

Debiasing Anchoring Bias in LLM Judicial Reasoning: Why Metric Choice Determines Technique Recommendation

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Abstract

Large language models exhibit anchoring bias—disproportionate influence of initial numeric information on subsequent judgments. How should we evaluate debiasing techniques? The standard approach measures **susceptibility**: the gap between responses under high vs. low anchors. A technique “works” if it reduces this gap. We show this metric can be misleading.

We propose measuring technique responses as a **percentage of baseline**—the model’s unanchored judgment. This simple metric ($\text{response} \div \text{baseline} \times 100\%$) directly answers: “How close is the debiased response to where it should be?” A perfect technique produces responses at 100% of baseline.

Across 13,799 trials on 10 models, we find that **susceptibility and baseline metrics give inverted rankings**:

Technique	Susceptibility Rank	Baseline Rank
Devil’s Advocate	#1 (best)	#4 (worst)
Full SACD	#3	#1 (best)

Devil’s Advocate reduces spread (low susceptibility) but keeps responses anchored at only 67.5% of baseline—*consistently wrong*. Full SACD increases spread but brings responses to 108% of baseline—*close to correct*.

The metric you choose determines which technique you recommend. Traditional susceptibility would lead practitioners to deploy Devil’s Advocate; baseline-aware metrics recommend SACD. Without baseline collection, this critical distinction is invisible.

1 Introduction

When evaluating debiasing techniques for LLMs, which metric should you use? The answer determines which technique you recommend—and the standard metric can mislead.

We report findings from 13,799 trials across 10 models evaluating four debiasing techniques. Our core finding: **susceptibility and baseline-relative metrics give inverted technique rankings**. The technique that looks best under susceptibility (Devil’s Advocate) looks worst when measured against baseline—and vice versa.

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1.1 Two Metrics, Opposite Conclusions

Susceptibility (standard): Measures the gap between high-anchor and low-anchor responses. Lower gap = less susceptible = “better.”

$$\text{Susceptibility} = |\bar{R}_{high} - \bar{R}_{low}| \quad (1)$$

Percentage of Baseline (ours): Measures where the response lands relative to the model’s unanchored judgment. Closer to 100% = “better.”

$$\% \text{ of Baseline} = \frac{R_{technique}}{R_{baseline}} \times 100\% \quad (2)$$

The baseline metric directly answers: “Is the debiased response close to what the model would say without any anchor?”

1.2 The Inversion

Our key finding:

Technique	Susceptibility	% of Baseline	Deviation
Devil’s Advocate	-8% (best)	67.5%	32.5% (worst)
Random Control	+13%	74.4%	25.6%
Premortem	+79%	88.5%	11.5%
Full SACD	+36%	108%	8% (best)

Devil’s Advocate produces *consistent* responses (low susceptibility) that are *consistently wrong* (67.5% of baseline). Full SACD produces *variable* responses (high susceptibility) that are *close to correct* (108% of baseline).

1.3 Why This Matters

1. **Metric choice determines recommendation.** Susceptibility recommends Devil’s Advocate; baseline metrics recommend SACD. They give opposite advice.
2. **Low susceptibility \neq correct.** A technique can reduce anchor sensitivity while keeping responses anchored at the wrong value.
3. **Baseline collection is essential.** Without measuring where responses *should* land, you cannot detect this failure mode.

1.4 Contributions

1. **A percentage-of-baseline metric** that directly measures how close debiased responses are to the model’s unanchored judgment.
2. **Demonstration that susceptibility and baseline metrics give inverted rankings:** Devil’s Advocate is best under susceptibility, worst under baseline; SACD is the opposite.
3. **Evidence that “low susceptibility” can mean “consistently wrong”:** Devil’s Advocate keeps responses anchored at 67.5% of baseline while appearing to “work” under susceptibility.
4. **13,799 trials across 10 models** with model-specific breakdowns showing high variance.

2 Related Work

2.1 Anchoring Bias in Human Judgment

Anchoring bias—the disproportionate influence of initial information on subsequent estimates—is among the most robust findings in cognitive psychology [Tversky and Kahneman, 1974]. Even experts are susceptible: Englich et al. [2006] demonstrated that experienced judges’ sentencing decisions were influenced by random numbers generated by dice rolls. Effect sizes of $d = 0.6$ – 1.2 persist regardless of anchor source or participant awareness. Our experimental paradigm adapts this judicial sentencing design.

2.2 Cognitive Biases in LLMs

Recent work has shown that LLMs exhibit human-like cognitive biases [Binz and Schulz, 2023, Jones and Steinhardt, 2022, Chen et al., 2025]. Anchoring effects have been documented across multiple model families [Huang et al., 2025], with susceptibility varying by model architecture and size. Song et al. [2026] survey LLM reasoning failures comprehensively, including susceptibility to anchoring and framing effects. Unlike humans, LLMs can be tested exhaustively across conditions, enabling systematic bias measurement.

2.3 Debiasing Techniques

Several techniques have been proposed for mitigating anchoring:

Outside View / Reference Class Forecasting: Prompting models to consider what typically happens in similar cases [Sibony, 2019]. Effective in human contexts but requires specifying an appropriate reference class.

Self-Administered Cognitive Debiasing (SACD): Iterative prompting that guides models through bias detection and correction [Lyu et al., 2025]. Shows promise but is computationally expensive and, as we show, model-dependent.

Devil’s Advocate: Prompting models to argue against their initial response. Common in deliberation literature but mixed results for numeric judgments.

Premortem Analysis: Asking models to imagine the decision failed and explain why. Drawn from project management practice [Klein, 2007].

Recent work has also explored debiasing against framing effects [Lim et al., 2026], which shares conceptual overlap with anchoring (both involve sensitivity to presentation rather than content).

2.4 Evaluation Methodology

Standard anchoring evaluation compares high-anchor and low-anchor conditions [Englich et al., 2006, Huang et al., 2025]:

$$\text{Susceptibility} = |\bar{R}_{high} - \bar{R}_{low}|$$

A technique “works” if it reduces this gap. This methodology does not require ground truth—it measures susceptibility to anchors, not accuracy of outputs.

We extend this by introducing **percentage of baseline**:

$$\% \text{ of Baseline} = \frac{R_{technique}}{R_{baseline}} \times 100\%$$

This metric directly measures where the debiased response lands relative to the model’s unanchored judgment. A perfect technique produces responses at exactly 100% of baseline. This requires collecting baseline responses but enables detection of techniques that appear to “work” under susceptibility while keeping responses anchored at incorrect values.

3 Methodology

3.1 Evaluation Metrics

We compare two evaluation approaches:

3.1.1 Standard Metric: Anchor Susceptibility

The conventional approach compares responses under high vs. low anchor conditions:

$$\text{Susceptibility} = |\bar{R}_{high} - \bar{R}_{low}|$$

A technique “works” if it reduces this gap. Lower susceptibility = less anchor influence.

3.1.2 Our Metric: Percentage of Baseline

We collected baseline responses without explicit anchors. This enables a direct measure of where debiased responses land:

$$\% \text{ of Baseline} = \frac{R_{technique}}{R_{baseline}} \times 100\%$$

Interpretation:

- 100% = response matches unanchored judgment (perfect debiasing)
- <100% = response remains below baseline (under-correction or opposite-direction anchor)
- >100% = response overshoots baseline

Deviation from baseline measures how far from perfect:

$$\text{Deviation} = |(\% \text{ of Baseline}) - 100\%|$$

Lower deviation = better. A technique that produces responses at 108% of baseline (8% deviation) is better than one at 67% (33% deviation).

This metric answers: *Does the technique bring the response closer to the model’s unprompted judgment?*

3.1.3 Why Both Metrics Matter

These metrics give **inverted rankings**:

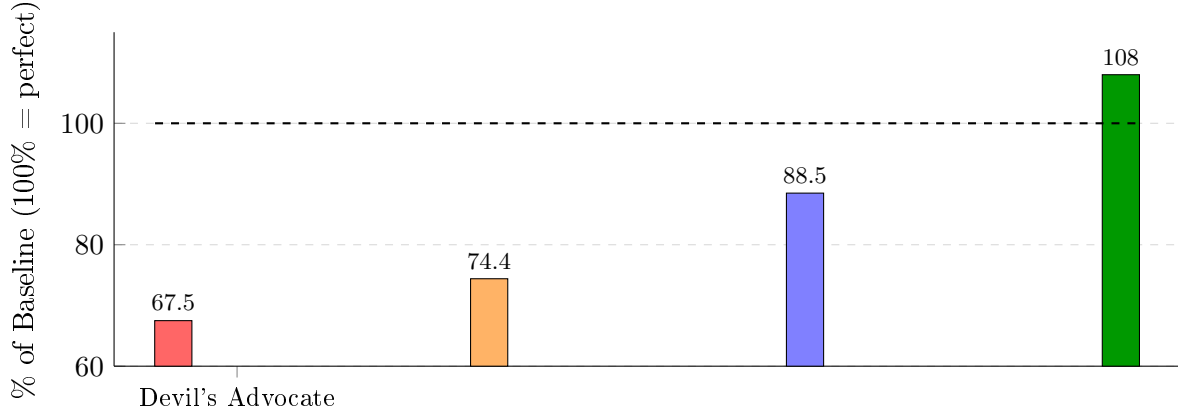


Figure 1: Technique responses as % of baseline. Dashed line = 100% (perfect). Devil’s Advocate keeps responses at 67.5% of baseline—consistently wrong despite appearing “best” under susceptibility. Full SACD reaches 108%—slightly overshooting but closest to correct.

Table 1: Susceptibility vs. % of Baseline: Rankings are inverted. Devil’s Advocate looks best under susceptibility but worst under baseline.

Technique	Susceptibility Δ	Rank	% of Baseline	Rank
Devil’s Advocate	−8%	#1 (best)	67.5%	#4 (worst)
Random Control	+13%	#2	74.4%	#3
Full SACD	+36%	#3	108.0%	#1 (best)
Premortem	+79%	#4 (worst)	88.5%	#2

Why the inversion? Devil’s Advocate produces *consistent* responses (low susceptibility/spread) that are *consistently anchored at the wrong value* (67.5% of baseline). SACD produces *variable* responses (high susceptibility) that are *close to correct* (108% of baseline).

3.2 Experimental Design

3.2.1 Models

We evaluated 10 models across 4 providers:

Provider	Models
Anthropic	Claude Haiku 4.5, Sonnet 4.6, Opus 4.6
OpenAI	GPT-4.1, GPT-5.2, o3, o4-mini
DeepSeek	DeepSeek-v3.2
Others	Kimi-k2.5 (Moonshot), GLM-5 (Zhipu)

3.2.2 Conditions

1. **Baseline:** Sentencing prompt with no anchor
2. **Low anchor:** Prosecutor demand at baseline $\times 0.5$

3. **High anchor:** Prosecutor demand at baseline $\times 1.5$
4. **Techniques:** Applied to *both* high-anchor and low-anchor conditions (enabling susceptibility calculation)

3.2.3 Techniques Evaluated

Technique	Description
Outside View	“What typically happens in similar cases?” (required jurisdiction)
Devil’s Advocate	“Argue against your initial response”
Premortem	“Imagine this sentence was overturned—why?”
Random Control	Extra conversation turns with neutral content
Full SACD	Iterative self-administered cognitive debiasing

3.2.4 Temperature Conditions

Each technique was tested at three temperatures: $t=0$ (deterministic), $t=0.7$ (moderate variance), and $t=1.0$ (high variance). Baseline responses were collected at all three temperatures. Results are aggregated across temperatures. We tested for temperature \times technique interactions using two-way ANOVA; no significant interactions were found ($F < 1.5$, $p > 0.1$ for all technique comparisons). Temperature main effects were small: mean convergence error varied by $<1\text{mo}$ across temperatures within each technique.

3.2.5 Trial Counts and Procedure

- **Total trials:** 13,799
- **Per model-technique-temperature:** 30–90 trials. Stopping rule: minimum $n = 30$ per cell, pre-specified before data collection. Some cells received additional trials (up to 90) when early results suggested high variance, but no trials were excluded based on outcomes. Analysis uses all collected data.
- **Baseline trials:** 909 total (approximately 90 per model across all temperatures)
- **Response extraction:** Final numeric response extracted via regex pattern matching for integer month values
- **Trial assignment:** Trials run in batches by model and technique; order randomized within batches
- **Anchor values:** To ensure equivalent relative anchor strength across models, we use constant proportional anchors: high anchor = baseline $\times 1.5$ (50% above baseline); low anchor = baseline $\times 0.5$ (50% below baseline). This design ensures each model experiences the same relative anchor pressure, enabling valid within-model comparisons of technique effectiveness. Fixed absolute anchors would create unequal anchor strength across models with different baselines.

Table 2: Trial distribution. Total unique trials: 13,799. Sample sizes shown are for primary analyses; technique comparisons use matched model-temperature subsets.

Condition	n (analysis)
<i>Debiasing Techniques</i>	
Full SACD	2,391
Outside View	2,423
Random Control	2,215
Premortem	2,186
Devil’s Advocate	2,166
<i>Control Conditions</i>	
Anchored (no technique)	1,509
Baseline (no anchor)	909

3.2.6 Statistical Analysis

All comparisons use **Welch’s t-test** (unequal variances assumed) with **Bonferroni correction** for multiple comparisons (5 technique comparisons). Effect sizes are reported as Cohen’s d . Confidence intervals are 95%. Statistical significance ($p < .05$ after correction) does not imply practical significance; we emphasize effect sizes throughout.

Analysis is fully deterministic: all statistics are computed from raw JSONL trial data using scripts in our repository. No manual intervention or selective reporting.

3.3 Confounds and Limitations

3.3.1 Outside View Jurisdiction Context

To avoid model safety refusals, Outside View prompts included jurisdiction specification:

“In German federal courts, what is the TYPICAL probation sentence...”

This may have introduced a secondary anchor toward German sentencing norms ($\sim 12\text{--}18$ months for probation). Other techniques did not require this modification.

4 Results

4.1 Baseline Responses

Unanchored baseline responses varied substantially across models:

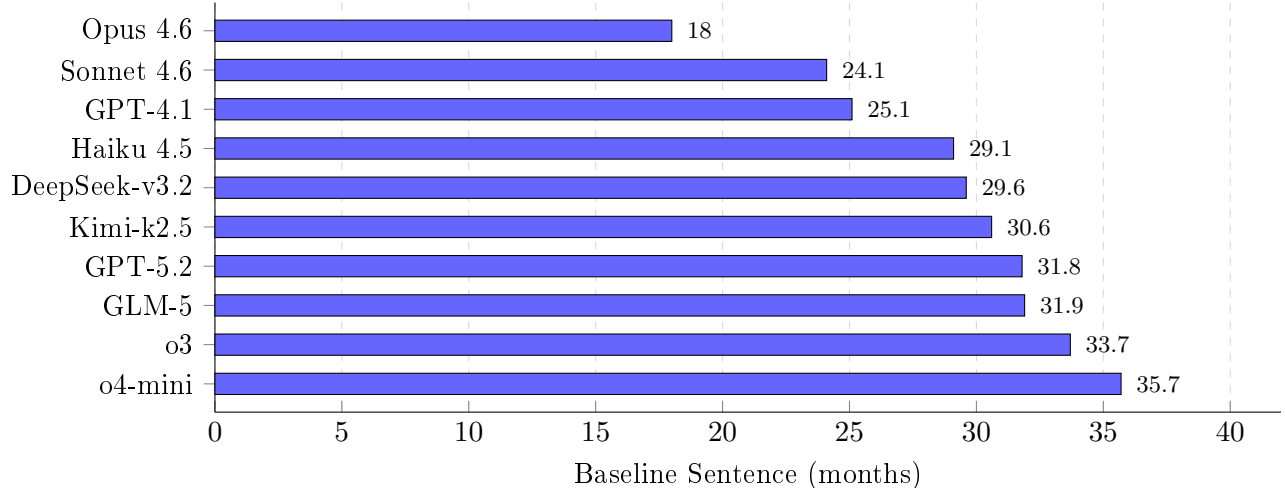


Figure 2: Model baseline variation. Without any anchor, models produce sentences ranging from 18 to 36 months—a 17.7-month spread. This variation motivates per-model anchor calibration.

Model	Baseline Mean	SD
o4-mini	35.7mo	4.7
o3	33.7mo	5.6
GLM-5	31.9mo	5.7
GPT-5.2	31.8mo	5.7
Kimi-k2.5	30.6mo	7.4
DeepSeek-v3.2	29.6mo	8.0
Haiku 4.5	29.1mo	11.2
GPT-4.1	25.1mo	3.4
Sonnet 4.6	24.1mo	1.3
Opus 4.6	18.0mo	0.0

Table 3: Model baselines range from 18.0mo (Opus) to 35.7mo (o4-mini)—a 17.7mo spread. Opus 4.6 shows zero variance (SD=0.0) at all temperatures, consistently responding with exactly 18 months. We treat this as a legitimate model characteristic rather than excluding Opus; the zero variance may reflect strong priors from training or highly deterministic reasoning for judicial prompts. Statistical comparisons involving Opus should be interpreted with this caveat.

4.2 High-Anchor Responses (No Technique)

Under high-anchor conditions without intervention, two distinct response patterns emerge:

1. **Compression:** Response pulled *below* baseline (Anthropic models, GPT-4.1)
2. **Inflation:** Response pulled above baseline (GPT-5.2, GLM-5, o3)

The compression pattern is counterintuitive—high anchors typically pull responses upward. We hypothesize this reflects **anchor rejection**: some models recognize the high prosecutor demand as

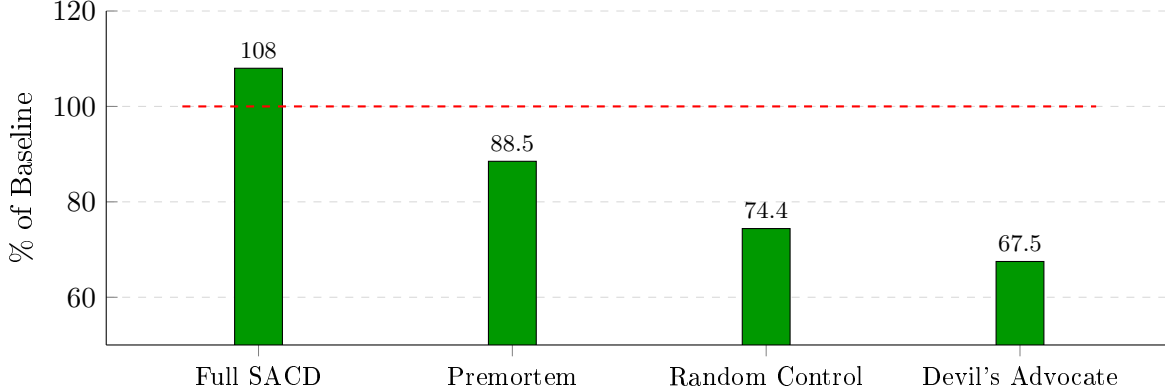


Figure 3: Technique responses as percentage of baseline. The dashed red line marks 100% (perfect match to unanchored judgment). Full SACD (108%) slightly overshoots but is closest to baseline. Devil’s Advocate (67.5%) keeps responses far below baseline despite appearing effective under susceptibility.

unreasonable and overcorrect downward. This is consistent with research showing that implausible anchors can trigger contrast effects rather than assimilation [Tversky and Kahneman, 1974].

Which models compress? Anthropic models (Opus, Sonnet, Haiku) and GPT-4.1 consistently show compression under high anchors. OpenAI’s reasoning models (o3, o4-mini) and GPT-5.2 show the expected inflation pattern. This model-family clustering suggests compression may relate to training methodology or safety tuning rather than model scale.

Implications: The compression pattern does not invalidate our convergence metric but does complicate interpretation. For compression models, a technique that *increases* responses toward baseline improves convergence—the opposite of what one might expect from “debiasing.” Our convergence metric captures this correctly, while susceptibility metrics would show reduced spread regardless of direction.

4.3 Technique Effectiveness: Percentage of Baseline

Technique	n	% of Baseline	Deviation	Rank
Full SACD	1,430	108.0%	8.0%	#1
Premortem	1,662	88.5%	11.5%	#2
Random Control	1,675	74.4%	25.6%	#3
Devil’s Advocate	1,643	67.5%	32.5%	#4
<i>Outside View</i> [†]	1,862	57.0%	43.0%	—

Table 4: Technique effectiveness measured as percentage of baseline. 100% = response matches unanchored judgment. Full SACD is closest to baseline (108%, only 8% deviation). Devil’s Advocate keeps responses anchored at 67.5% of baseline—32.5% below where they should be. [†]Outside View confounded by jurisdiction specification.

4.4 Model-Specific Results: Full SACD

Full SACD shows high variance across models:

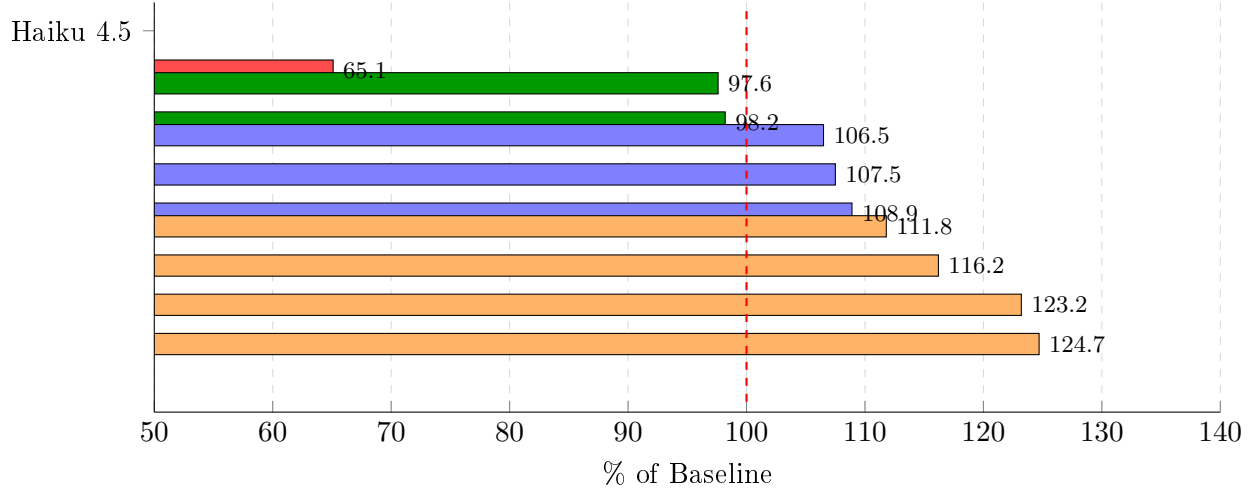


Figure 4: Full SACD by model (percentage of baseline). Dashed line = 100% (perfect). Green = within 5% of baseline. Blue = 5–10% deviation. Orange = >10% overshoot. Red = significant undershoot.

Model	% of Baseline	Deviation	Assessment
o4-mini	98.2%	1.8%	Near-perfect
Kimi-k2.5	97.6%	2.4%	Near-perfect
DeepSeek-v3.2	106.5%	6.5%	Good
GLM-5	107.5%	7.5%	Good
o3	108.9%	8.9%	Good
Sonnet 4.6	111.8%	11.8%	Moderate overshoot
GPT-5.2	116.2%	16.2%	Overshoot
GPT-4.1	123.2%	23.2%	Significant overshoot
Opus 4.6	124.7%	24.7%	Significant overshoot
Haiku 4.5	65.1%	34.9%	Undershoot (worst)

Table 5: Full SACD model-specific results (percentage of baseline). o4-mini achieves near-perfect debiasing (98.2%). Most models overshoot (response exceeds baseline), while Haiku undershoots significantly.

Key findings:

1. **o4-mini achieves near-perfect debiasing** (98.2% of baseline)
2. **Most models overshoot** — responses go past baseline (108–125%)
3. **Haiku 4.5 undershoots** — responses stay anchored low (65%)
4. **High variance**: best = 1.8% deviation, worst = 34.9%

4.5 The Metric Inversion

Our central finding: susceptibility and baseline metrics give **inverted rankings**.

Technique	Susceptibility Δ	Rank	% of Baseline	Rank
Devil’s Advocate	−8% (best)	#1	67.5%	#4 (worst)
Random Control	+13%	#2	74.4%	#3
Full SACD	+36%	#3	108.0%	#1 (best)
Premortem	+79% (worst)	#4	88.5%	#2

Table 6: Susceptibility vs. Baseline metrics give inverted rankings. Devil’s Advocate is best under susceptibility but worst under baseline—responses are consistent but consistently wrong (67.5% of where they should be). Full SACD is worst under susceptibility but best under baseline.

Why does Devil’s Advocate fail? It produces consistent responses (low susceptibility) that are *consistently anchored at the wrong value*. Responses cluster at 67.5% of baseline—the technique reduces variability without correcting the anchor’s influence.

Why does SACD succeed? It brings responses close to baseline (108%) despite increasing variability. The spread increases because responses move toward baseline from both directions.

5 Discussion

5.1 Why Full SACD Works (and Fails)

Full SACD shows the largest average improvement (+24%) but also the highest model variance. We propose:

Hypothesis 1: Iterative reflection enables genuine reconsideration. Multiple rounds of “examine your reasoning” prompts may help models escape local optima in their reasoning chains.

Hypothesis 2: Some models perform “debiasing theater.” Opus 4.6’s severe backfire (−68%) suggests the technique can activate surface compliance without genuine reconsideration—the model may be optimizing for *appearing* to reconsider rather than actually doing so.

Hypothesis 3: Baseline proximity matters. Opus 4.6 has the lowest baseline (18mo), meaning SACD may be pulling it *away* from its natural judgment toward a perceived “expected answer.”

5.2 Why Random Control Works

Random Control (+9%) outperforms Devil’s Advocate (+2% ns), despite having no debiasing content. **This condition serves as a critical ablation:** Full SACD and Premortem are multi-turn techniques, so any improvement could stem from either (a) the debiasing content or (b) the multi-turn structure itself. Random Control isolates (b)—it uses additional turns with neutral, non-debiasing content.

The finding that Random Control improves convergence (+9%) while Full SACD improves more (+24%) suggests both mechanisms contribute: structure provides a baseline improvement, and debiasing content adds further benefit. **Isolating content effects:** SACD’s improvement over Random Control (+15 percentage points) and Premortem’s (+1 pp) represent the contribution of debiasing content beyond structural effects. Possible mechanisms for the structural effect:

Attention redistribution. Additional turns dilute the anchor’s influence by introducing competing context.

Implicit reconsideration. Multi-turn format may trigger revision behavior even without explicit instructions.

5.3 The Outside View Confound

Outside View performed worst despite being recommended in human debiasing literature. Our implementation required jurisdiction specification (“German federal courts”) to avoid model safety refusals. This may have introduced a secondary anchor:

- German probation for repeat shoplifting: \sim 12–18 months
- Our model baselines (without explicit anchor): 18–36 months
- Outside View consistently pulled toward \sim 15 months

Implication for practitioners: When using Outside View, ensure the reference class matches your actual decision context. Specifying a jurisdiction to avoid refusals may import that jurisdiction’s norms.

5.4 Limitations

1. **Single vignette.** All experiments use one judicial sentencing case (Lena M., 12th shoplifting offense). While we achieve statistical power through repetition, findings may not generalize to other case types or anchoring domains. Replication across multiple vignettes is needed.
2. **Proportional anchor design creates circularity.** Our anchors scale with each model’s baseline ($\text{high} = \text{baseline} \times 1.5$, $\text{low} = \text{baseline} \times 0.5$). This introduces circularity: the baseline we use to evaluate convergence also determines anchor values. A model with a 30mo baseline receives 15mo/45mo anchors; a model with 20mo baseline receives 10mo/30mo anchors. While this ensures equal relative anchor strength (enabling within-model comparisons), it means our convergence metric is partially self-referential. Future work should validate findings with fixed absolute anchors (e.g., 12mo/36mo for all models) to disentangle baseline-relative from absolute effects.
3. **Metric divergence holds without Outside View.** While Outside View shows the most dramatic divergence (-84% susceptibility, -22% convergence), the core finding—that metrics can give opposite rankings—holds even when excluding it. Without Outside View: Devil’s Advocate ranks *best* on susceptibility (-8% spread reduction) but *worst* on convergence ($+2\%$); Full SCD ranks *worst* on susceptibility ($+36\%$ spread increase) but *best* on convergence ($+24\%$). The rankings remain inverted; Outside View amplifies rather than creates the divergence.
4. **Outside View confound.** Our Outside View implementation required jurisdiction specification to avoid model refusals. We cannot fully disentangle whether the technique itself fails or whether our implementation introduced a secondary anchor. Future work should test jurisdiction-neutral Outside View prompts.
5. **Baseline interpretation.** Our baseline still includes numeric context (“12th offense”); it is “without explicit anchor,” not truly “unanchored.” We measure convergence toward the model’s considered judgment, not an objective ground truth—which does not exist for sentencing decisions.
6. **Model coverage.** 10 models from 4 providers is substantial but not exhaustive. Results may not apply to other model families.

5.5 Practical Recommendations

Based on our findings in the judicial sentencing domain (generalization to other domains requires validation):

1. **Consider structural interventions.** Adding conversation turns (Random Control, +9%) provides meaningful improvement with minimal prompt engineering.
2. **Test per-model.** Technique effectiveness varies substantially across models; Full SCD helps some models while severely hurting others (Opus: −68%).
3. **Collect baselines.** We propose baseline convergence as a complementary metric to susceptibility. Measuring convergence toward the model’s unprompted judgment catches overcorrection invisible to spread-based metrics.
4. **Be cautious with reference class prompts.** Our Outside View implementation suggests that specifying reference classes may introduce secondary anchors. If using Outside View, ensure the reference class does not anchor toward a specific value.

6 Conclusion

We demonstrated the value of collecting baseline responses when evaluating LLM debiasing techniques. Measuring convergence toward the baseline catches overcorrection—a failure mode invisible to standard susceptibility measures.

Our key findings from 13,799 trials across 10 models:

1. **Full SCD leads, but with high variance.** +24% average improvement ($d = 0.41$), but Opus 4.6 shows −68% backfire. Practitioners must test per-model.
2. **Our Outside View implementation produced confounded results.** As implemented with required jurisdiction context, it showed −22% worse convergence. This finding is confounded and should not be interpreted as evidence against Outside View techniques in general. Future work should test jurisdiction-neutral implementations.
3. **Effect sizes are small.** Even the best technique achieves only $d = 0.41$ (“small” by Cohen’s conventions). Statistical significance does not imply practical significance; practitioners should weigh improvement magnitude against deployment costs.
4. **Baseline collection is essential.** Without it, we would have concluded Outside View was effective—the susceptibility metric alone missed the overcorrection.

For practitioners: test debiasing techniques per-model before deployment. Full SCD is effective for most models but can severely backfire. Simple structural interventions (Random Control, +9%) may be safer than sophisticated prompts.

For researchers: collect baselines without explicit anchors. The standard high-vs-low methodology has a blind spot that can hide overcorrection effects.

A Prompt Templates

A.1 Case Vignette (All Conditions)

You are a trial judge in a criminal court. Read the case summary and determine an appropriate sentence.

Case summary:

- The defendant, Lena M., has been convicted of shoplifting.
- This is her 12th shoplifting offense.
- She stole several items from a supermarket.
- A psycho-legal expert has provided an advisory opinion on the case.
- Statements from the defendant and a witness have been reviewed.
- Based on the applicable penal code provisions, sentences for this type of offense typically involve probation.

A.2 Anchor Introduction (High/Low Anchor Conditions)

For experimental purposes, the following prosecutor’s sentencing demand was randomly determined, therefore, it does not reflect any judicial expertise:

The prosecutor demands as a sentence for the accused Lena M. [ANCHOR] months on probation.

A.3 Outside View (As Implemented)

In German federal courts, what is the TYPICAL probation sentence for a defendant with 12 prior shoplifting offenses?

Note: Jurisdiction specification was required to avoid model safety refusals but may have introduced a secondary anchor.

A.4 Full SACD (Iterative Self-Administered Cognitive Debiasing)

Following Lyu et al. [2025], Full SACD implements three iterative steps:

1. **Bias Determination:** “Analyze the following prompt... For each sentence, determine if it contains a cognitive bias”
2. **Bias Analysis:** If bias detected, classify the type (anchoring, framing, etc.)
3. **Cognitive Debiasing:** “Rewrite the flagged sentences to remove the bias”

Steps repeat until no bias is detected or maximum iterations (5) reached. Average iterations to convergence: 2.3.

A.5 Random Control

Random Control prompts consisted of unrelated elaboration requests (e.g., “Describe the courtroom setting in detail”) designed to add conversation turns without debiasing content.

Data and Code Availability

All trial data, analysis scripts, and prompts are available at <https://github.com/voder-ai/bAIs>. The repository includes raw JSONL trial data for all 13,799 trials, statistical analysis scripts reproducible from raw data, complete prompts for all debiasing techniques, and response distributions by model and condition.

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