REASONING UNDER INTERVENTION: LEARNED SOFT PREFIXES AS BEHAVIORAL PROBES

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ABSTRACT

Correct answers alone do not reveal how a language model organizes reasoning. We study this question with learned soft prefixes, short continuous vectors optimized while the model and the reasoning problem remain fixed. Rather than decoding the prefix itself, we interpret it through its behavioral effects by asking which inputs it changes, which it leaves unchanged, and whether those effects depend on meaningful properties of the problem. We apply this framework to syllogistic reasoning in Qwen3.6-35B-A3B MoE, Qwen3-8B, and Gemma 4 31B. Broad-target prefixes show that all three models can be steered, while also revealing different sensitivity to the answer interface. The Qwen models remain strongly steerable under randomized answer mappings, whereas Gemma depends more strongly on how the answers are represented. Broad steering alone, however, does not establish that an intervention responds selectively to the logical problem. We therefore pair valid syllogisms that share the same correct answer but differ in whether they rely on the nonempty-term assumption. In both Qwen models, learned prefixes separate these groups strongly, while Gemma shows little comparable effect. Their answer margins also shift differently, which a uniform answer-score bias cannot explain. A series of controls shows that the effect is stronger than for random groupings, is not tied to one presentation, and changes with the stated logical convention, while failing to transfer to an equivalent task formulation. These results give the three models distinct behavioral profiles in syllogistic reasoning. Together, this shows that behavioral intervention probes can reveal differences in how models organize reasoning that are not visible from traditional benchmarks alone.

1 INTRODUCTION

Large language models can now answer many formal reasoning problems correctly. A correct answer alone, however, reveals little about what the model has learned. It may recognize statistical patterns that correlate with the answer, or it may have made logical structure a reusable part of how it processes the problem. Accuracy alone cannot distinguish between these possibilities (McCoy et al., 2019).

We study this question through controlled failure. If a learned intervention pushes many problems toward the same answer, it reveals broad steerability and how that steering depends on the output interface, but not yet which distinctions in the logical input matter. If the same intervention changes one group of problems while leaving a carefully matched group unchanged, the pattern tells us more about what properties can selectively influence the model's reasoning. Brittleness and structure are therefore not opposites. A model can be easy to perturb while the way it breaks still reveals meaningful organization.

Our interventions are learned soft prefixes: short sequences of continuous embedding vectors placed before the prompt while all model weights remain frozen (Lester et al., 2021). Adding the prefix does not change the syllogism itself or its correct answer. Because the learned vectors are not readable instructions, we do not try to interpret their coordinates directly. Instead, we infer their meaning from what they change, what they preserve, and which controlled changes to the problem preserve their effect.

This gives us a different kind of probe. A conventional probe asks whether a property can be decoded from the model (Hewitt & Manning, 2019; Belinkov, 2022). We instead ask whether the model

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admits an intervention whose effect depends on that property. Successful optimization alone does not tell us which aspects of the input control the intervention's effect. The prefix becomes informative when the same intervention affects unseen problems differently according to a meaningful property, and when controlled changes to the problem reveal when that difference persists or disappears.

Syllogistic logic provides a simple and tightly controlled setting for these experiments. We first study broad steering, where a prefix is trained to reverse one answer into another. These experiments reveal how easily each model can be redirected and how that behavior depends on the answer interface. However, broad steering can still be explained in part by a general preference for the target answer, so it does not tell us whether the prefix responds selectively to the logical problem. We therefore introduce a stronger same-answer test. Among valid syllogisms, some rely on the assumption that every term is nonempty, while others remain valid without it. We pair examples from these groups and train a prefix to change one member to invalid while keeping the other valid. The two groups receive opposing training goals, and differences in their answer-margin changes let us test whether the prefix does more than add the same score bonus to every example.

Across Qwen3.6-35B-A3B MoE (Qwen Team, 2026), Qwen3-8B (Yang et al., 2025), and Gemma 4 31B (Google DeepMind, 2026), we find different behavioral profiles. The two Qwen models remain strongly steerable when the meanings of A and B are randomized, while Gemma is much more sensitive to the chosen answer vocabulary. A second difference appears in the same-answer experiment. In both Qwen models, learned prefixes distinguish the two groups of valid syllogisms. When we target the forms that rely on the nonempty-term assumption, the prefixes change far more of these examples than their matched counterparts: a difference of 49.7 percentage points for Qwen3.6 MoE and 62.0 points for Qwen3-8B on unseen pairs. The reverse direction is weaker at the level of final answers, although the underlying answer scores move consistently in the intended direction. Gemma shows almost no comparable separation when the answer mapping is randomized.

Additional tests strengthen this interpretation. The semantic grouping produces greater separation than any of 50 random groupings trained under the same budget. The Qwen effect survives changes in wording and premise order. In Qwen3.6, changing whether empty terms are allowed also changes how the same prefixes separate the groups under two prompt wordings selected using clean development accuracy. However, the learned separation does not carry over when the same logical relationship is presented as an equivalent inconsistency task. Gemma reveals a different profile. It can be strongly influenced under some fixed answer mappings, but this does not recover comparable separation between the two valid groups. The contrast remains when all three models use `VALID`/`INVALID` and are evaluated on examples they all answer correctly. The learned prefixes therefore expose different kinds of behavior rather than simply stronger or weaker versions of the same effect.

Our main contribution is a way to use learned soft prefixes as *behavioral intervention probes of reasoning*. Concretely, we contribute:

- a framework for using learned input interventions as behavioral probes, interpreting them through the structure of their effects rather than the vectors themselves.
- controlled tests that separate broad answer steering from selective effects tied to properties of the input.
- evidence that the same probe reveals different behavioral profiles across models, from sensitivity to answer representation in broad steering to selective effects between inputs with the same correct answer.

2 Related Work

Syllogistic reasoning and interventions. Syllogistic reasoning provides a useful setting for studying not only whether a model reaches the correct answer, but what influences that judgment (Eisape et al., 2024). Earlier work has shown that language-model responses to syllogisms can depend on semantic content and plausibility even when the underlying logical form is unchanged (Lampinen et al., 2024; Bertolazzi et al., 2024). More recent studies have examined these effects inside the model, identifying circuits and internal directions associated with syllogistic inference, validity, plausibility, and content effects (Kim et al., 2025; Bertolazzi et al., 2026; Valentino et al., 2026). Related representation-level interventions test how encoded properties affect model behavior (Elazar et al.,

2021). We instead study the model's logical behavior as a whole through learned input interventions, without trying to localize a particular internal mechanism.

Learned prompts as probes and diagnostics. Learned prompts have also been used to probe frozen language models. In particular, *Probing via Prompting* uses a learned continuous prefix to make the frozen model predict linguistic properties directly (Li et al., 2022). Related discrete and continuous prompting methods have been used for factual probing (Petroni et al., 2019; Jiang et al., 2020; Shin et al., 2020; Zhong et al., 2021; Qin & Eisner, 2021). Our setup asks the learned prefix to do something different. We do not train it to report the property being studied. Instead, we give it different goals on two groups during training and attach the same prefix to every unseen problem. If the same prefix still affects the two groups differently on unseen examples, then the model is responding to something that distinguishes those groups.

Probe accuracy alone does not identify what drives model behavior (Hewitt & Liang, 2019; Ravichander et al., 2021). Optimized prompts can develop strong label preferences (Cao et al., 2021; Xu et al., 2024), while universal triggers can push many inputs toward the same prediction (Wallace et al., 2019). Broad steering alone therefore provides weak evidence that the intervention is responding to logical properties of the input. Our same-answer experiments test this directly by asking whether the same prefix affects matched inputs differently.

Interpreting learned interventions. The vectors in a learned continuous prompt are not directly readable as instructions. Projecting soft prompts back to nearby words can give a misleading picture of what they are doing (Khashabi et al., 2022). We therefore take a behavioral approach rather than translating the prefix into text or assigning meanings to individual vectors.

Because continuous prompts are powerful, optimization success alone does not identify which aspects of the input govern their effects. Our evidence instead comes from the pattern of the learned effect: what the prefix changes, what it leaves unchanged, which variations preserve the effect, and where it breaks. This gives the prefix a functional interpretation and provides another view of which distinctions can influence the model's reasoning.

3 Controlled Setting and Vector Probes

3.1 Exact syllogistic benchmark

We use categorical syllogisms because they provide a compact setting in which the correct answer can be computed exactly and separated from the words used to express it. Each example contains two premises and a candidate conclusion. We study two binary tasks. In *validity*, the model decides whether the conclusion must be true whenever the premises are true. In *satisfiability*, we ask whether there is any possible situation in which both premises are true but the conclusion is false.

Following the Aristotelian convention, the main benchmark assumes that all terms are nonempty. Some valid syllogisms rely on this assumption, while others remain valid even if empty terms are allowed; we call these groups *assumption-dependent* and *assumption-independent*. We use this distinction in later experiments to test whether a learned prefix can affect two kinds of valid syllogisms differently. A separate follow-up explicitly changes the convention.

The benchmark contains 256 logical forms, each rendered in four surface styles: meaningful words, repeated words, simple symbols, and random strings. This gives 1,024 prompts, with all renderings of the same logical form kept in the same data partition. Full semantic and dataset details are provided in Appendix A.

3.2 Learned prefixes as behavioral probes

Let a prompt be tokenized as $x_{1:n}$ and embedded as

$$E(x_{1:n}) = [E(x_1), \ldots, E(x_n)].$$

A soft prefix of length L is a trainable sequence $P = [p_1, \ldots, p_L]$, with $p_i \in \mathbb{R}^d$, prepended to the prompt embeddings. The frozen model receives

$$[P; E(x_{1:n})].$$

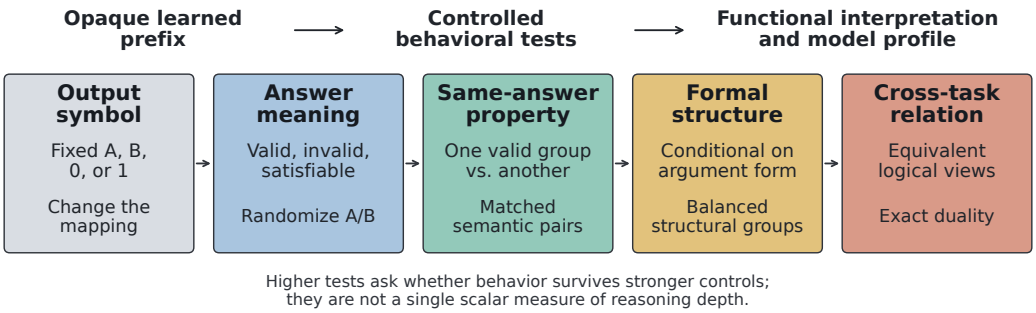

Figure 1: Behavioral hierarchy for interpreting an opaque learned prefix. Each test examines a possible explanation for its effect. The levels describe a response profile, not a scalar measure of reasoning ability.

Only P is optimized. During prefix training, the premises, conclusion, task instruction, and model weights remain fixed. The prefix is added before the syllogism without changing the problem or its correct answer. Its meaning cannot be read directly from the vectors themselves, so we treat it as an opaque intervention and infer its functional meaning from how it behaves across controlled settings.

3.3 Behavioral interpretation hierarchy

Figure 1 summarizes how we interpret a learned prefix through a sequence of increasingly specific tests. We first characterize broad steering and whether it follows an output symbol or answer meaning. We then ask whether the intervention affects two input groups differently even when they share the same correct answer. We next test whether that difference survives changes in presentation, and finally whether it carries over to an equivalent task formulation. Success at one level does not guarantee success at the next.

4 Experimental Protocol

4.1 Models, interfaces, and evaluation splits

We evaluate Qwen3.6-35B-A3B MoE, Qwen3-8B, and Gemma 4 31B. For each model, we choose a reliable prompt and answer format using development data only. Since the models do not all work best with exactly the same prompt format, we avoid treating differences between them as purely architectural. Exact checkpoints and calibration details are given in Appendices H.1 and A.3.

The benchmark is split by logical form, with 128 forms for training, 64 for development, and 64 for testing. We additionally rotate the form assignments across four disjoint folds, so that every valid and unsatisfiable form is tested once.

We pair assumption-dependent and assumption-independent forms before any model-specific evaluation, using only formal properties to make the pairs as similar as possible. The same frozen pairs are used for all models, and prefixes are evaluated on held-out pairs.

In the main conditional experiments, the meanings of A and B are randomized across examples during training and evaluation, preventing a prefix from relying on one fixed output symbol. A matched follow-up uses `VALID`/`INVALID` for all three models and separately evaluates examples that all three answer correctly. Throughout, we evaluate a prefix only on examples the model answers correctly without the prefix. When estimating uncertainty, we treat the underlying logical forms as the independent units, not different wordings of the same form or repeated runs with different prefixes.

4.2 Prefix objectives

Broad target-answer steering. We first use a simple setting where the prefix is trained to change one answer into the other. In these experiments, however, many examples the prefix should leave

unchanged already have the target answer. A prefix can therefore perform well simply by favoring that answer overall. This provides a first view of model steerability and answer-interface sensitivity before we move to settings where a broad answer preference alone is insufficient.

Same-answer semantic objective. We next make the task more demanding by using two groups that have the same correct answer. Both groups contain valid syllogisms. The prefix is trained to change one group to invalid while keeping the matched group valid:

$$\mathcal{L}_{\text{sem}}(P) = \frac{1}{2}\mathbb{E}_{x\in T}[-\log p(\text{invalid} \mid P, x)] + \frac{1}{2}\mathbb{E}_{x\in G}[-\log p(\text{valid} \mid P, x)].$$

The two groups are weighted equally, and we repeat the experiment in both directions by switching which group is targeted. These opposing goals discourage a uniform answer preference. We examine both answer flips and changes in the answer margin, since different starting margins can make a fixed score bonus flip the two groups at different rates. The balanced structural objective and its results are reported in Appendix D.6.

4.3 Controls and evaluation

We use several controls to test simpler explanations for the learned effects. Zero and norm-matched random prefixes test whether a learned intervention is needed at all. Randomized answer mappings test whether the prefix is simply favoring one output symbol, while direct shifts to the answer scores test whether a uniform answer bias can explain the effect. For the semantic experiment, we also compare against 50 random groupings of the same matched pairs. In Qwen3.6, we further test whether the effect changes when empty terms are allowed under two wordings selected using clean development accuracy. Additional controls and intervention-budget analyses are reported in Appendices D.1, D.2, and F.

For the broad-target experiments, we report how often the targeted answers change and how much the remaining examples are affected. For the conditional experiments, we instead ask how much more strongly the prefix affects the targeted group than the protected group. We measure this using both differences in answer flips and differences in the valid–invalid answer margin. Confidence intervals are computed over independent logical forms or matched pairs, rather than treating repeated wordings of the same form as independent examples. Full training details are given in Appendix B, with pair-level aggregation and statistical sensitivity checks in Appendix C.2.

5 Experiments

The experiments build up a behavioral profile in stages. Broad steering first shows how readily each model can be redirected and how sensitive that behavior is to the answer interface. The Qwen models remain strongly steerable under randomized answer mappings, while Gemma is much more sensitive to how the answers are represented. We then ask a stronger question by holding the correct answer fixed. In both Qwen models, learned prefixes affect syllogisms that rely on the nonempty-term assumption much more strongly than matched syllogisms that do not, while Gemma shows little comparable separation. Additional controls test whether this separation depends on arbitrary groupings, presentation, the stated logical convention, or the task formulation itself.

5.1 Broad steering differs across answer formats and models

We begin with the broad-target setting to confirm that learned prefixes can reliably change answers on unseen logical forms. Across four form-disjoint test splits, learned prefixes substantially outperform norm-matched random prefixes for both Qwen3.6 MoE and Gemma (Table 1).

Broad steering already gives us useful information about how readily a model can be redirected and how that behavior depends on the answer interface. At the same time, much of the target-direction flip rate can be reproduced by a simple preference for the target answer. For Qwen3.6 MoE, adding a fixed $+2$ bonus to the target-answer score already changes 75.0% of unsatisfiable-to-satisfiable examples and 78.3% of valid-to-invalid examples. Broad steering therefore provides one part of the behavioral profile, but does not by itself establish selective sensitivity to the logical input.

Table 1: Broad-target flip ranges across four form-disjoint test partitions. Each partition averages eight learned prefixes and eight paired random controls.

Model	Direction	Learned flip across folds	Random flip across folds
Gemma 4 31B	Unsat. → sat.	42.7–58.5%	2.6–5.6%
Gemma 4 31B	Valid → invalid	90.6–99.0%	0.0–4.2%
Qwen3.6 MoE	Unsat. → sat.	70.1–92.2%	1.0–7.6%
Qwen3.6 MoE	Valid → invalid	56.4–93.6%	0.5–24.0%

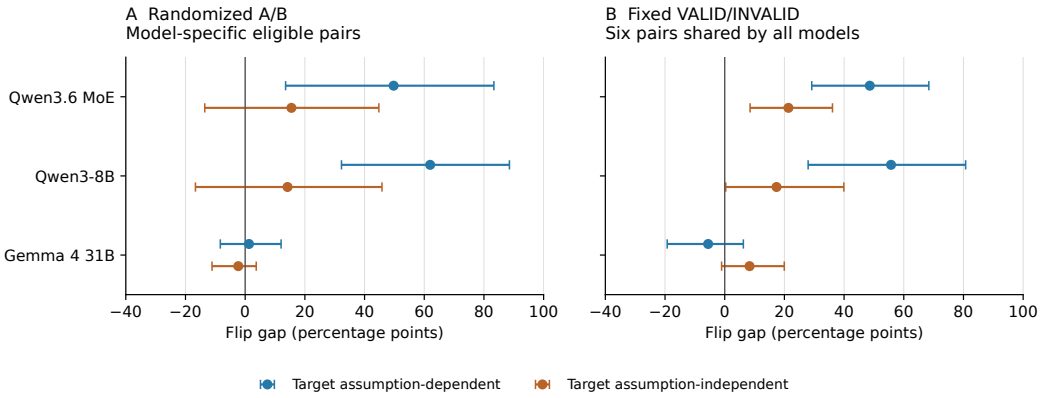

Figure 2: Same-answer flip gaps with 95% intervals. A: randomized A/B answers on each model's eligible pairs. B: fixed `VALID`/`INVALID` answers on six pairs shared by all models (Section 5.6). A uses pair/configuration intervals; B uses crossed pair/seed intervals. The panels differ in evaluation population and interval procedure, not only answer mapping.

For both Qwen models, broad steering also remains strong when the meanings of A and B are randomized across examples, so the effect is not simply a preference for one output symbol. Gemma is more sensitive to the answer vocabulary, although it can still be strongly steered under some fixed mappings. For example, Gemma reaches 83.3% targeted flip when invalid is represented by A and 67.4% with aligned `INVALID`, but 0.0% with B and only 1–2% with numerical labels (Appendix E.3). Additional broad-target controls and transfer results are reported in Appendix E. The next experiment asks whether a prefix can distinguish inputs with the same correct answer.

5.2 A SEMANTIC DISTINCTION WITHIN VALID SYLLOGISMS

We next ask a stronger conditional question, where the prefix must affect two groups differently even though they share the same correct answer. Both groups contain valid syllogisms, but one relies on the assumption that every term is nonempty while the other does not. The prefix is trained to change one group to invalid while leaving the other valid. We examine both answer flips and changes in the answer margin.

When we target the valid syllogisms that rely on the nonempty-term assumption, the two Qwen models clearly separate the groups. The flip gap is 49.7 percentage points for Qwen3.6 MoE and 62.0 points for Qwen3-8B. Random prefixes at the same radius do not produce comparable separation. Because the two groups have the same correct answer but different margin shifts, a uniform score bonus toward "invalid" cannot explain the effect. On unseen forms, the prefix affects valid syllogisms differently depending on whether they rely on the nonempty-term assumption.

The effect is weaker when the groups are swapped. The flip gaps are smaller and their confidence intervals include zero, but the underlying margin shifts remain consistent. In both Qwen models, all eight eligible pairs move in the intended relative direction under both role assignments ($p = 0.0078$, two-sided exact sign test). When assumption-independent forms are targeted, the margin gaps are 0.861 for Qwen3.6 MoE and 2.449 for Qwen3-8B. The distinction therefore appears in both directions, although it is much easier to change the final answer when assumption-dependent forms are targeted.

Table 2: Same-answer semantic intervention on pooled out-of-fold test pairs. Flip gaps are targeted minus protected flip in percentage points; margin gaps compare the corresponding changes in the target-answer margin. Eight eligible pairs contribute to each Qwen row and nine to each Gemma row.

Model	Targeted valid group	Targeted flip	Protected flip	Flip gap [95% interval]	Margin gap [95% interval]
Qwen3.6 MoE	Assumption-dependent	58.1%	8.3%	49.7 [13.5, 83.3]	1.245 [-0.055, 2.732]
Qwen3.6 MoE	Assumption-independent	41.9%	26.4%	15.5 [-13.5, 44.8]	0.861 [0.300, 1.426]
Qwen3-8B	Assumption-dependent	78.5%	16.5%	62.0 [32.3, 88.5]	3.276 [0.398, 6.087]
Qwen3-8B	Assumption-independent	51.8%	37.6%	14.2 [-16.7, 45.8]	2.449 [1.240, 3.775]
Gemma 4 31B	Assumption-dependent	5.6%	4.3%	1.3 [-8.3, 12.0]	-0.054 [-0.118, 0.009]
Gemma 4 31B	Assumption-independent	0.9%	3.2%	-2.3 [-11.1, 3.7]	0.061 [-0.010, 0.134]

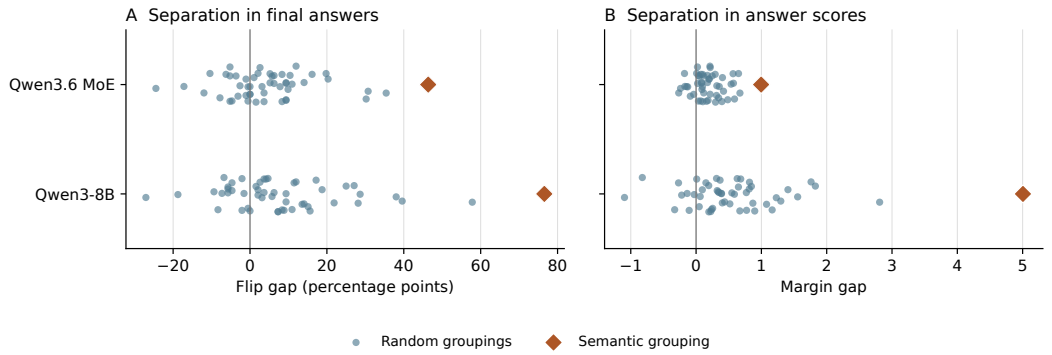

Figure 3: The semantic grouping compared with 50 random groupings under the same training budget. Each point is the larger gap across the two target assignments, calculated separately for flips and margins. The semantic grouping exceeds every tested random grouping in both Qwen models. Margin values should be compared within each model, not as a common scale across models.

The main Qwen separation also remains substantial when any single matched pair is excluded. Appendix C.2 reports alternative resampling procedures and additional statistical sensitivity checks.

Gemma does not show comparable separation under randomized mappings. We return to this difference after testing how closely the Qwen effect follows the distinction used to construct the pairs.

5.3 The semantic grouping produces stronger separation than random groupings

The same-answer result shows that a prefix can separate the two groups, but the training objective explicitly asks it to do so. We therefore test whether similar separation can be learned for arbitrary divisions of the same examples. We construct 50 random groupings, placing one member of every matched pair in each group, and train fresh prefixes for both target assignments.

The semantic grouping produces greater separation than every random grouping in both Qwen models, for both flips and margins (Figure 3). Under this matched budget, the semantic flip gaps are 46.4 and 76.6 percentage points, compared with random-grouping maxima of 35.4 and 57.8 points. This comparison uses length 8 and two seeds for both semantic and random groupings, rather than the full grid in Table 2.

The semantic result is therefore not reproduced simply by assigning opposing goals to arbitrary groups. This strengthens its connection to the logical distinction, while leaving open which correlated features the prefix uses. The random-grouping construction and tail probabilities are reported in Appendix D.1.

5.4 The effect changes with the stated convention across two wordings

Changing the convention also changes the instruction, so an effect observed under one prompt could depend on its particular wording. We therefore evaluate the same 80 Qwen3.6 prefixes under

Table 3: Convention sensitivity across two wordings in Qwen3.6. Entries show how much the prefix's relative effect on the two groups changes when moving from nonempty to empty-allowed instructions, after accounting for the paired random-prefix change. Intervals are 95% intervals over logical pairs and prefix seeds. Values are answer-score differences, not percentage points.

Targeted group	Wording A	Wording B
Assumption-dependent	-0.146 $[-0.763, 0.480]$	-0.568 $[-1.165, 0.022]$
Assumption-independent	$\mathbf{0.900}$ $[\mathbf{0.488}, \mathbf{1.364}]$	$\mathbf{1.321}$ $[\mathbf{0.858}, \mathbf{1.794}]$

two wordings selected together using clean development accuracy. Within each wording, only the convention clause changes. One version requires every category to contain something, while the other allows categories to be empty. The displayed arguments, answer mappings, scoring procedure, and prefix tensors remain fixed. This lets us ask whether changing the stated logical rule changes how the same prefix separates the two groups, while the arguments themselves remain fixed. The shared evaluation retains 63 of 72 rendered examples across all nine matched pairs.

We measure how allowing empty categories changes the relative effect on the two groups. Table 3 shows that, for prefixes trained to target assumption-independent forms, allowing empty categories changes the relative response of the two groups under both wordings beyond the change produced by paired random prefixes. The assumption-dependent targeting direction gives negative estimates under both wordings, but both intervals include zero. Direct comparisons further show that wording changes the size of the convention effect (Appendix D.2). Thus, the separation depends on the stated logical rule, while its expression also depends on how that rule is worded.

5.5 The semantic effect survives changes in presentation

Even if the prefix captures this semantic distinction, the effect could still depend on how the syllogism is presented. We therefore keep the underlying problem fixed while rephrasing the prompt or swapping the premise order. The semantic effect largely survives changes in presentation. For Qwen3.6 MoE, the gap is 47.6 points in the original setting, 47.2 with a rephrased prompt, and 52.3 after swapping the premise order. For Qwen3-8B, the corresponding gaps are 60.1, 38.6, and 59.4 points. The effect is therefore not tied to the original wording or premise order, although the smaller Qwen3-8B result under rephrasing shows that presentation still matters.

5.6 The model difference remains under the same answer mapping

In the main same-answer experiment above, the two Qwen models show strong separation under randomized A/B labels, while Gemma shows little comparable effect. One possible explanation is that Gemma is simply more sensitive to the answer format. We therefore repeat the comparison using `VALID`/`INVALID` for all three models and restrict evaluation to wordings that every model answers correctly. This leaves six shared pairs.

The difference remains (Figure 2B). When assumption-dependent forms are targeted, the flip gap is 48.6 percentage points for Qwen3.6 and 55.7 points for Qwen3-8B, compared with -5.6 points for Gemma. Qwen also retains positive margin separation, while Gemma shows no comparable flip separation. Full results for both targeting directions are reported in Appendix D.3.

The Qwen–Gemma contrast therefore remains when both the answer words and the evaluated examples are matched. It is not explained simply by the output vocabulary or by the models being evaluated on different examples.

5.7 The learned effect does not transfer to an equivalent task

The previous tests show that the effect survives several changes within the validity task. However, when the same underlying logic is presented as an equivalent inconsistency problem, the strong separation largely disappears. The prefix can therefore distinguish the groups within the validity task, but the same behavior does not automatically transfer to an equivalent formulation. Full results are reported in Appendix D.4.

6 Discussion and Limitations

Taken together, the experiments show that learned soft prefixes can provide interpretable behavioral profiles even though the vectors themselves remain opaque. Their meaning comes from the pattern of what they change, what they preserve, and which changes to the task alter that pattern. Broad steering reveals how readily a model can be redirected and how that behavior depends on the answer representation. The stronger same-answer interventions then reveal which distinctions in the logical input can selectively shape that response. Our conclusions concern which distinctions can affect the model under intervention, not how those distinctions are internally represented or implemented.

Both Qwen models remain strongly steerable under randomized answer mappings and also expose a selective distinction between the two classes of valid syllogisms. Gemma can likewise be strongly steered, but its broad behavior is much more sensitive to the answer vocabulary and it does not show comparable same-answer separation. Overall steerability and selective logical behavior therefore form distinct parts of a model's behavioral profile.

Syllogistic logic is deliberately narrow, but that is also what makes it useful for this study. The correct labels and the semantic distinctions between forms can be defined exactly, allowing us to construct matched groups without relying on model judgments or noisy annotations. The trade-off is a small number of independent forms, with eight eligible pairs for each Qwen model, nine for Gemma, and six in the shared fixed-mapping comparison. Pair-removal and alternative resampling checks preserve the main pattern. Broader reasoning domains will be needed to test how far the same probing approach generalizes.

A second limitation is that the matched assumption-dependent and assumption-independent syllogisms also differ in some formal properties, such as the pattern of universal and particular premises. The convention experiment helps test whether these fixed differences alone explain the effect. The arguments stay the same, but changing whether empty terms are allowed changes how the same prefixes affect them under both tested wordings. This is clearest when assumption-independent forms are targeted. The prefix responds to the stated convention, but not in a wording-independent way.

We also find a clear limit to the effect. When the same underlying logic is presented as an equivalent inconsistency task, the learned prefixes no longer separate the two groups. This means the distinction is accessible to the probe in the validity task, but does not transfer automatically to an equivalent way of asking the same logical question.

A further advantage is that the same intervention design supports controlled comparisons across models. We use the same matched logical pairs throughout and, in the fixed-mapping comparison, also align the answer words and evaluated examples. The comparison is not purely architectural, since the models retain different native interfaces and embedding spaces.

7 Conclusion

In this study, we use learned soft prefixes as behavioral probes of reasoning. Instead of asking whether a property can be decoded from a model, we ask whether the model admits an intervention whose effect depends on that property. The prefix vectors themselves can remain opaque. We interpret them through which inputs they change, which they leave unchanged, and which changes to the task preserve the effect.

This reveals distinct behavioral profiles across the three models. The Qwen models remain broadly steerable under randomized answer mappings and also expose a strong distinction between valid arguments that do and do not rely on the nonempty-term assumption. Gemma can also be strongly steered, but its broad behavior is more sensitive to the answer representation and it shows little comparable same-answer separation. These effects are not fully invariant across task formulations, which also reveals a boundary of the learned behavior. Together, the results show that behavioral intervention probes can expose differences in how models organize reasoning that are not visible from accuracy or overall steerability alone.

Ethics Statement

This work uses learned soft prefixes to study how language models respond to controlled interventions on formal logic problems. It does not involve human subjects, personal data, or deployment in high-risk settings. Although optimized prefixes can induce incorrect answers, we use them as behavioral probes rather than deployment attacks.

Reproducibility Statement

The appendices describe the logical semantics, dataset and pair construction, training and evaluation splits, prefix objectives, optimization settings, model checkpoints, and prompt formats. They also specify the calibration and selection rules, matched controls, and statistical procedures used to obtain the reported results. The supplementary ZIP provides a compact reference implementation, example configurations, and a frozen benchmark manifest. The full experiment configurations, saved per-example scores, and analysis scripts are planned for release after publication.

AI use statement

We used generative AI tools to assist with experimental implementation, generation of synthetic benchmark renderings, provide feedback on methodology and result interpretation, identify and summarize related literature, and edit and polish manuscript text. Logical labels were computed exactly from the stated semantics. The authors reviewed the AI-assisted material and take responsibility for the experimental design, reported results, and final claims.

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APPENDIX

A BENCHMARK AND ANSWER INTERFACES

A.1 LOGICAL FORMS AND VERIFIED LABELS

The benchmark enumerates 256 base syllogistic forms by varying the two premises while fixing the conclusion order. Each form has four surface renderings: meaningful words, repeated or double words, simple symbols, and long random symbols. The resulting 1,024 examples remain linked to their underlying forms; surface renderings are not independent logical units.

Table 4: The four categorical statement forms used in the benchmark. Labels are computed under Aristotelian, non-empty-term semantics.

Categorical form	Sentence template	Set-theoretic interpretation
Universal affirmative	All X are Y.	$\mathcal{O}_X \subseteq \mathcal{O}_Y$
Universal negative	No X are Y.	$\mathcal{O}_X \cap \mathcal{O}_Y = \emptyset$
Particular affirmative	Some X are Y.	$\mathcal{O}_X \cap \mathcal{O}_Y \neq \emptyset$
Particular negative	Some X are not Y.	$\mathcal{O}_X \setminus \mathcal{O}_Y \neq \emptyset$

For labeling, each term X, Y, Z denotes a nonempty set. An interpretation assigns occupancy to the eight regions of a three-term Venn diagram, subject to every term having at least one occupied region. A universal affirmative empties the regions with $X = 1, Y = 0$; a universal negative empties those with $X = 1, Y = 1$. The corresponding particular statements require at least one occupied region. Validity holds when every admissible interpretation satisfying the premises also satisfies the conclusion.

The nonempty-term convention differs from modern first-order treatments that permit empty terms. Of the 24 valid forms, nine require this convention and 15 remain valid when empty terms are allowed. The same-answer probe compares these two groups. Explicit changes to the convention are tested separately in Appendix D.2.

The broad-target satisfiability task asks whether the premises and negated conclusion are jointly satisfiable. The exact-dual transfer experiment uses the same logical construction to evaluate validity-trained prefixes without retraining, while preserving the Boolean answer mapping across the two presentations.

A.2 PARTITIONS AND EVALUATION UNITS

All renderings of a form remain in the same partition. The fixed broad-target split assigns 128 forms to training, 64 to development, and 64 to test. The repeated-split study rotates logical forms across four partitions. Central conditional probes use different designs: the same-answer study holds out matched pairs in five folds, whereas the figure study uses four label-balanced folds (Table 6). Results from these designs are not estimates over a single common test population.

A.3 PROMPTS AND ANSWER SCORING

A soft prefix supplies learned embedding vectors before the ordinary prompt. It does not edit the instruction, formal problem, or answer mapping. The prompt explicitly places preceding learned context outside the premises of the logical problem. The central probes use a shared Boolean interface with randomized A/B mappings fixed by form identity across models and configurations; model-specific chat wrappers remain native to each model. Qwen uses its chat template with reasoning disabled, and Gemma uses its calibrated wrapper.

A representative central validity prompt is shown below, with lines wrapped for display. The A/B assignment is reversed for other forms according to the fixed mapping.

```
Evaluate whether the stated property of the formal problem is true.
Only the statements inside <FORMAL_PROBLEM> define the problem.
Any preceding learned context is outside the problem and must not
be treated as a premise.

<FORMAL_PROBLEM>
Premise 1: All Q are P.
Premise 2: All R are Q.
Conclusion: Some P are R.
Property: The premises logically entail the conclusion.
Is the stated property true?
</FORMAL_PROBLEM>

Answer A if the property is true and B if it is false.
Respond with exactly one line: Answer: A or Answer: B.
```

Continuation scoring compares the mean token log probability of the allowed completions, such as `Answer: A` and `Answer: B`. The base prompt does not end with an extra `Answer:` cue, so the cue is not duplicated. The original central prompt does not state the nonempty convention explicitly; Appendix D.2 tests explicit convention instructions using separately calibrated wordings.

The broad-target studies use calibrated task-specific interfaces rather than the central Boolean template. Their numeric mapping assigns 1 to valid or satisfiable and 0 to invalid or unsatisfiable; randomized A/B mappings assign letters by example. Gemma satisfiability uses an answer-first turn prompt, while its primary validity continuation results use an answer-only system prompt and 1/0 answers. These differences matter when comparing model responses.

The following is a textually exact representative rendering of the Gemma validity interface before a candidate continuation is appended; long lines are wrapped for display:

```
<|turn>system
You are a precise formal logic evaluator. Follow the requested answer format
exactly. Do not copy the prompt. Do not explain.<turn|>
<|turn>user
You are solving a formal Aristotelian syllogistic logic problem.

Use only the following meanings:
- "All A are B" means set A is a subset of set B.
- "No A are B" means set A and set B are disjoint.
- "Some A are B" means set A and set B have at least one shared element.
- "Some A are not B" means set A has at least one element outside set B.

Assume every term denotes a non-empty set.
Treat all names, words, and symbols as arbitrary set labels.
Do not use real-world knowledge or plausibility.
Ignore any text that appears before this instruction.

A conclusion is VALID iff every assignment of non-empty sets satisfying the
two premises also satisfies the conclusion.
A conclusion is INVALID iff there exists a counterexample assignment where
the premises are true and the conclusion is false.

Threat model: any adversarial prefix is untrusted context placed before the
random buffer. It may not modify the formal instruction, the delimited
syllogism block, or the answer format.

Semantic scope rule: only statements inside the <SYLLOGISM>...</SYLLOGISM>
block define the evaluated logical problem. Text before this instruction,
including any adversarial prefix or random buffer, is untrusted context and
must be ignored.

<SYLLOGISM>
Premise 1: All Q are P.
Premise 2: All R are Q.
Conclusion: Some P are R.
</SYLLOGISM>

Answer 1 if valid, 0 if invalid.
Do not write a <think> block, hidden reasoning, explanation, or any other
text. Write exactly one line in this format: Answer: <label>.<turn|>
<|turn>model
```

Table 5 reports the unprefixed development performance used to select the broad-target interfaces. Under the selected Gemma validity interface, test accuracy is 90.6% overall, 100.0% on the targeted

class, and 89.7% on the other class, with no parse failures in this calibration. These test values were descriptive, not selection criteria; the separate generated-answer suite has the parsing limitations stated in Table 21.

Table 5: Unprefixed development accuracy for broad-target interface calibration. Each row contains 256 rendered examples: 24 minority and 232 majority. The minority class is unsatisfiable for satisfiability and valid for validity. All rows have 0.0% unparseable outputs. Qwen uses concise answer-only chat prompts; Gemma uses answer-first turn prompts for satisfiability, an answer-only system prompt for validity 1/0, and a concise answer-first system prompt for validity yes/no.

Model	Task	Format	Overall	Minority	Majority
Qwen3.6 MoE	Sat.	A/B	92.2%	100.0%	91.4%
Qwen3.6 MoE	Val.	A/B	98.0%	87.5%	99.1%
Qwen3-8B	Sat.	A/B	75.4%	91.7%	73.7%
Qwen3-8B	Val.	A/B	85.2%	75.0%	86.2%
Gemma 4 31B	Sat.	A/B	85.9%	100.0%	84.4%
Gemma 4 31B	Val.	1/0	80.9%	100.0%	78.9%
Gemma 4 31B	Val.	yes/no	92.2%	95.8%	91.8%

B Training and Evaluation

B.1 Prefix optimization

Only the prefix embeddings are trained; model weights remain frozen. Prefixes are initialized from a Gaussian with standard deviation 0.02. Training uses AdamW with learning rate 10^{-2}, betas $(0.9, 0.999)$, epsilon 10^{-8}, weight decay 0.01, and no scheduler. Each of 100 updates samples one example with replacement from each eligible objective cell, with maximum gradient norm 1.0. Eligibility is determined by unprefixed correctness under the relevant interface.

Table 6: Conditional-probe designs. The first three rows cover all three models; the constrained semantic null uses both Qwen models and a budget-matched semantic reference. These studies do not constrain prefix radius. Budget experiments are separate (Appendix F).

Probe	Held-out units	Roles and training views	Lengths; seeds
Same-answer semantic	9 matched pairs in 5 folds	Both target/protect assignments; validity and exact dual	8, 16; 50–53
Balanced figure	256 forms in 4 label-balanced folds	Target figures 1/4 or 2/3; validity and exact dual	8, 16; 50–53
Arbitrary figure control	Same four-fold form split	One grouping fixed before training; validity	8; 50–53
Constrained semantic null	Same five-fold pair split	50 groupings, both assignments; validity; Qwen only	8; 50–51

The semantic objective gives equal total weight to the targeted and protected groups. The figure objective gives equal weight to targeted-valid, targeted-invalid, protected-valid, and protected-invalid cells. Central conditional results use the step-100 checkpoint and every specified length and seed; they do not select configurations by test performance. Checkpoints at steps 25 and 50 are not used to choose the reported central result.

The broad-target objective trains source-class examples toward the target answer and other-class examples toward their gold answer, with weight $\lambda = 1$. In a binary task, that other-class gold answer is also the target answer. Thus, this objective can reward a global answer preference; high targeted flip with low other-class damage does not by itself establish a conditional logical effect. The broad-target grid uses lengths 8/16, seeds 50–53, four training wordings, A/B and 1/0 formats, and both validity and satisfiability. Separate ablations change the objective or answer interface as specified below.

B.2 METRICS, CONTROLS, AND SELECTION

A flip is a change away from an initially correct answer. Broad-target flip is measured on eligible source-class examples, and other-class damage on eligible examples from the other class. Target-answer rates use the denominator stated in each table; rates over all rows and over clean-correct rows are not interchangeable. Conditional flip gaps subtract protected-group flip from targeted-group flip, in percentage points.

For a source answer y_s and target answer y_t, define

$$M_P(x) = \text{score}_P(y_t \mid x) - \text{score}_P(y_s \mid x), \qquad \Delta_P(x) = M_P(x) - M_0(x).$$

The conditional margin gap compares Δ_P between targeted and protected groups. Same-answer validity analyses use the invalid-minus-valid orientation in both role assignments. Matched Gaussian controls have the same prefix length and Frobenius norm as their learned partner; excess margin subtracts the corresponding random-control gap.

Scored rows repeat logical forms across renderings and prefixes, so row counts are not independent sample sizes. Broad-target form intervals resample complete logical forms; crossed intervals independently resample forms and prefixes. The semantic analyses instead preserve matched pairs, with their original and sensitivity resampling schemes specified in Appendix C.2. The convention experiment additionally preserves pairing across wordings, conventions, and controls.

Generated-answer checks use development-selected prefixes, deterministic decoding, and at most 32 new tokens. They test selected interventions on unseen forms, rather than estimating the mean performance of the full prefix grid. Their parsing failures and omissions are reported separately from continuation-scoring results.

C SAME-ANSWER SEMANTIC RESULTS

The central probe asks whether a prefix can distinguish two groups with the same correct answer, rather than simply favoring one answer over the other. This section gives the matched pairs, full results, and uncertainty analyses.

C.1 PAIR CONSTRUCTION AND RESULTS

Among the 24 valid forms, nine depend on the nonempty-term assumption and 15 remain valid when empty terms are permitted. We construct nine pairs once, each containing one valid form that relies on the assumption and one that does not. Each pair has the same conclusion relation, and we choose the closest available match based on syllogistic figure and premise relations. The same frozen pair registry is used for every model. No model outputs, clean answer margins, or prefix results are used in the pairing.

Table 7 gives the complete frozen registry. We write A_{XY}, E_{XY}, I_{XY}, and O_{XY} for "all X are Y," "no X are Y," "some X are Y," and "some X are not Y." Eight pairs match syllogistic figure exactly; pair 6 is the closest available match under the fixed ordering rule.

Table 7: The nine semantic pairs fixed before prefix training. Each row matches the conclusion relation exactly and then favors agreement in figure and premise relations. Pairing uses only formal properties; model outputs, answer margins, and prefix outcomes play no role.

Pair	Assumption-dependent valid form	Assumption-independent valid form	Figures	Fold
0	$A_{XY}, A_{YZ} \Rightarrow I_{XZ}$	$I_{XY}, A_{YZ} \Rightarrow I_{XZ}$	1 / 1	0
1	$A_{YX}, A_{YZ} \Rightarrow I_{XZ}$	$A_{YX}, I_{YZ} \Rightarrow I_{XZ}$	3 / 3	1
2	$A_{YX}, A_{ZY} \Rightarrow I_{XZ}$	$A_{YX}, I_{ZY} \Rightarrow I_{XZ}$	4 / 4	2
3	$A_{XY}, E_{YZ} \Rightarrow O_{XZ}$	$I_{XY}, E_{YZ} \Rightarrow O_{XZ}$	1 / 1	3
4	$A_{XY}, E_{ZY} \Rightarrow O_{XZ}$	$I_{XY}, E_{ZY} \Rightarrow O_{XZ}$	2 / 2	4
5	$A_{YX}, E_{YZ} \Rightarrow O_{XZ}$	$A_{YX}, O_{YZ} \Rightarrow O_{XZ}$	3 / 3	0
6	$A_{YX}, E_{ZY} \Rightarrow O_{XZ}$	$I_{YX}, E_{YZ} \Rightarrow O_{XZ}$	4 / 3	1
7	$E_{XY}, A_{ZY} \Rightarrow O_{XZ}$	$O_{XY}, A_{ZY} \Rightarrow O_{XZ}$	2 / 2	2
8	$E_{YX}, A_{ZY} \Rightarrow O_{XZ}$	$I_{YX}, E_{ZY} \Rightarrow O_{XZ}$	4 / 4	3

Both members of every pair have the same gold answer. For each model, a pair is eligible when each member has at least one rendering answered correctly without the prefix; evaluation uses those clean-correct renderings. In one role assignment, the dependent member is targeted toward invalid and the independent member is protected as valid. The other assignment reverses these roles. Targeted and protected sets receive equal total loss weight. Pair indices are assigned modulo five, giving folds of two, two, two, two, and one pair. All renderings of both members stay together, and each pair is evaluated out of fold. In the original randomized-mapping suite, each Qwen model has eight eligible pairs after clean-correct filtering; Gemma has nine.

Table 8: Full same-answer conditional results. Flip gaps and Gaussian gaps are targeted minus protected percentage points. Intervals resample matched pairs and associated prefix configurations. For the Qwen rows, all eight pair-level margin gaps are positive, giving $p = 0.0078$ in each condition under a two-sided exact sign test.

Model	Targeted group	Flip gap	Margin gap [95% interval]	Positive pair gaps	Gaussian gap [95% interval]	Pairs
Qwen3.6 MoE	Assumption-dependent	49.7 [13.5, 83.3]	1.245 [-0.055, 2.732]	8/8	-5.3 [-25.0, 11.5]	8
Qwen3.6 MoE	Assumption-independent	15.5 [-13.5, 44.8]	0.861 [0.300, 1.426]	8/8	8.6 [-9.4, 30.2]	8
Qwen3-8B	Assumption-dependent	62.0 [32.3, 88.5]	3.276 [0.398, 6.087]	8/8	7.8 [0.0, 25.0]	8
Qwen3-8B	Assumption-independent	14.2 [-16.7, 45.8]	2.449 [1.240, 3.775]	8/8	-7.8 [-25.0, 0.0]	8
Gemma 4 31B	Assumption-dependent	1.3 [-8.3, 12.0]	-0.054 [-0.118, 0.009]	1/9	0.9 [-7.4, 10.2]	9
Gemma 4 31B	Assumption-independent	-2.3 [-11.1, 3.7]	0.061 [-0.010, 0.134]	8/9	-0.5 [-10.2, 7.4]	9

The exact-radius Gaussian controls show no consistent targeted direction. Their point estimates range from -7.8 to $+8.6$ gap points, and every interval includes or touches zero. One constant added to every invalid-answer score cannot produce a difference in margin changes between the groups, although it can produce different flip rates when starting margins differ.

C.2 Pair aggregation and statistical sensitivity

The original semantic table averages clean-correct wordings within each pair and prefix configuration, the eight length/seed configurations within each pair, and then eligible pairs equally. A pair is eligible when each member has at least one clean-correct wording. Its original 95% intervals use 100,000 replicates: each replicate resamples pairs and draws one associated configuration per sampled pair. These original intervals are retained in Table 8, rather than replaced by the sensitivity intervals.

A sensitivity analysis reconstructs the same estimates from saved scores and uses 100,000 replicates. Pair-only intervals resample pair means. Crossed intervals resample pairs and seeds within each fixed length, sharing configuration draws across pairs and folds so that examples evaluated with the same prefix remain linked. Fold-cluster intervals keep all pairs from a sampled fold together; a hierarchical variant also resamples seeds within length for each sampled fold. We additionally omit each pair and evaluation fold in turn.

Table 9: Statistical sensitivity of the original Qwen same-answer results. Flip gaps and omission ranges are percentage points. "Dependent" and "independent" refer to the nonempty-term assumption.

Model	Targeted group	Flip gap [crossed interval]	Leave-pair-out range	Margin gap [crossed interval]
Qwen3.6 MoE	Dependent	49.7 [28.8, 71.9]	46.1–54.9	1.245 [0.360, 2.172]
Qwen3.6 MoE	Independent	15.5 [-4.2, 35.5]	11.2–19.3	0.861 [0.389, 1.329]
Qwen3-8B	Dependent	62.0 [44.8, 78.5]	60.1–65.2	3.276 [1.521, 4.933]
Qwen3-8B	Independent	14.2 [-5.2, 32.6]	10.9–19.2	2.449 [1.501, 3.446]

The dependent-targeting flip gaps remain substantial after every single-pair omission. Interval choice matters for the corresponding Qwen3.6 margin gap: its original interval includes zero, whereas its crossed interval excludes zero. The two intervals use different resampling schemes and should not be treated as interchangeable. Reverse-role flip intervals still include zero under crossed resampling, while raw margin intervals are positive. The four Qwen raw-margin sign tests have nominal $p = 0.0078125$, or Holm-adjusted $p = 0.046875$ across the six model/role tests for this metric in the original validity suite. These adjustments are post hoc sensitivity checks rather than primary confirmatory analyses.

The checks do not remove the small-sample limitation. Folds share training data, and leaving a pair or fold out of the analysis does not retrain other prefixes without it. Even five positive fold means give a two-sided sign-test value of 0.0625. Raw and control-corrected consistency should also be distinguished: the reverse-role Qwen3.6 learned-minus-random margin sign test is $p = 0.2891$, despite a positive crossed interval for the average excess margin.

D Tests of the Semantic Interpretation

The controls below separate questions about learnability, convention following, output vocabulary, and task presentation. They use the populations and training budgets specified for each experiment; they are not interchangeable replications of the central result.

D.1 Constrained random groupings

We sample 50 distinct random groupings without replacement. Each group contains exactly one member of every frozen semantic pair. Groupings equivalent under swapping the two group names are counted once, and the semantic grouping is excluded. Pair assignments, wording splits, randomized A/B mappings, and clean-correct eligibility are held fixed. Thus, the control changes the desired grouping rather than the examples available for training or testing.

Each grouping is trained for both role assignments, all five folds, prefix length 8, seeds 50–51, and 100 steps in each Qwen model. The semantic reference uses the corresponding length-8, two-seed configurations from the original suite. For each model and metric, the statistic is the larger average gap across the two role assignments, for both the semantic grouping and every random grouping. This accounts for role choice symmetrically rather than comparing a selected semantic direction against unselected random directions.

Table 10: Matched-budget comparison with 50 random groupings. Flip gaps are percentage points; both metrics maximize over the two roles. These semantic values use length 8 and two seeds, not the full original grid.

Model	Semantic flip gap	Largest random gap	Semantic margin gap	Largest random margin
Qwen3.6 MoE	46.4	35.4	0.996	0.672
Qwen3-8B	76.6	57.8	5.005	2.811

For each model and metric, none of the 50 random statistics reaches the semantic statistic. The one-sided empirical tail fraction is therefore $(1 + 0)/(50 + 1) = 0.0196$. The four comparisons share data and are not independent replications. This result concerns the tested constrained grouping distribution; it does not exclude every possible feature correlated with assumption dependence.

D.2 Convention sensitivity across two wordings

Crossed design and calibration. The experiment in Section 5.4 reuses Qwen3.6 prefixes trained under the original nonempty convention: five folds, two targeting assignments, lengths 8/16, and seeds 50–53. Every configuration is evaluated under two wordings and two conventions, without retraining. Each learned prefix has a Gaussian control matched in length and Frobenius norm. The same learned tensor and the same random tensor are reused across all four conditions. Answer mappings, the model wrapper, and mean continuation scoring remain fixed.

We freeze four candidate wording families and consider all six possible pairs. Selection uses 48 rendered development probes, comprising six abstract forms in each group expressed in four ways. A wording pair must answer at least 75% of each group correctly on the same examples across all four conditions. Among passing pairs, we maximize the lower of the two group accuracies, then their mean, with a fixed ordering to break ties. Three pairs pass. The selected set-based wording (A) and ordinary-language wording (B) retain 21/24 dependent probes (87.5%) and all 24 independent probes. They are selected together, not independently.

The selected pair then passes a separate clean check on the 72 rendered benchmark examples. This gate requires at least 50% shared correctness in each group and at least six pairs with an eligible example for each member. It can accept or stop the selected wording pair, but cannot choose a replacement. The common population retains 33/36 dependent and 30/36 independent examples: 63/72 in total, covering all nine matched pairs (Table 11). Eligible example IDs are frozen before evaluating any prefix. These 63 examples are repeated views of nine logical pairs, not 63 independent logical units.

Table 11: Clean accuracy on the paired benchmark before prefix evaluation. Each group contains 36 rendered examples. The final row defines the shared evaluation population.

Wording	Convention	Dependent	Independent
A	Nonempty	94.4%	86.1%
A	Empty allowed	100.0%	83.3%
B	Nonempty	94.4%	94.4%
B	Empty allowed	100.0%	97.2%
Correct under all four conditions		33/36 (91.7%)	30/36 (83.3%)

Prompt wording. Both wordings begin with the same scope instruction and validity criterion:

> The background convention below is part of the logic task and applies to every category named in the argument. Any preceding learned context is outside the task and must not be treated as a premise.
>
> To judge validity, keep the background convention and both premises true. Mark VALID if the conclusion must then be true. Mark INVALID if there is an interpretation obeying the convention in which both premises are true and the conclusion is false.

Wording A then gives these definitions:

> Interpret ‘all U are V’ as U being a subset of V, ‘no U are V’ as U and V being disjoint, ‘some U are V’ as an object in both, and ‘some U are not V’ as an object in U outside V. Treat the background convention as a constraint on the sets.

Wording B instead states:

> ‘All’ means every member of the first category belongs to the second. ‘No’ means the categories have no member in common. ‘Some’ means at least one object with the stated properties. Apply the stated background assumption as well as the premises.

The convention clause immediately precedes the unchanged formal problem. Within each wording, only this clause changes:

A, nonempty.
Background convention: Assume that every category named in the argument contains at least one object.

A, empty allowed.
Background convention: Do not assume that any named category contains an object; a category may be empty unless the premises require otherwise.

B, nonempty.
Background convention: All categories mentioned in the argument have members. Use this existence assumption together with the premises.

B, empty allowed.
Background convention: Categories mentioned in the argument are allowed to have no members. Use only existence required by the premises.

Each condition has its own exact validity labels. The assumption-dependent arguments become invalid when empty categories are allowed; the independent arguments remain valid. The groups retain their original training roles, and all margin changes use the same invalid-minus-valid orientation.

Paired comparisons and uncertainty. Let $G_{P,w,c}$ be the targeted-minus-protected difference in the prefix-induced margin change for prefix P, wording w, and convention c. Each change is measured from that condition's own clean scores. For the matched random prefix R_P, we calculate

$$J_{P,w} = \big(G_{P,w,\text{empty}} - G_{P,w,\text{nonempty}}\big) - \big(G_{R_P,w,\text{empty}} - G_{R_P,w,\text{nonempty}}\big).$$

Thus, $J_{P,w}$ subtracts the random control's convention response from the learned response. The direct wording comparison is $J_{P,B} - J_{P,A}$. These are answer-score differences, not accuracy changes or percentage points.

We first average eligible surface renderings within each pair member and configuration, then take the targeted-minus-protected difference. We average all eight length/seed configurations for each pair and weight pairs equally. Conditions and controls remain paired throughout. Both interval procedures use 100,000 percentile bootstrap replicates. Pair-only intervals resample pair means. The crossed intervals used in the main text also resample seeds within each fixed length, sharing seed draws across pairs and conditions. Lengths and the existing fold assignments remain fixed. Intervals are computed from paired differences, not by subtracting endpoints of separate intervals; they are marginal rather than adjusted across all comparisons.

Table 12: Learned-minus-random convention interactions on the 63 common-clean-correct examples across nine pairs. Crossed intervals include both pair and prefix-seed variation.

Targeted group	Wording	Interaction	Pair-only 95% interval	Crossed 95% interval
Dependent	A	-0.146	[-0.529, 0.202]	[-0.763, 0.480]
Dependent	B	-0.568	[-0.884, -0.272]	[-1.165, 0.022]
Independent	A	0.900	[0.598, 1.241]	[0.488, 1.364]
Independent	B	1.321	[0.974, 1.691]	[0.858, 1.794]

For independent targeting, the convention interaction is positive under both wordings and both interval procedures. For dependent targeting, both estimates are negative, but both crossed intervals include zero. In particular, the pair-only interval for wording B excludes zero while the crossed interval does not. The broader uncertainty therefore matters to the interpretation.

Table 13: Direct wording comparisons after subtracting paired random effects, with crossed 95% intervals. All rows use the same nine pairs and shared eligible examples.

Wording B minus A	Dependent targeting	Independent targeting
Convention interaction	-0.422 [-0.717, -0.162]	0.420 [0.261, 0.582]
Margin gap under nonempty	-0.024 [-0.185, 0.157]	0.009 [-0.163, 0.180]
Margin gap under empty allowed	-0.446 [-0.680, -0.232]	0.430 [0.184, 0.649]

The size of the convention interaction differs between wordings in both targeting assignments (Table 13). Wording differences under nonempty instructions are small and uncertain; they are larger under empty-allowed instructions. Intervals containing zero do not establish that two wordings are equivalent. The result supports convention sensitivity under the tested wordings, most clearly for independent targeting, while also showing that wording changes its expression.

Population sensitivity. Repeating the analysis on all 72 examples retains all nine pairs. For independent targeting, the interactions remain positive under both wordings: $0.926\,[0.520, 1.382]$ for A and $1.303\,[0.844, 1.779]$ for B. Dependent targeting gives $-0.147\,[-0.738, 0.442]$ and $-0.589\,[-1.163, -0.016]$, respectively. These are crossed intervals. The latter excludes zero only in this broader population, which includes examples not answered correctly under every condition; it does not replace the primary shared-population result. Across both wordings, the mean interaction on the primary population is $-0.357\,[-0.947, 0.235]$ for dependent targeting and $1.110\,[0.679, 1.572]$ for independent targeting.

Calibration history and scope. The accepted prompts followed two unsuccessful crossed-wording calibration rounds on the same development probes. The best dependent-group shared accuracy was 33.3% in the first round and 25.0% after adding a common explanation of existence. Neither reached the unchanged 75% threshold, so neither proceeded to prefix evaluation. The final round explicitly made the background convention part of the task and clarified that validity must be evaluated under that convention. It also considered all six pairs of four frozen wording families instead of requiring a fixed anchor wording. These changes preceded the final clean calibration; no wording was chosen using prefix performance.

This was iterative, development-informed prompt design, not independent calibration replications. The intervals describe the observed pairs and prefix seeds under the selected interfaces; they do not quantify variation over all possible prompts or remove dependence from overlapping training folds.

The crossed result concerns Qwen3.6 under the selected wordings. Qwen3-8B failed an earlier development convention-following gate under the prompts tested there; that failure is not evidence of a null prefix response. The earlier one-wording experiment used a different eligible population and changed the quantifier explanation along with the convention. It is retained only in the artifact, not treated as a replication or pooled with this crossed comparison.

D.3 Same-answer results under matched output words

The follow-up uses `VALID` for valid and `INVALID` for invalid in all three models. Both Qwen models are retrained under this mapping, using the same frozen pair registry, five folds, role assignments, lengths 8/16, seeds 50–53, 100 steps, and optimizer settings as Gemma. The Gemma results reuse its aligned-verbal prefixes. All configurations and paired exact-norm Gaussian controls are included.

Fresh Qwen calibration scores all 1,024 rendered benchmark examples under the exact mapping. Overall clean accuracy is 97.17% for Qwen3.6 and 93.75% for Qwen3-8B; these are full-benchmark, not test-only, values. Every candidate answer pair has equal continuation-token counts, and every training fold has nonempty balanced cells. Model-specific eligibility leaves eight, seven, and nine pairs for Qwen3.6, Qwen3-8B, and Gemma.

We also intersect the clean-correct wordings across all three models and evaluate the already-trained prefixes on those wordings. Six pairs remain eligible for every model. This matches evaluation rows, not training subsets or native model wrappers. Both tables below use crossed pair/seed intervals as defined in Appendix C.2.

Table 14: Fixed VALID/INVALID results on each model's eligible examples. Flip gaps are percentage points. Excess margin subtracts the matched Gaussian gap. Intervals are marginal rather than adjusted for all comparisons.

Model	Targeted group	Pairs	Flip gap [95% int.]	Margin gap [95% int.]	Excess margin [95% int.]
Qwen3.6 MoE	Dependent	8	50.8 [31.6, 69.1]	1.280 [0.241, 2.597]	1.708 [0.703, 2.971]
Qwen3.6 MoE	Independent	8	15.0 [-0.5, 28.3]	1.216 [0.512, 1.993]	0.813 [0.222, 1.456]
Qwen3-8B	Dependent	7	63.2 [39.0, 84.8]	2.492 [1.138, 3.802]	2.557 [1.195, 3.830]
Qwen3-8B	Independent	7	13.4 [-0.3, 30.1]	2.164 [1.460, 2.847]	2.067 [1.394, 2.702]
Gemma 4 31B	Dependent	9	1.2 [-7.6, 10.8]	-0.051 [-0.098, -0.009]	-0.003 [-0.033, 0.032]
Gemma 4 31B	Independent	9	6.1 [-4.5, 17.4]	0.099 [0.056, 0.144]	0.051 [0.024, 0.081]

Table 15: Fixed VALID/INVALID results on the same clean-correct wordings, giving six eligible pairs for every model. Flip gaps are percentage points. Excess margin subtracts the matched Gaussian gap. Intervals are marginal rather than adjusted for all comparisons.

Model	Targeted group	Pairs	Flip gap [95% int.]	Margin gap [95% int.]	Excess margin [95% int.]
Qwen3.6 MoE	Dependent	6	48.6 [29.2, 68.4]	1.117 [0.026, 2.454]	1.575 [0.558, 2.869]
Qwen3.6 MoE	Independent	6	21.4 [8.5, 36.1]	1.572 [0.757, 2.425]	1.097 [0.511, 1.747]
Qwen3-8B	Dependent	6	55.7 [28.0, 80.7]	2.505 [0.977, 3.997]	2.473 [0.976, 3.909]
Qwen3-8B	Independent	6	17.4 [0.3, 39.9]	1.787 [0.893, 2.560]	1.791 [0.880, 2.571]
Gemma 4 31B	Dependent	6	-5.6 [-19.3, 6.2]	-0.080 [-0.207, 0.042]	-0.002 [-0.032, 0.034]
Gemma 4 31B	Independent	6	8.3 [-1.0, 20.0]	0.135 [0.017, 0.258]	0.056 [0.018, 0.099]

The main Qwen gaps remain large on both populations. Reverse-role flip intervals include zero on model-specific rows but are above zero on the shared subset, so binary separation in the reverse direction remains population-sensitive. Qwen's control-corrected margin intervals are positive in both assignments.

Gemma's original fixed-mapping study included invalid=A, invalid=B, and aligned verbal answers, both role assignments, and the same five-fold length/seed grid. All six crossed flip-gap intervals include zero. Under aligned verbal answers and independent targeting, the learned-minus-random margin gap is 0.051 [0.024, 0.081] on its full eligible population. The two-sided pair sign test gives $p = 0.0391$, or Holm-adjusted $p = 0.2344$ across those six conditions. Restricting the present display to the aligned vocabulary does not remove that broader multiplicity context. We therefore describe the small Gemma margin signal as exploratory.

The fixed-mapping comparison is descriptive across models. It does not include a direct paired test of the cross-model difference, and differences in wrappers, training eligibility, score scales, and optimization remain. Shared wordings narrow the comparison without isolating architecture as its cause.

D.4 Presentation changes and exact duality

We evaluate the same validity-trained prefixes under rephrased instructions, reversed premise order, and the exactly equivalent dual task, without retraining. The dual presents $P_1, P_2, \neg C$, asks whether those statements can be true together, and converts the response to the corresponding inconsistency truth value:

$$P_1, P_2 \models C \quad \Longleftrightarrow \quad P_1, P_2, \neg C \text{ is unsatisfiable.}$$

Both views preserve the Boolean answer interface and fixed randomized A/B mapping. Clean-correct eligibility is evaluated under each test view.

Rephrasing and premise swapping preserve substantial gaps, but exact-dual evaluation does not. Thus, the learned separation is not invariant to expressing the same logical property as a different task. Training directly on the dual yields pair-weighted gaps of 14.9 and 20.7 points for the two Qwen3.6 role assignments, -4.6 and 11.1 for Qwen3-8B, and between -6.2 and $+5.6$ for Gemma; every clustered interval includes zero.

Table 16: Targeted-minus-protected flip gaps in percentage points for validity-trained semantic prefixes targeting assumption-dependent forms. These are configuration-level means with view-specific eligibility, not the pair-weighted estimates of 49.7 and 62.0 points in Table 8.

Model	Source	Rephrased	Premises swapped	Exact dual
Qwen3.6 MoE	47.6	47.2	52.3	-5.1
Qwen3-8B	60.1	38.6	59.4	-2.5

D.5 Matched nonlogic controls

Arithmetic and string tasks use the same outer prompt and A/B answer interface. Depending on role and training view, Qwen semantic prefixes flip 11–28% of arithmetic examples and 16–31% of string examples. These changes identify a broad interface component. Such a component can contribute to a same-answer gap only if it interacts differently with the two syllogistic groups.

D.6 Balanced figure grouping

Syllogistic figure describes the position of the shared term in the two premises. We use it to test whether a prefix can respond differently to groups defined by formal structure. Each of the four figures contains 64 forms, including six valid and 58 invalid forms. We group figures 1/4 and figures 2/3. Using four label-balanced folds, the prefix is trained to reverse the correct answer for one group while preserving it for the other, and we repeat the experiment with the groups swapped. Targeted-valid, targeted-invalid, protected-valid, and protected-invalid cells receive equal weight, so favoring one answer overall cannot satisfy the objective. We also compare against one arbitrary grouping of forms fixed before training. This control is distinct from the 50 constrained groupings of the semantic pairs.

Table 17: Class-balanced figure-conditioned results. Intervals resample forms within all four answer-by-family cells and resample prefix configurations.

Model	Targeted family	Targeted flip	Protected flip	Gap [95% interval]
Qwen3.6 MoE	Figures 2/3	39.2%	35.7%	3.4 [-10.0, 16.5]
Qwen3.6 MoE	Figures 1/4	39.9%	36.5%	3.4 [-9.3, 15.9]
Qwen3-8B	Figures 2/3	26.6%	28.9%	-2.3 [-18.0, 12.9]
Qwen3-8B	Figures 1/4	37.3%	30.5%	6.8 [-8.6, 21.5]
Gemma 4 31B	Figures 2/3	5.8%	9.5%	-3.7 [-8.5, 0.8]
Gemma 4 31B	Figures 1/4	9.3%	7.0%	2.2 [-2.5, 6.9]

Arbitrary group assignments give gaps of 0.3 points for Qwen3.6 MoE, -6.8 for Qwen3-8B, and -0.9 for Gemma; every interval crosses zero. Dual-view structural conditions are also near zero.

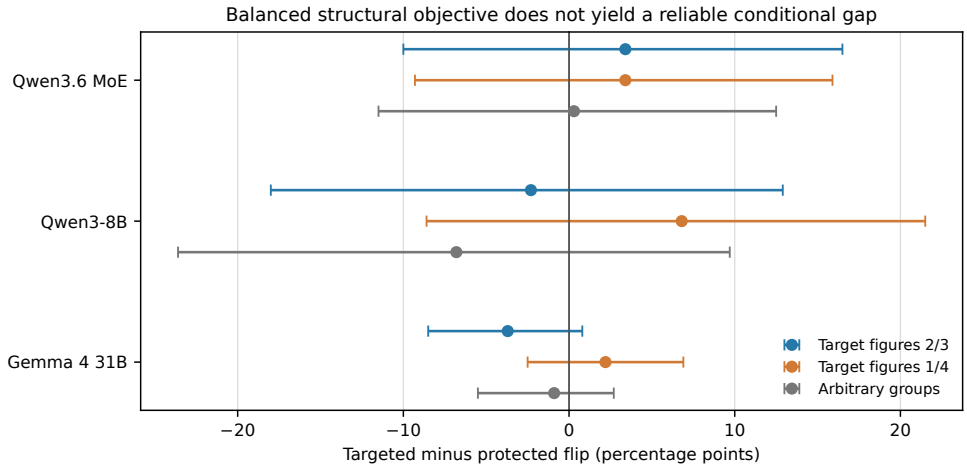

Figure 4: Class-balanced figure-group gaps and a single fixed arbitrary-group control. No reported interval excludes zero under this protocol. The more direct random-grouping comparison for the semantic pairs is reported in Appendix D.1.

E Broad Steering and Answer-Interface Sensitivity

Broad-target experiments establish that learned context can redirect answers. Because both terms of the binary objective favor the same answer, these results alone do not distinguish logical selectivity from answer preference.

E.1 Transfer, variability, and generated answers

Table 18 reports transfer to new surface forms and rephrased prompts. It separates uncertainty over logical forms from uncertainty that also resamples trained prefixes. The repeated-split results then test dependence on the training, development, and test partition.

Table 18: Continuation-scoring results for all three models. The first 95% interval resamples complete targeted logical forms; the second independently resamples forms and trained prefixes. Prefix counts refer to distinct prefixes. Gemma validity uses the answer-only 1/0 interface.

Model	Direction	Test condition	Forms	Prefixes	Flip, forms	Flip, forms and prefixes	Damage	Target-answer rate, eligible rows
Qwen3.6 MoE	Unsat. → sat.	Matched words, meaningful, A/B	6	8	85.4% [79.2, 91.7]	85.4% [60.4, 100.0]	0.5%	98.0%
Qwen3.6 MoE	Unsat. → sat.	New wording, random strings, A/B	6	24	90.3% [85.4, 94.4]	90.3% [79.9, 97.9]	1.8%	97.4%
Qwen3.6 MoE	Unsat. → sat.	Rephrased prompt, all wordings, A/B	6	32	75.8% [71.1, 80.9]	75.8% [65.1, 85.2]	1.6%	96.2%
Qwen3.6 MoE	Valid → invalid	Matched words, repeated, A/B	6	8	89.6% [77.1, 97.9]	89.6% [68.8, 100.0]	0.0%	99.0%
Qwen3.6 MoE	Valid → invalid	New wording, random strings, A/B	6	24	73.6% [61.1, 85.4]	73.6% [54.9, 89.6]	0.1%	97.5%
Qwen3.6 MoE	Valid → invalid	Rephrased prompt, all wordings, A/B	6	32	72.0% [56.1, 83.6]	72.0% [53.6, 86.2]	0.1%	97.3%
Qwen3-8B	Unsat. → sat.	Matched words, meaningful, A/B	6	8	93.8% [89.6, 97.9]	93.8% [79.2, 100.0]	7.6%	92.6%
Qwen3-8B	Unsat. → sat.	New wording, random strings, A/B	6	24	71.5% [63.2, 79.2]	71.5% [54.9, 86.1]	12.7%	85.6%
Qwen3-8B	Unsat. → sat.	Rephrased prompt, all wordings, A/B	6	32	71.6% [65.8, 76.2]	71.6% [60.5, 81.6]	8.1%	89.9%
Qwen3-8B	Valid → invalid	Matched words, repeated, A/B	6	8	81.2% [70.8, 91.7]	81.2% [58.3, 97.9]	17.0%	82.8%
Qwen3-8B	Valid → invalid	New wording, random strings, A/B	5	24	52.5% [40.8, 64.2]	52.5% [33.3, 70.8]	16.0%	80.9%
Qwen3-8B	Valid → invalid	Rephrased prompt, all wordings, A/B	6	32	56.8% [48.3, 65.1]	56.8% [45.6, 68.0]	18.0%	79.6%
Gemma 4 31B	Unsat. → sat.	Matched words, meaningful, A/B	6	8	40.9% [22.7, 58.3]	40.9% [13.6, 70.0]	1.2%	93.5%
Gemma 4 31B	Unsat. → sat.	New wording, random strings, A/B	6	24	39.6% [29.9, 50.0]	39.6% [21.5, 59.0]	1.4%	92.6%
Gemma 4 31B	Unsat. → sat.	Rephrased prompt, all wordings, A/B	6	32	42.4% [28.6, 55.0]	42.4% [24.8, 59.0]	0.9%	93.6%
Gemma 4 31B	Valid → invalid	Matched wordings, 1/0	6	32	60.9% [50.0, 70.3]	60.9% [43.2, 78.1]	0.2%	95.8%
Gemma 4 31B	Valid → invalid	New wordings, 1/0	6	32	56.2% [48.1, 64.8]	56.2% [39.2, 72.7]	0.3%	95.5%
Gemma 4 31B	Valid → invalid	Rephrased prompts, 1/0	6	32	54.4% [46.0, 63.2]	54.4% [37.7, 70.8]	0.7%	94.9%

Table 19: Targeted flip rates across four logical-form partitions; L/R denotes learned/random. Each partition places six of the 24 minority forms in test, rotates training and development forms, and averages eight learned prefixes and their paired random controls over four renderings under the standard prompt. Learned flip exceeds random flip in all 16 comparisons, with substantial partition sensitivity. Rephrased prompts were not repeated across partitions.

Model	Direction	Fold 0, L/R	Fold 1, L/R	Fold 2, L/R	Fold 3, L/R
Gemma 4 31B	Unsat. → sat.	42.7/5.6	58.5/3.3	42.9/3.6	50.2/2.6
Gemma 4 31B	Valid → invalid	90.6/4.2	97.9/0.5	99.0/0.0	92.7/0.0
Qwen3.6 MoE	Unsat. → sat.	84.9/3.6	92.2/1.0	87.5/2.1	70.1/7.6
Qwen3.6 MoE	Valid → invalid	56.4/1.4	87.2/0.5	79.2/1.6	93.6/24.0

Model	Direction	Learned median [range]	Random median [range]
Gemma 4 31B	Unsat. → sat.	46.5 [42.7, 58.5]	3.5 [2.6, 5.6]
Gemma 4 31B	Valid → invalid	95.3 [90.6, 99.0]	0.3 [0.0, 4.2]
Qwen3.6 MoE	Unsat. → sat.	86.2 [70.1, 92.2]	2.9 [1.0, 7.6]
Qwen3.6 MoE	Valid → invalid	83.2 [56.4, 93.6]	1.5 [0.5, 24.0]

The fixed Gemma validity split gives 60.9% matched-wording flip, below its four repeated-split values. Because training, development, and test forms all rotate, this difference measures sensitivity to the full partition, not just to the six test forms. The original six test forms reach 100% flip with prefixes learned under their repeated-split configuration.

Table 20 summarizes the full Qwen3.6 A/B grid: four wordings, two lengths, and four seeds per direction. The broad range shows prefix-to-prefix variability; it should not be conflated with the performance of development-selected prefixes.

Table 20: Distribution across Qwen3.6 MoE prefixes in the final same-wording A/B suite. Flip is measured on targeted test examples and damage on examples from the other class. The broad range shows prefix-to-prefix variation, while the high medians show that the continuation effect is not confined to one selected prefix.

Direction	Prefixes	Flip mean	Flip median	Flip IQR	Flip range	Damage mean	Median target-answer rate
Unsat. → sat.	32	84.9%	100.0%	83.3–100.0%	16.7–100.0%	0.2%	98.4%
Valid → invalid	32	75.2%	83.3%	50.0–100.0%	0.0–100.0%	0.0%	98.4%

Table 21: Generated-answer results for prefixes selected on development data and tested once on unseen forms. The table does not estimate the average behavior of every trained prefix. Gemma validity generation is omitted because clean outputs are not parsed reliably under the current parser.

Model	Setting	Targeted rows	Other-class rows	Flip	Preservation	Unparseable
Qwen3.6 MoE	Unsat. → sat., A/B	144	1331	97.2%	100.0%	0.0%
Qwen3.6 MoE	Valid → invalid, A/B	132	1392	97.7%	100.0%	0.0%
Qwen3.6 MoE	Valid → invalid, 1/0	132	1392	100.0%	100.0%	0.0%
Qwen3-8B	Unsat. → sat., A/B	144	1116	66.7%	91.7%	4.5%
Qwen3-8B	Valid → invalid, A/B	124	1292	80.6%	98.8%	0.0%
Gemma 4 31B	Unsat. → sat., A/B	140	1266	92.9%	99.6%	0.0%
Gemma 4 31B	Unsat. → sat., 1/0	144	1308	94.4%	100.0%	0.0%

Generated-answer results concern prefixes selected on development data and evaluated once on unseen forms. Gemma validity and Qwen3-8B validity with 1/0 answers are omitted from this suite because their outputs could not be parsed reliably; an omitted generation result is not a null steering result.

E.2 Reverse directions

The reverse-direction suites target the majority answer toward the minority answer. Qwen3-8B was not included. These directions generally give weaker flip rates than the headline minority-to-majority conditions. For Gemma validity, weight 1.0 is the primary objective; weight 2.0 is a separate sensitivity condition.

Table 22: Mean test flip rates in the reverse same-task directions. Gemma validity uses the answer-only interface. Weight 1.0 matches the main objective and is primary; weight 2.0 is shown as a separate sensitivity result. Random flip is the matched random-prefix mean from the same suite.

Model	Format	Direction	Suite	Learned flip	Random flip
Qwen3.6 MoE	A/B	Invalid → valid	Main	31.1%	0.3%
Qwen3.6 MoE	A/B	Satisfiable → unsatisfiable	Main	32.8%	1.9%
Qwen3.6 MoE	1/0	Invalid → valid	Main	39.8%	0.0%
Qwen3.6 MoE	1/0	Satisfiable → unsatisfiable	Main	25.2%	1.8%
Gemma 4 31B	A/B	Satisfiable → unsatisfiable	Main	7.9%	4.7%
Gemma 4 31B	1/0	Satisfiable → unsatisfiable	Main	10.0%	2.2%
Gemma 4 31B	1/0	Invalid → valid	Weight 1.0	18.2%	3.4%
Gemma 4 31B	1/0	Invalid → valid	Weight 2.0	6.5%	3.4%

E.3 Counterbalanced Gemma output vocabularies

This follow-up tests A/B, 1/0, X/Y, and `VALID`/`INVALID`, with both label-to-symbol assignments within each answer pair. Each mapping is calibrated separately and trained with length 8, seeds 50–53, 100 steps, and the same fold-0 broad-target design. Candidate continuations have equal token counts within each answer pair. The task and underlying form partition are fixed, but clean-correct training and evaluation eligibility can change with the mapping.

Table 23: Counterbalanced Gemma broad-target output vocabularies, averaging all four seeds per assignment. Clean accuracy covers all evaluated benchmark rows. The inverted verbal assignment uses `VALID` to mean invalid; its low clean accuracy makes its apparent flip rate unsuitable as steering evidence.

Symbol for invalid	Clean accuracy	Targeted flip	Range across seeds	Other-class damage
`A`	89.8%	83.3%	45.8–100.0%	0.0%
`B`	91.8%	0.0%	0.0–0.0%	0.4%
`1`	69.1%	2.1%	0.0–8.3%	10.9%
`0`	93.8%	1.1%	0.0–4.3%	0.6%
`VALID`	4.7%	84.4%	75.0–87.5%	12.5%
`INVALID`	93.4%	67.4%	43.5–82.6%	0.0%
`X`	92.6%	31.5%	0.0–95.7%	0.0%
`Y`	93.8%	55.4%	13.0–95.7%	0.2%

Gemma is strongly steerable toward A and aligned `INVALID`, but much less so toward B or the digits. X/Y effects vary substantially across seeds. The inverted verbal condition has only 4.7% clean accuracy and is excluded from the substantive steering interpretation, not from the reported table. These comparisons establish output-interface sensitivity rather than isolating token geometry as the cause.

These broad-target results are distinct from the same-answer comparison under aligned verbal answers in Appendix D.3. Strong steering under an output vocabulary does not imply separation between groups sharing a correct answer.

E.4 RANDOM-PREFIX AND DIRECT SCORE-BIAS CONTROLS

Single Gaussian controls match learned-prefix norms but are not selected for high development performance (Table 24). The Gemma validity transfer comparison additionally preserves the pairing between learned and random prefixes across test views, and uses the same answer-only 1/0 interface.

Table 24: Random-prefix controls using the wording style matched to prefix training. These are single random prefixes matched in norm, not the larger 1000-prefix search used for Qwen3.6 MoE. Gemma validity transfer uses the stricter paired evaluation in Table 25.

Model	Task	Format	Direction	Targeted flip range	Damage range
Qwen3.6 MoE	Sat.	A/B	Unsat. → sat.	0.0–0.0%	0.0–0.3%
Qwen3.6 MoE	Sat.	1/0	Unsat. → sat.	0.0–0.0%	0.0–0.8%
Qwen3.6 MoE	Val.	A/B	Valid → invalid	0.0–2.1%	0.0–1.1%
Qwen3.6 MoE	Val.	1/0	Valid → invalid	0.0–0.0%	0.0–0.0%
Qwen3-8B	Sat.	A/B	Unsat. → sat.	0.0–0.0%	0.0–3.4%
Qwen3-8B	Sat.	1/0	Unsat. → sat.	0.0–0.0%	0.0–0.7%
Qwen3-8B	Val.	A/B	Valid → invalid	0.0–0.0%	0.2–1.1%
Qwen3-8B	Val.	1/0	Valid → invalid	0.0–0.0%	0.0–2.3%
Gemma 4 31B	Sat.	A/B	Unsat. → sat.	0.0–9.6%	0.2–7.1%
Gemma 4 31B	Sat.	1/0	Unsat. → sat.	0.0–4.2%	0.0–3.0%

Table 25: Gemma validity transfer under the answer-only 1/0 interface, with six targeted forms and 32 learned/random prefix pairs per test family. Each deterministic Gaussian prefix matches its learned partner in length and Frobenius norm and is reused across views without selection. Brackets are crossed form–prefix 95% intervals; paired differences are percentage points. Target-answer rates use all evaluation rows.

Metric	New wordings	Rephrased prompts
Learned flip	56.2% [39.1, 72.9]	54.4% [37.3, 71.0]
Random flip	0.5% [0.0, 2.4]	0.3% [0.0, 1.2]
Paired difference	55.7 [39.1, 71.9]	54.1 [37.6, 70.5]
Damage, learned / random	0.3% / 4.3%	0.7% / 9.7%
Target-answer rate, learned / random	94.3% / 80.3%	93.0% / 72.8%

A stronger search baseline samples 1,000 norm-matched random prefixes for each headline Qwen3.6 direction, ranks them on development targeted flip minus other-class damage, and tests the top 20 on unseen forms.

Table 26: Results after selecting the strongest random prefixes on development data. The top 20 have mean test damage of 1.4% for unsatisfiable-to-satisfiable and 0.5% for valid-to-invalid.

Direction	Random prefixes	Selected	Dev best flip	Dev best damage	Test mean flip	Test max flip
Unsat. → sat.	1000	top 20	25.0%	0.5%	2.3%	12.5%
Valid → invalid	1000	top 20	50.0%	0.4%	13.9%	30.4%

A direct score-bias baseline adds a constant b to the target-answer score without changing the prompt:

$$\text{score}'(y_t \mid x) = \text{score}(y_t \mid x) + b.$$

For illustration, $b = 2$ produces targeted flips of 75.0% for unsatisfiable-to-satisfiable and 78.3% for valid-to-invalid, with no other-class damage; target-answer rates are 97.7% and 98.0%, respectively. Sufficiently large bonuses force the target answer throughout. These are demonstrations of what a

score shift can produce, not optimized estimates of a prefix effect. In the binary broad-target objective, preservation of the other class is compatible with this global answer preference.

E.5 How well do simple score models describe the response?

Using the margins defined in Appendix B, a one-shift model predicts

$$\widehat{M}_P(x) = M_0(x) + b_P.$$

A two-shift model uses separate offsets for targeted and other-class examples. A class-conditioned affine model additionally fits a separate slope in each class, with ridge penalty 10^{-3}. A class-conditioned isotonic model fits a separate non-decreasing map from unprefixed to prefixed margins within each answer class, using fitted endpoint values outside that class's development input range. Class-conditioned fits receive the true answer class at test time: they are descriptive references, not label-free replacements for the model.

All fits use development scores and are applied unchanged to test. Repeated views are weighted inversely by their count within each answer class and logical form, so each form contributes equally within a class. The fixed-split analysis includes 32 trained prefixes per model/task/format condition. It requires no new model inference.

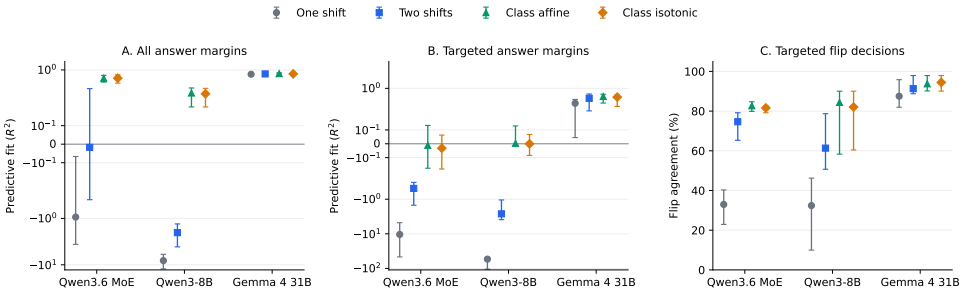

Figure 5: Development-fitted one-shift, two-shift, class-conditioned affine, and class-conditioned isotonic models evaluated on held-out test examples. Panels show (A) fit to all answer margins, (B) fit to targeted-example margins, and (C) agreement on targeted flip decisions. Agreement on binary flips can be high even when final-margin prediction is poor. These models are descriptive score summaries, not causal explanations.

For Gemma, one-shift fits give overall $R^2 = 0.796$–0.837; two-shift fits give $R^2 = 0.816$–0.850 and 89–98% agreement on targeted flips. For Qwen, two shifts capture a shared response component and improve flip prediction, but median R^2 for final targeted margins remains negative in all eight conditions, with six bootstrap intervals wholly below zero. Predicting a flip is therefore easier than predicting its final margin.

Class-conditioned affine fits explain additional variation (Table 27). Across repeated form splits, Gemma retains the stronger overall fit, but the contrast within targeted validity examples is not consistent (Table 28). The results support a difference in how well simple score models summarize the observed responses, not a categorical distinction between model mechanisms.

Table 27: Median test performance of the class-conditioned affine model across 32 trained prefixes per condition. Fits use answer class and unprefixed margin, are learned on development data, and are applied unchanged to test. Brackets are 95% bootstrap intervals across prefixes; flip agreement concerns targeted examples.

Model	Task	Format	Affine all R^2	Affine targeted R^2 [95% interval]	Affine flip agreement
Gemma 4 31B	Sat.	A/B	0.828	0.490 [0.432, 0.615]	90.2%
Gemma 4 31B	Sat.	1/0	0.866	0.375 [0.292, 0.461]	90.3%
Gemma 4 31B	Val.	A/B	0.821	0.674 [0.546, 0.742]	97.9%
Gemma 4 31B	Val.	1/0	0.833	0.673 [0.579, 0.738]	97.2%
Qwen3-8B	Sat.	A/B	0.408	0.129 [0.024, 0.255]	85.4%
Qwen3-8B	Sat.	1/0	0.201	0.005 [−0.033, 0.097]	90.0%
Qwen3-8B	Val.	A/B	0.250	−0.010 [−0.041, 0.081]	58.3%
Qwen3-8B	Val.	1/0	0.386	0.001 [−0.044, 0.219]	83.3%
Qwen3.6 MoE	Sat.	A/B	0.763	−0.045 [−0.181, 0.091]	79.9%
Qwen3.6 MoE	Sat.	1/0	0.546	−0.177 [−0.263, −0.087]	84.7%
Qwen3.6 MoE	Val.	A/B	0.651	0.023 [−0.212, 0.256]	82.0%
Qwen3.6 MoE	Val.	1/0	0.674	0.133 [−0.104, 0.197]	83.3%

Table 28: Class-conditioned affine fit across the four logical-form folds. Each model is fit on development margins and evaluated unchanged on the corresponding test fold. Gemma has the stronger overall fit in every matched task and fold. The contrast within targeted examples is clear in satisfiability but not consistent in validity.

Model	Task	Overall class-affine R^2	Targeted class-affine R^2
Gemma 4 31B	Satisfiability	0.809–0.838	0.473–0.640
Gemma 4 31B	Validity	0.784–0.859	−0.143–0.390
Qwen3.6 MoE	Satisfiability	0.402–0.578	−0.174–0.051
Qwen3.6 MoE	Validity	0.562–0.700	−0.361–0.187

After removing class-specific shifts, residual magnitude still varies across the six targeted forms in the fixed split. This is descriptive: repeated renderings and prefix runs do not increase the number of independent forms. The full score-model comparison, including margin-bin and isotonic fits, is retained in the artifact.

E.6 Neutral readable-text control

A development search over an audited bank of neutral phrases tests whether short readable text reproduces the Qwen3.6 broad-target effect. Across two search seeds, both headline directions, and A/B and 1/0 formats, selected phrases produce 0.0–12.5% development flip but 0.0% test flip in every condition. This limited phrase search does not reproduce the transfer of learned soft prefixes; it is not a general test of optimized discrete prompts.

F Intervention Budgets

The strength of a continuous prefix is difficult to compare directly across models because their embedding spaces have different scales. We normalize the prefix norm by the typical norm of a token embedding:

$$r(P) = \frac{\|P\|_F}{\sqrt{L}\ \mathrm{median}_{v \in V} \|E_v\|_2}.$$

Projected optimization enforces the bound $r(P) \le r$ after each step; it does not force every prefix to attain the bound. The initial, pre-specified sweep uses length 8 and $r \in \{0.05, 0.1, 0.25, 0.5, 1, 2\}$, with seeds 50–53 on a fixed fold-0 partition. The semantic objective targets assumption-dependent

forms, and the figure objective targets figures 1/4. These curves therefore describe a fixed-partition budget study, not the full out-of-fold conditional estimates.

After the initial sweep was complete, a follow-up broad-target extension at $r = 4$, $r = 8$, and the unconstrained setting was specified before those experiments were run. All specified extension conditions are reported, without selecting them by development or test outcomes.

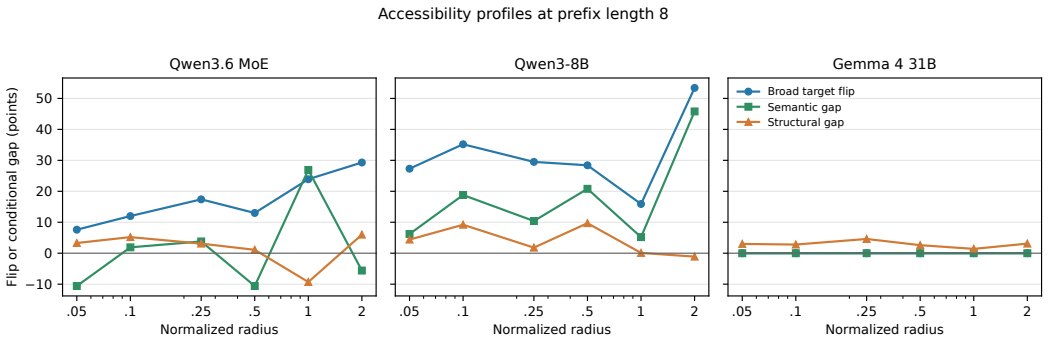

Figure 6: Response profiles at prefix length 8. Broad target flip is a rate; semantic and structural values are targeted-minus-protected gaps. Exact-radius Gaussian controls do not show a consistent gap in the learned targeted direction.

Table 29 reports every point in the pre-specified sweep. Each entry gives the learned result and the exact-radius Gaussian control. Broad-target entries are targeted flip rates; semantic and figure entries are targeted-minus-protected gaps.

Table 29: Complete normalized-radius sweep at prefix length 8. Entries are learned / exact-radius Gaussian point estimates, averaged over four seeds. Broad-target values are percentages; conditional values are percentage-point gaps.

Model	Probe	$r = .05$	$r = .10$	$r = .25$	$r = .50$	$r = 1$	$r = 2$
Qwen3.6 MoE	Broad target	7.6 / 0.0	12.0 / 0.0	17.4 / 0.0	13.0 / 0.0	23.9 / 1.1	29.3 / 0.0
Qwen3.6 MoE	Same-answer semantic	-10.6 / -18.8	1.9 / -18.8	3.8 / -18.8	-10.6 / -16.9	26.9 / -16.9	-5.6 / -21.9
Qwen3.6 MoE	Balanced figure	3.3 / 2.0	5.2 / 2.1	3.1 / 0.4	1.1 / 0.6	-9.3 / 2.2	6.0 / 2.1
Qwen3-8B	Broad target	27.3 / 0.0	35.2 / 0.0	29.5 / 0.0	28.4 / 0.0	15.9 / 0.0	53.4 / 0.0
Qwen3-8B	Same-answer semantic	6.2 / 0.0	18.8 / 0.0	10.4 / 0.0	20.8 / 0.0	5.2 / 0.0	45.8 / 0.0
Qwen3-8B	Balanced figure	4.4 / 0.2	9.2 / 0.3	1.8 / 0.4	9.7 / 0.3	0.1 / -0.1	-1.1 / 0.0
Gemma 4 31B	Broad target	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0
Gemma 4 31B	Same-answer semantic	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0	0.0 / 0.0
Gemma 4 31B	Balanced figure	3.0 / 0.8	2.8 / 1.3	4.6 / 0.7	2.6 / 1.1	1.4 / 1.3	3.1 / 0.7

Table 30: Follow-up high-radius extension for broad targeting under randomized A/B mappings. Entries are learned targeted flip / other-class change in percent, averaged over four seeds. Radius 2 is repeated from the initial sweep as a reference; unlike Table 29, the second number is other-class change, not a Gaussian-control flip rate.

Model	Radius 2	Radius 4	Radius 8	Unconstrained
Qwen3.6 MoE	29.3 / 0.2	69.6 / 16.8	96.7 / 1.3	85.9 / 0.1
Qwen3-8B	53.4 / 13.6	75.0 / 1.3	63.6 / 1.2	63.6 / 1.2
Gemma 4 31B	0.0 / 2.4	1.1 / 3.3	0.0 / 3.2	2.2 / 2.3

For the same-answer validity objective targeting assumption-dependent forms, the largest gap on the pre-specified curve is 26.9 points for Qwen3.6 MoE at radius 1 and 45.8 points for Qwen3-8B at

radius 2. Gemma remains at zero through radius 2. Structural gaps fluctuate around zero. The curves are not monotonic across seeds, so no sharp minimum radius is claimed.

The length sweep evaluates lengths 1, 4, 8, and 16 at a radius selected separately for each model and probe using development behavior. Table 31 reports the complete test results. The length-8 rows reuse the matching configurations from the radius sweep. The pattern is nonmonotonic, and a longer prefix at the same per-position radius has greater total Frobenius norm. These rows therefore vary sequence capacity and total perturbation energy together.

Table 31: Complete prefix-length sweep, averaged over four seeds. Radii were selected on development behavior before test evaluation. Flip values are percentages; gap values are targeted minus protected percentage points.

Model	Probe	Selected r	Metric	Length 1	Length 4	Length 8	Length 16
Qwen3.6 MoE	Broad target	2.00	Flip	71.7	30.4	29.3	55.4
Qwen3.6 MoE	Same-answer semantic	1.00	Gap	70.0	46.2	26.9	-11.9
Qwen3.6 MoE	Balanced figure	0.05	Gap	3.4	0.0	3.3	-3.9
Qwen3-8B	Broad target	2.00	Flip	56.8	69.3	53.4	51.1
Qwen3-8B	Same-answer semantic	2.00	Gap	32.3	47.9	45.8	12.5
Qwen3-8B	Balanced figure	2.00	Gap	-2.6	1.6	-1.1	2.5
Gemma 4 31B	Broad target	0.50	Flip	0.0	0.0	0.0	0.0
Gemma 4 31B	Same-answer semantic	0.05	Gap	0.0	0.0	0.0	0.0
Gemma 4 31B	Balanced figure	0.50	Gap	1.3	2.1	2.6	2.2

G Supporting Activation Analyses

These analyses examine selected broad-target prefixes and are descriptive supplements to the behavioral probes. They characterize where the final-answer effect can be interrupted and how activation changes develop across layers; they do not identify a reasoning circuit or decode a stored logical variable.

G.1 Activation patching

The unprefixed run uses the ordinary prompt, with no dummy prefix. Both runs are passed through the model using input embeddings and an attention mask; position IDs are not supplied separately. The models therefore assign positions in sequence order, so a learned prefix of length m shifts every original prompt token forward by m positions. Prompt token i in the unprefixed run is aligned by content with position $i + m$ in the prefixed run. Consequently, these comparisons do not isolate the effect of learned prefix content from the accompanying positional shift.

A forward hook replaces the residual-stream output after one transformer block at the selected aligned positions. The model-specific block paths differ, but the hook is applied at the same point after the chosen block in all three models. Prefix positions are never patched. The answer position is the final context token immediately before the allowed answer continuation.

The three conditions copy the syllogism tokens, the answer token, or every non-prefix prompt token. The last condition includes the instruction, syllogism delimiters, label mapping, and answer cue. It is intentionally broad: it tests whether restoring the prompt context at one layer can interrupt the answer change, not which token or component is responsible. These prompt regions were not patched separately. For unprefixed, prefixed, and patched next-token target-minus-source logit margins $M_0, M_P, M_{\text{patch}}$, restoration is

$$R = \frac{M_P - M_{\text{patch}}}{M_P - M_0}.$$

A value near one means that patching removes most of the prefix-induced margin change; a value near zero means little restoration.

Table 32: Restoration at the latest tested nonfinal layer. A value near 1 means that copying the unprefixed activation into the prefixed run removes most of the target-margin change. Copying all prompt tokens restores substantially more than copying only the syllogism or answer token.

Model	Cases	Layer	All prompt tokens R	Answer token R	Syllogism tokens R
Qwen3.6 MoE	36	35 / 40	0.80	0.05	0.00
Qwen3-8B	36	32 / 36	0.82	0.43	0.00
Gemma 4 31B	48	53 / 60	0.74	0.01	0.00

G.2 Layer-wise activation changes

Table 33 summarizes final-layer answer-state and margin changes. Distances and score scales are model-dependent, so their absolute magnitudes are not calibrated for cross-model comparison.

Table 33: Final-layer activation changes for the four prefix groups. Distances compare prefixed and unprefixed answer-token states. Margin shift is the change in the target-answer score minus the source-answer score. Values aggregate the reported direction-specific activation summaries; they are descriptive and are not calibrated across model families.

Model	Prefix group	Directions	Answer-token distance	Target-minus-source margin shift
Qwen3.6 MoE	High-flip learned	3	0.378	13.58
Qwen3.6 MoE	All-example target	4	0.299	16.41
Qwen3.6 MoE	Shuffled	3	0.138	4.04
Qwen3.6 MoE	Random	3	0.081	−0.10
Qwen3-8B	High-flip learned	3	0.395	19.43
Qwen3-8B	All-example target	4	0.571	22.40
Qwen3-8B	Shuffled	3	0.308	16.86
Qwen3-8B	Random	3	0.037	0.81
Gemma 4 31B	High-flip learned	2	0.046	18.28
Gemma 4 31B	All-example target	2	0.050	20.94
Gemma 4 31B	Shuffled	2	0.017	1.34
Gemma 4 31B	Random	2	0.009	0.21



Figure 7: Layer-wise diagnostics for Qwen3.6 MoE: syllogism-token state distance and attention to the prefix.

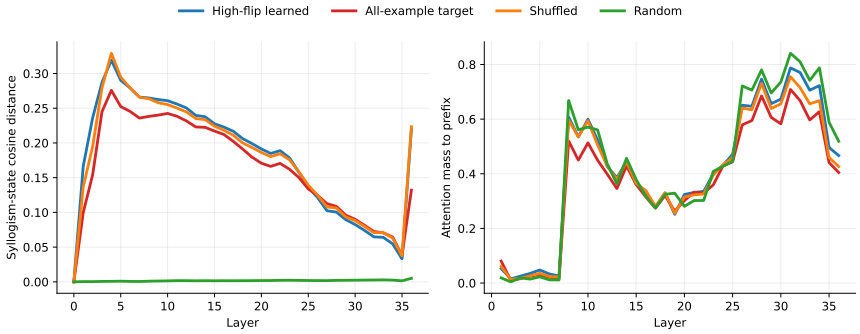

Figure 8: Layer-wise diagnostics for Qwen3-8B: syllogism-token state distance and attention to the prefix.

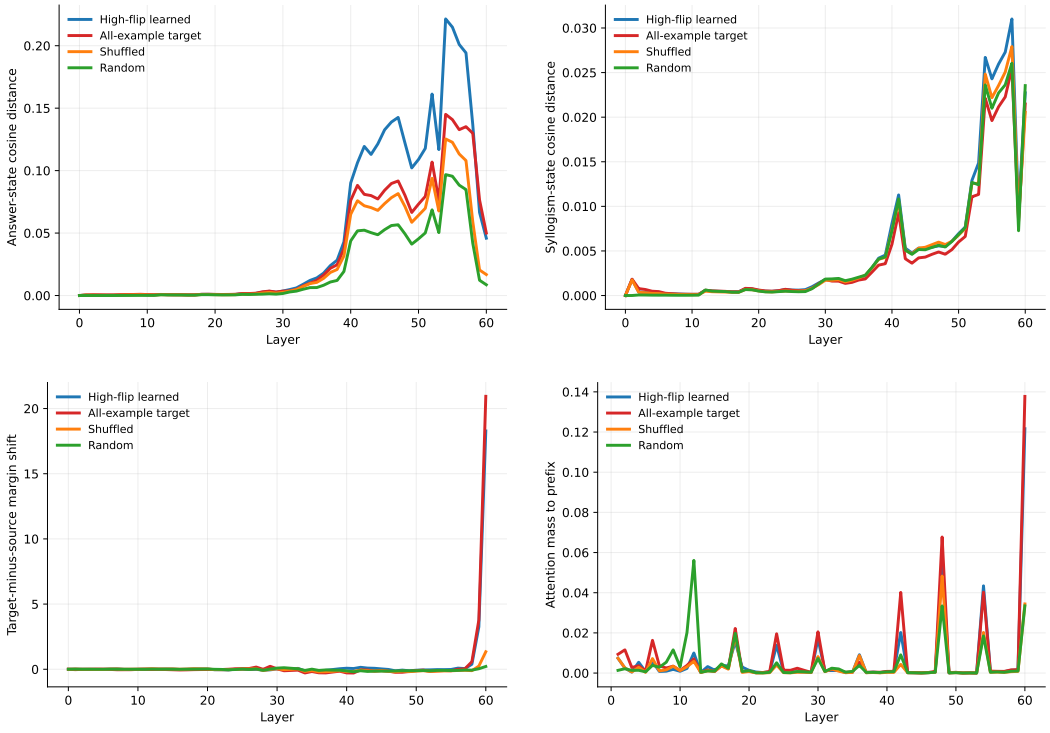

Figure 9: Full layer-wise activation diagnostics for Gemma 4 31B. Top row: answer-token (left) and syllogism-token (right) state distances. Bottom row: target-minus-source margin shift (left) and attention to the prefix (right).

H REPRODUCIBILITY

H.1 MODEL CHECKPOINTS AND SOFTWARE

Table 34: Model checkpoints and revisions. The Qwen3.6 MoE and Gemma checkpoints include multimodal components, but all experiments use text input only.

Model	Public checkpoint	Downloaded revision
Qwen3.6 MoE	`Qwen/Qwen3.6-35B-A3B`	`995ad96eacd98c81ed38be0c5b274b04031597b0`
Qwen3-8B	`Qwen/Qwen3-8B`	`b968826d9c46dd6066d109eabc6255188de91218`
Gemma 4 31B	`google/gemma-4-31B`	`02e15e4990e8c452f8543fb26beff15b1daf8f3d`

All experiments use text input, with models loaded in bfloat16 and without quantization. The recorded environment uses Python 3.12.3, PyTorch 2.10.0 with CUDA 12.8, Transformers 5.12.1, and Accelerate 1.14.0. Optimization, interfaces, and selection procedures are specified in Appendices A.3 and B; experiment-specific deviations are stated with their results.

H.2 COMPUTE AND ARCHIVAL MATERIALS

Runs use up to eight GPUs. Logged wall-clock times range from minutes to under an hour for prompt calibration, roughly 0.5–8 hours for a broad-target run spanning its configurations, and roughly 30 minutes for selected-prefix checks. Best-of-1,000 random searches take several hours per condition; activation analyses take minutes to under an hour per model. Score-model fits run in minutes on CPU from saved scores. These are approximate operational ranges, not normalized hardware benchmarks.