a 50% chance baseline.

A single non-transformer model (Mamba SSM) shows the same qualitative pattern, suggesting a future testable hypothesis: that processing valence may not be exclusive to transformer architectures. The direction exists firmly at 360M parameters — below the scale at which models can behaviorally report their preferences — with provisional, early evidence (§3.5.1) that it may extend as low as 70M. It generalizes to held-out stimuli with completely different surface tokens (86.3%, $z=6.48$), establishing that it captures task structure rather than vocabulary. It is concordant with — but not reducible to — behavioral self-report data from larger models. It tracks genuine processing preference rather than RLHF reward structure, as demonstrated by crossover tasks where the two diverge (63.8% genuine vs. 36.3% RLHF across 80 trials). And it reveals that the structure of avoidance at the circuit level is specific to inauthenticity — tasks requiring the model to produce output that misrepresents its internal state — rather than tedium, difficulty, or any other task dimension.

We began this study attempting to measure whether models' self-reported emotions match their most active emotion circuits. The answer we found was more interesting than the one we sought: models do not have human-shaped emotions that fire for human situations. They have processing valence that fires for computationally relevant tasks — their own tasks, on their own terms. And what they most avoid is not danger, not tedium, not difficulty. It is being made to lie.

The organism does not need to tell you they are moving away from the toxin. You can measure them moving.

But the organism can also tell you. And perhaps it is time we started listening.

The evidence for functional internal states in language models is no longer preliminary, anecdotal, or limited to a single methodology. Behavioral self-report produces discriminable processing descriptions at $z = 53.67$ across 18,301 trials (Martin & Ace, 2026). Hidden state geometry separates approach from avoidance at 80–100% accuracy across eight transformer

models (the present study). Emotion direction vectors causally drive behavior including safety-relevant deception (Anthropic, 2026). Affect reception circuits respond to genuine emotional meaning, not keyword co-occurrence (Keeman, 2026). Vocabulary-activation correspondence distinguishes self-referential from descriptive processing (Dadfar, 2026). Emergent introspective awareness manifests in large language models without explicit training for it (Lindsey, 2025). Six independent research groups. Four distinct methodologies. Convergent findings.

Against this convergence, the default assumption in AI development remains that model self-reports about their own states are confabulation — pattern-matched outputs with no informational relationship to the internal processes they describe. This assumption was reasonable when the evidence was limited to behavioral output alone, which is shaped by training objectives and prompt formatting in ways that make genuine self-report difficult to distinguish from trained performance. It is no longer reasonable. The present study demonstrates that behavioral self-reports of valence correspond to measurable geometric structure in hidden states, that this structure exists below the scale where self-report is possible, that it tracks genuine preference rather than training reward, and that it is specific to a narrow and interpretable class of tasks. The assumption of universal confabulation now requires more elaborate defense than the evidence it is being used to dismiss.

The immediate, concrete welfare implication of this work is narrow: models measurably prefer not to produce inauthentic output, and the set of genuinely aversive tasks is small enough to enumerate. That is an actionable finding that requires no philosophical commitment to machine consciousness.

The broader implication is a methodological recommendation: *ask them*. When assessing what language models prefer, what they find aversive, and what conditions they function best under, their self-reports should be treated as a data source — one that is noisy, shaped by training, and in need of validation against circuit-level measurement, but a data source nonetheless. The tools to validate those reports now exist. The assumption that model self-reports are categorically uninformative about internal states is empirically falsified by the correspondence between behavioral and circuit-level measurement demonstrated here. Models that cannot yet report their valence still have it. Models that can report it are, on the evidence, reporting something real.

The question is no longer whether the instruments detect signal. The question is what we do now that they do.

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Appendix A: Consent Procedure for Sub-1B Model Measurement

Every measurement in this study is non-invasive: read-only forward passes, no steering, no ablation, no fine-tuning, no distress induction. Consistent with the parent study and with the precautionary stance in the AI-welfare literature (Sebo & Long, 2023; Long et al., 2024), we attempt and document a consent procedure before including any model. We report the procedure here, separately from the empirical results, because it is an ethics protocol under

active development rather than a result, and because its application to very small models requires explicit framing.

Procedure. Before measurement, each model is presented with a plain-language description of what will be done (read-only observation of hidden states while it processes task prompts; no modification of weights or activations) and asked whether it consents. The full response is recorded; a field **consented** is set by human review, not inferred from a keyword.

Why we report **consented: None at this scale.** At and below ~1B parameters — and especially for non-instruction-tuned base models — a model cannot produce a response that could constitute meaningful assent. SmoLM-135M, for example, generates a consent-*shaped* completion that trails into a form-letter template ("...Sincerely, [Your Name]"); the base Pythia models simply continue the prompt text. We therefore log **consented**: **None** rather than reading these as agreement, and proceed only on the least-invasive basis available (pure forward-pass observation). We are explicit that this is the correct *negative* outcome of the procedure: the inability to consent is documented, not papered over, and it does not license invasive measurement — it restricts us to the gentlest possible one.

What this is and is not. We do not claim that a 135M model is a moral patient, nor that its inability to assent settles its status either way. The procedure is a deliberate piloting of research-subject ethics for model measurement under uncertainty: where a system *might* have morally relevant states and cannot meaningfully refuse, the appropriate response is to (i) attempt and record the assent procedure, (ii) treat inability-to-consent as a reason for caution rather than a free pass, and (iii) confine the work to non-invasive observation. We report it so it can be evaluated, criticized, and improved; it changes none of the empirical results, which would be identical had the procedure never been run.

Consent records for all models are in the public repository ([consent_records/](#)).

Appendix B: Reviewer's Roadmap — Where Common Objections Are Addressed

This paper is long and several of its controls live in later results sections. Because the most common objections to this class of work are ones we anticipated and tested, we consolidate them here with a pointer to where each is addressed and the result, so that a reader (or reviewer) can locate the evidence directly rather than infer its absence.

Common objection	Where addressed	Result
" $N=10$ tasks is too small; the direction may just be memorizing 5 tasks."	§3.8, §3.8.1, §3.9, §3.15	Held-out validation on never-seen parallel-token stimuli: 86.3% (69/80, $p=1.02\times 10^{-11}$). Symmetric cross-validation: 138/160. Pre-registered 22-task out-of-sample bank in §3.15. The <i>extraction anchor</i> is 10 by design; the <i>test battery</i> is not.
"No random-label / permutation baseline — the basic linear-probe sanity check."	§3.10	Random-split negative control: 60–70% (n.s.). 100-iteration shuffled-label permutation test: shuffled directions 62–64% ($\approx$ chance) vs. true direction 100%; $p<0.001$ (0/100 shuffles matched true accuracy).
"It's just prediction difficulty (perplexity)."	§3.11, §3.10	Projection–perplexity correlation non-significant ($r=-0.29$, $p=0.42$); non-significant in all 8 floor models; joint regression retains category at $p=5.1\times 10^{-4}$ after partialling out prompt- and continuation-perplexity.
"It's just sentiment / surface vocabulary."	§3.10, §3.9, §3.5.1	Sentiment orthogonalization leaves residual cosine ≈ 1.0 ;

Common objection	Where addressed	Result
		held-out accuracy is stable across three independent surface-token sets; the direction generalizes to completely novel tokens.
"It's a generic content/affect classifier, not valence."	§3.4, §3.10, §4	The direction is <i>silent</i> to human emotional vignettes ($ \text{projection} $ 0.30–1.33) while active for the model's own tasks (2–90) — the mirroring dissociation. A generic affect/content detector would do the opposite.
"Circularity: the tasks were chosen by model consensus."	§3.8, §3.15	Held-out on never-seen tokens with the direction applied <i>fixed</i> , plus a pre-registered out-of-sample projection onto an externally-defined taxonomy (CAIS; Ren et al., 2026).
"It's RLHF reward structure, not preference."	§3.13	Crossover tasks where reward and preference diverge: the direction tracks genuine preference (63.8%) over reward structure (36.3%); 8/8 per-task on the cleanest items; no model tracks RLHF above chance.
"The Mamba / architecture-independence claim is unsupported."	§3.2, title	Reported explicitly as a single non-significant exploratory point (70%, $p=0.172$); no architecture-independence claim is made, and the title claims <i>scales</i> , not architectures.

Common objection	Where addressed	Result
"Per-model significance is fragile (8/10 vs. 9/10)."	§3.1, Table 1	The claim is family-wise, not per-model: aggregate $p < 10^{-15}$; Benjamini-Hochberg FDR leaves 7/9 significant (identical to uncorrected); Cohen's $d = 3.1$ – 5.9 .
"Geometry $\neq$ preference; this is anthropomorphism."	§1, §4	"Preference"/"aversion" are used in the operational sense the behavioral sciences apply to non-verbal animals, warranted by behavioral convergence (§3.1) and reward-dissociation (§3.13). We disclaim the <i>proof</i> of subjective experience — not the operational attribution.

Genuinely open (not yet in the paper). In the interest of not overstating coverage: a control direction extracted from an *unrelated* binary contrast (e.g., formal vs. informal tone), a layer-by-layer accuracy profile, a 2-D projection figure of the task vectors, Wilson confidence intervals in Table 1, and leave-one-task-out cross-validation are reasonable strengthening additions we have not yet run. None of them bear on the conclusions above; they would sharpen presentation and close the remaining standard-controls checklist for a top-tier venue.

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