Self-Refine Fails for Grounded RAG at Small Scale: From Passage-Overlap Heuristic to Attribution-Guided Refinement

(Version 3 — cross-backbone replication on Llama-3.2-3B, LXT v2.1 attribution reproduction, inverted-trigger finding)

Phume Ngam

April 19, 2026

Abstract

We study whether inference-time self-refinement improves citation faithfulness in retrieval-augmented generation (RAG) at small model scale. We evaluate on two grounded-QA benchmarks with different retrieval noise levels—ALCE-ASQA (5 retrieved passages per question, mostly relevant) and GaRAGe (15 retrieved passages per question, mixed relevance)—across two decoder-only instruction-tuned LLMs (Qwen3-4B and Llama-3.2-3B-Instruct). Our study has two phases plus a cross-backbone replication.

Phase 1 — heuristic refinement ($N = 900$ per condition from 300 questions $\times$ 3 seeds on each benchmark): we compare six conditions including Self-Refine [Madaan et al., 2023] and a simple *passage-overlap heuristic* that triggers a targeted regeneration when the draft answer's content words do not appear in the retrieved passages. Three findings: (1) Self-Refine degrades citation precision on ALCE (-9.37 pts, $p < 0.01$) but slightly helps on GaRAGe (+2.37 pts)—a benchmark-dependent effect. (2) The passage-overlap heuristic significantly beats Self-Refine on both benchmarks (ALCE: +11.45 pts NLI—"NLI citation precision" is a natural-language-inference-based metric checking whether each cited passage entails the cited sentence—plus 85.4% blinded-judge win rate; GaRAGe: +4.72 pts RFCP—"retrieval faithfulness / citation precision," GaRAGe's distractor-aware faithfulness score—58.8% judge) and beats the no-refinement baseline on GaRAGe (+7.09 pts, $p < 0.01$). (3) STR-EM ("string exact-match," standard ASQA metric) is completely blind to these effects.

Phase 2 — attribution-triggered refinement. We replace the passage-overlap heuristic with per-token attribution: a gradient-based scoring of how much each input token contributes to each predicted output token, aggregated to a per-passage score that flags which retrieved passages the model is over-relying on (the "distractor-flagging" signal). We document an attempted transplant of DynamicLRP [Lee & Millan-Arias, 2025] (a Layer-wise Relevance Propagation variant for transformers) onto Qwen3-4B, its failure on our stack (mean ABPC = -0.087, 95% CI $[-0.155, -0.022]$—ABPC, "area between perturbation curves," measures whether an attribution method ranks causally important tokens correctly; positive = faithful), a paper-grounded code review that identified six deviations, and the pivot to plain input$\times$gradient, which produces statistically positive ABPC (+0.077, CI $[+0.023, +0.156]$) on the same samples. We integrate this gradient signal into the same distractor- flagging pipeline with three critique-prompt variants, and compare the winning variant—an *attribution-triggered rewrite* that uses the heuristic's critique phrasing but fires only when attribution indicates distractor overuse—against the Phase 1 conditions on both backbones.

Generation setup for all attribution-triggered rewrite comparisons: temperature 0.7, max 1024 tokens, chat-template-formatted prompts, SDPA (PyTorch's fused attention kernel) + KV cache (standard causal-LM inference optimization that reuses key/value tensors across generation steps), blinded pairwise judge with bidirectional X/Y swap for position-bias control.

GaRAGe results. On Qwen3-4B ($N = 24$ flagged), the attribution-triggered rewrite significantly beats all three baselines: $\Delta = +0.625$ vs Self-Refine (CI $[+0.375, +0.833]$), $\Delta = +0.417$ vs no-refinement baseline (CI $[+0.167, +0.625]$), $\Delta = +0.417$ vs the passage-overlap heuristic (CI $[+0.167, +0.625]$). On Llama-3.2-3B-Instruct ($N = 21$ flagged, attribution via the LXT v2.1 library's AttnLRP patch giving $1.77\times$ ABPC over plain gradient; conservation 0.35, well below the ~ 1.0 reported in Achtibat et al., 2024 on encoder-family benchmarks—a gap we attribute to the autoregressive next-token setting being outside that paper's validation set), the rewrite beats Self-Refine only directionally ($\Delta = +0.286$, CI $[+0.000, +0.571]$, borderline) and *does not reproduce* the baseline win ($\Delta = -0.048$, CI $[-0.381, +0.286]$). The Qwen3-4B +0.417 win over baseline is therefore backbone-specific.

ALCE cross-dataset ($N = 16$ flagged on Qwen3-4B with a label-free attribution-concentration trigger). The attribution-triggered rewrite significantly beats Self-Refine ($\Delta = +0.500$, CI $[+0.188, +0.812]$), is borderline vs no-refinement baseline ($\Delta = +0.188$, CI $[+0.000, +0.375]$), and ties the passage-overlap heuristic ($\Delta = +0.062$, CI $[+0.000, +0.188]$)—consistent with Phase 1's finding that ALCE's cleaner 5-passage retrieval leaves less distractor mass to clean up. Advantage over non-critic baselines is retrieval-noise-dependent.

Scope of positive claim. The attribution-triggered rewrite is a reliable *Self-Refine alternative* across both backbones and both benchmarks (significantly on Qwen3-4B on both; directionally/borderline on Llama-3.2-3B). It is an improvement over no-refinement baseline only on Qwen3-4B. Attribution-concentration as a selective-refinement *trigger* is a documented negative result across all attribution methods and backbones tested (plain gradient on Qwen3, Captum Integrated Gradients on Qwen3, LXT AttnLRP on Llama). A post-hoc threshold-sensitivity analysis of the passage-overlap signal on ALCE reveals an *inverted-trigger* pattern: refinement gains concentrate on low-attribution examples (lowest quartile $\Delta = +0.133$, CI $[+0.040, +0.240]$) and vanish on the highest quartile ($\Delta = +0.040$, CI straddles zero). Threshold-based selective refinement selects against the examples that would benefit most.

We also document three parameter mismatches between our Phase 2 pilot and the Phase 1 setup (`max_new_tokens`, sampling temperature, few-shot demos), and provide an honest efficiency analysis showing that our current implementation costs more per-query than the passage-overlap heuristic, contradicting our initial intuition. Attribution-triggered refinement is a *quality win, not a compute win*, in the current setup.

Contents

1 Introduction

Retrieval-augmented generation (RAG) is the dominant pattern for grounding language models in external knowledge. Yet even with relevant passages in the prompt, models routinely mix faithful citations with unsupported claims from parametric memory—a well-documented failure mode [Liu et al., 2023, Shi et al., 2023]. The problem is acute for small models ($<$ 10B parameters) where the model cannot reliably distinguish "known from training" from "present in the passage."

A popular fix is **Self-Refine** [Madaan et al., 2023]: generate an answer, have the same LLM critique it, regenerate. This works well on reasoning tasks with large models, but Huang et al. [2024] showed that small LLMs cannot self-correct reasoning. Whether Self-Refine helps or hurts *grounding* specifically—where the failure mode is unfaithful citation rather than logical error—is less studied.

This paper asks: **does Self-Refine improve citation faithfulness at small model scale, and can a simpler or more principled alternative do better?**

Study structure. We investigate this in two phases plus a cross-backbone replication:

1. **Phase 1 — Heuristic refinement.** We introduce a trivially cheap *passage-overlap heuristic* (denoted S_3 in formulas) that triggers a targeted regeneration when the draft answer's content words do not appear in the retrieved passages, and show it beats Self-Refine and the no-refinement baseline on two benchmarks ($N = 900$ per condition from 300 questions $\times$ 3 seeds). This establishes the evaluation framework and target to beat.

2. **Phase 2 — Attribution-triggered refinement.** We attempt to replace the passage-overlap heuristic with token-level attribution—first via DynamicLRP [Lee & Millan-Arias, 2025], then via plain input$\times$gradient after DLRP fails on our stack. We integrate the working attribution signal into a distractor-flagging pipeline (flag which retrieved passages the model is over-relying on, then rewrite) with three critique-prompt variants (§4.3.3), judge the output against the Phase 1 conditions, and report honest efficiency numbers. The winning variant—the *attribution-triggered rewrite* (hereafter referred to descriptively; internally labeled `v2c`)—reuses the passage-overlap heuristic's critique phrasing but fires only on examples where attribution flags distractor overuse.

3. **Cross-backbone replication.** We replicate Phase 2 on `Llama-3.2-3B-Instruct` (Meta) using the upstream LXT v2.1 AttnLRP library, which ships production-quality LRP rules for Llama and experimental ones for Qwen3. The replication partially reproduces Phase 2: the

attribution-triggered rewrite still outperforms Self-Refine on Llama-3.2-3B directionally, but the absolute improvement over the no-refinement baseline is Qwen3-specific.

Who this paper is for. We write with a deliberately wide audience in mind: the reader may be a data scientist familiar with general ML but not LLM interpretability, or an LLM researcher familiar with attribution methods but new to grounded RAG. We use the Phase 1 sections to establish task definitions and the Phase 2 sections to walk through mechanistic attribution step by step—including the engineering reality of porting a published method onto a different model architecture, and the judgment calls made when results did not match the paper's Table 1.

Phase 1 contributions. (i) A quantified failure mode of Self-Refine for grounded RAG; (ii) A trivially cheap passage-overlap heuristic that avoids this failure; (iii) A metric pathology diagnosis (STR-EM is blind).

Phase 2 contributions. (i) A full engineering write-up of porting DynamicLRP from LLaMA-3.2-1B (paper's setup) to Qwen3-4B (our stack), the six deviations identified by a paper-grounded code review, and why the transplant fails on the paper's own MoRF/LeRF faithfulness metric. A follow-up with the updated LXT v2.1 library (released after our initial transplant) contextualizes this: upstream now ships an official (but experimental) Qwen3 patch with the same first-token-skew failure mode our transplant exhibits, confirming the negative result is a library-maturity issue rather than an implementation bug. (ii) A paired comparison of DynamicLRP vs input×gradient on the same examples showing plain gradient is significantly more faithful on our stack ($\Delta \mathrm{ABPC}_{\mathrm{DLRP-Gradient}} = -0.164$, CI $[-0.265, -0.075]$, $p < 0.001$), plus a cross-backbone follow-up showing that LXT AttnLRP on Llama-3.2-3B reverses this ranking (LRP/gradient ABPC ratio 1.77×, conservation 0.35)—illustrating that the gradient-vs-LRP ranking is architecture-dependent and scoped to library-supported backbones. (iii) A critique-prompt ablation (§4.3.3) showing that telling the model *which* passages to disregard actively hurts refinement, while using the passage-overlap heuristic's proven critique phrasing with only a binary "something is off" trigger wins. We call this the **binary-trigger insight**: attribution's value is *when to refine*, not *what to criticize*. (iv) An honest efficiency analysis: our current gradient-based pipeline costs $\sim 4.3\times$ per query vs baseline, 2× vs the passage-overlap heuristic, because attribution backward passes dominate. Deployable only when the critic itself is the expensive step (e.g., larger critic model or tool-use). (v) A post-hoc threshold sensitivity analysis of the passage-overlap signal S_3 on ALCE (§3.8) that reveals an **inverted-trigger** pattern: refinement gains concentrate on the *lowest-*S_3 quartile ($\Delta = +0.133$, CI $[+0.040, +0.240]$) and vanish on the highest-S_3 quartile ($\Delta = +0.040$, CI straddles zero). High attribution concentration is not a useful "refine now" signal; it is the opposite. We are not aware of a prior report of this inverse relationship at the generation side.

What we can and cannot claim. We can claim that the attribution-triggered rewrite outperforms Self-Refine on grounded RAG across both tested backbones—significantly on Qwen3-4B (CI $[+0.375, +0.833]$) and directionally on Llama-3.2-3B (CI $[+0.000, +0.571]$, borderline). We can claim that the rewrite outperforms the no-refinement baseline on Qwen3-4B with ground-truth distractor labels, but this does *not* reproduce on Llama-3.2-3B—so that specific baseline-beating result is currently backbone-specific. We cannot claim compute efficiency wins. We cannot claim per-token attribution would improve results in the current passage-level pipeline (Phase 2 Section 4.3.4 argues the opposite). Attribution-concentration as a selective-refinement trigger is a documented negative result across plain gradient (Qwen3), Integrated Gradients (Qwen3), and LXT AttnLRP (Llama) pilots; the inverted-trigger analysis in §3.8 explains mechanistically why. These are explicitly noted as limitations.

2 Related Work

Self-refinement. Self-Refine [Madaan et al., 2023] demonstrated iterative self-critique for code, math, and dialogue. Huang et al. [2024] showed intrinsic self-correction fails on reasoning at small scale, and Stechly et al. [2024] extended this to verification on Game of 24, graph coloring, and STRIPS planning—LLMs cannot reliably self-verify. We extend both findings to the grounding domain and quantify the failure mechanism (distractor citation inflation).

RAG evaluation. ALCE [Gao et al., 2023] augmented ASQA with NLI-based citation metrics. GaRAGe [Kim et al., 2025] introduced per-passage relevance labels and a distractor-aware faithfulness score; headline numbers show current RAG systems deflect irrelevant grounding at only 31% true-positive rate, motivating unsupervised distractor-handling. We evaluate on both, exposing that STR-EM disagrees with all faithfulness-oriented metrics.

Counterfactual context signals. Context-Aware Decoding [Shi et al., 2023] uses the with/without-context delta as a *decoding-time* signal. Our S_3 is conceptually related but used as a post-hoc *routing* signal—a much cheaper intervention that does not modify decoding.

LLM-as-judge. LLM-based evaluation [Zheng et al., 2023] is increasingly used as a practical alternative to human annotation. We use Claude 4.6 as a blinded pairwise judge with explicit anti-length-bias rubrics.

Mechanistic attribution for LLMs. Layer-wise Relevance Propagation (LRP) is a family of attribution methods that propagate output relevance backward through the network using per-operation rules that preserve a conservation property [Bach et al., 2015, Montavon et al., 2019]. AttnLRP [Achtibat et al., 2024] extended LRP to transformer attention. DynamicLRP (DLRP) [Lee & Millan-Arias, 2025] generalizes AttnLRP to an arbitrary autograd graph, automatically deriving propagation rules from traced PyTorch operations. Plain input×gradient is the simplest attribution baseline and has known limitations—magnitude bias, noise sensitivity [Adebayo et al., 2018]—but requires only one backward pass and is architecture-agnostic.

Attribution-guided refinement for RAG. Using attribution to steer RAG output is an active area. Palm & Winther [2023] (locate-and-edit for factuality) and Gupta et al. [2023] (context attribution for grounding) attribute at token level to guide rewriting. The closest methodological neighbor is AttentionRAG [Fang et al., 2025], which uses focal-token attention to *prune* retrieved passages before generation (6.3× context compression). LRP4RAG [Hu et al., 2024] applied module-level LRP to RAG for hallucination *detection*, and Influence-Guided Context Selection [Deng et al., 2025] uses leave-one-out contextual influence as a selection signal (expensive—many forward passes—vs. our single backward pass). Our work differs on three axes: (i) gradient-based LRP vs. raw attention or LOO, (ii) concentration-over-passages as a scalar signal rather than per-passage scores, and (iii) triggering a text-template refinement rewrite rather than pruning, detection, or abstention.

Grounding signals and sufficient-context detection. A growing body of work probes model internals to measure grounding quality. ReDeEP [Sun et al., 2025] decouples external-context use (Copying Heads) from parametric-knowledge use (Knowledge FFN) to detect hallucinations. TPA [TPA, 2025] decomposes token probabilities into seven provenance sources. The most conceptually adjacent work is "Sufficient Context" [Joren et al., 2025], which defines a binary autorater signal for whether retrieved context is sufficient to answer and uses it to drive selective abstention; their finding that RAG actually *reduces* models' ability to abstain is a mirror-image of our selective *refinement* result. Our S_3 signal differs by being (i) unsupervised (no autorater), (ii) a distribution-over-passages statistic rather than a scalar sufficiency flag, and (iii) used to trigger a rewrite rather than to abstain.

Corrective and adaptive RAG. CRAG [Yan et al., 2024] trains a lightweight retrieval evaluator that triggers use-as-is / web-search-fallback / decompose actions. Self-RAG [Asai et al., 2024] trains an LLM to emit reflection tokens driving on-demand retrieval. Adaptive-RAG [Jeong et al., 2024] routes by query complexity. All three require either fine-tuning or a trained classifier; our approach is training-free on an off-the-shelf 4B model, using the model's own gradients as the routing signal.

Inverse alignment–benefit findings. The surprising finding that refinement helps most on *low-*S_3 examples (§3.8) has one published structural analogue in the retrieval literature: Kotte [2026] shows query rewriting *hurts* on queries already well-aligned with relevant documents (FiQA −9.0 nDCG@10) and *helps* on poorly-aligned queries (TREC-COVID +5.1). Our result is the generation-side counterpart at the attribution signal level; we are not aware of a prior report of an inverted attribution-concentration / refinement-benefit relationship.

3 Phase 1: Passage-Overlap Heuristic for RAG Refinement

3.1 Problem Setup

Given a question q, retrieved passages P, and an LLM M, the task is to produce an answer y that is faithful to P. We denote by $A(q, P)$ the baseline answer with standard citation prompting.

3.2 Passage-Overlap Heuristic (score S_3)

We generate two answers: $A = M(q, P)$ (with passages) and $B = M(q)$ (without passages). We compute:

S_3 **(passage-token overlap):** the fraction of content tokens in A (stopwords and citation markers excluded) that appear in any passage, using whitespace tokenization with number-variant expansion. High S_3 indicates the answer's content is lexically present in the passages.

We stress that S_3 is **not** mechanistic attribution—it is a simple string-matching heuristic. It cannot detect cases where the model cites the correct passage but fabricates a specific detail not present in that passage. We use the term "passage-overlap heuristic" throughout to avoid overstating its sophistication.

Why S_3? Three candidate grounding signals considered. The subscript "3" is not cosmetic—it marks S_3 as the third of three signals we prototyped in Phase 1 before settling on it. All three are different angles on the same question: *is the answer actually grounded in the retrieved passages, or is it coming from parametric memory?*

- S_1 — **ROUGE-L F1 between the with-passages answer A and the without-passages answer B.** Intuition: if the same model produces very similar text with or without the passages, the passages are not doing work. **Ideal: low.** *Dropped* because at 4B scale the two outputs were often highly similar even on examples where the answer was clearly distractor-driven; the signal was too noisy to threshold reliably.
- S_2 — **mean per-token log-prob delta** $\log p(A \mid P) - \log p(A \mid \emptyset)$ (where $\emptyset$ denotes the no-passages counterfactual). Intuition: if adding passages raises the model's per-token probability of its own output, it is conditioning on them. **Ideal: high.** *Dropped* for two reasons: (i) cost—each score requires a second full forward pass without passages, roughly doubling compute; (ii) BPE tokenization edge cases produced frequent token-count mismatches between the two passes that we could not robustly reconcile without subprocess-level isolation (see signal computation code for the relevant assertions).
- S_3 — **fraction of content tokens in A that appear verbatim in any retrieved passage** (stopwords and citation markers excluded, with number-variant expansion). Intuition: if the answer's words come from the passages, it is lexically grounded. **Ideal: high.** *Kept* because it is the cheapest (no extra model call, just string match), the most interpretable (a reviewer can inspect which tokens overlap), and produced the cleanest threshold separation in the Phase 1 pilot.

For a grounded answer we therefore expect *low S_1, high S_2, high S_3*—three independent angles on the same underlying phenomenon. We report results using S_3 alone; the fact that S_3 is crude is exactly what motivates the Phase 2 pivot to gradient attribution.

3.3 Datasets, Retrieval Setup, and Benchmark Examples

This subsection covers three things a reader will need later: (a) which benchmarks we use and what shape their data comes in, (b) the shared vocabulary of "N passages," "seeds," "k-shot demos," and the `[cite_N]` citation format, and (c) one representative worked example from each benchmark.

What a "RAG dataset" actually contains. For each question, a grounded-QA benchmark ships three things: (i) the question text, (ii) a *fixed list of retrieved passages* (the benchmark already ran a retriever and froze the outputs so everyone compares against the same passage set), and (iii) one or more gold answers used to score outputs. "Retrieval" happens upstream of us; we never re-retrieve.

ALCE-ASQA. Source: ASQA [Stelmakh et al., 2022], a factoid-QA dataset of ambiguous questions from Wikipedia, extended by ALCE [Gao et al., 2023] with per-question retrieval and citation-scoring infrastructure. Full test set: $\approx$ 948 questions (ALCE dev-test split). We draw a fixed $N = 300$ subset for this paper, chosen by a fixed random seed over the full set. Each question ships with 5 **retrieved passages** (ALCE's default top-5 Wikipedia retrieval; the benchmark provides them and we use them as-is).

GaRAGe. Source: Amazon's GaRAGe benchmark [Kim et al., 2025], designed specifically to stress distractor-handling in RAG. Full test set: $\approx$ 2,366 questions. We draw a fixed $N = 300$ subset, with the stratification described below. Each question ships with 15 **retrieved passages** drawn from mixed sources (papers, tutorials, news, summaries)—retrieval is intentionally noisier than ALCE's.

Why the passage counts differ. 5 vs 15 is a benchmark-design choice, not our choice: ALCE is built around small, focused retrieval sets, while GaRAGe tests distractor resistance and therefore hands the model a wider net that contains both useful and irrelevant passages. Throughout this paper, "clean retrieval" refers to the ALCE regime and "noisy retrieval" to the GaRAGe regime.

"3 seeds" means 3 decoding runs over the same 300 questions. A decoding seed controls the random sampler inside the LLM (which token to pick when the model is about to sample with temperature > 0). For each condition we run the *same* 300 questions three times with different seeds (1337, 2024, 42), giving $N = 900$ (question, output) pairs per condition. This buys statistical power for the bootstrap CIs: a result is averaged across both questions and sampling noise. It is *not* sampling 300 questions three times without replacement—the questions are fixed, only the decoding randomness changes.

"2-shot demos" means 2 worked examples prepended to every prompt. Small instruction-tuned models follow citation-format instructions much more reliably when they see a couple of fully-formatted examples first. A k-shot demo is a (question, passages, gold answer with correct `[cite_N]` citations) triple prepended to the prompt as an in-context example. We use $k = 2$: two worked demos, different from the 300 test questions, show the model the desired output shape. ALCE's demos come from ALCE's released demo pool; GaRAGe's are pinned to questions outside our test 300 so no test example ever appears as its own demo.

"Mixed-relevance examples only" (GaRAGe). GaRAGe stratifies its questions by retrieval quality: some have almost all 15 passages relevant, some have almost none. A question where 0/15 or 15/15 passages are relevant is trivial in opposite ways—a model either has nothing to cite or cannot be wrong. We therefore restrict our GaRAGe subset to questions with *mixed* relevance (at least one relevant and at least one irrelevant passage per question, i.e., the useful middle of the distribution), and sample 100 questions per relevance stratum (low / medium / high *fraction* of relevant passages), interleaved for $N = 300$ total. This is the regime where distractor handling actually matters.

The `[cite_N]` (or `[N]`) citation format. For grounding to be automatically scorable, the model must tell us *which* passage is supporting *which* claim. We do this by asking the model, in its system prompt, to wrap every factual claim with an inline citation marker: `[cite_1]`, `[cite_3][cite_11]`, etc., where the integer refers to the passage position in the retrieved set. ALCE's convention is `[N]` (plain integer brackets); GaRAGe's is `[cite_N]`. We follow each benchmark's convention in its own condition. The NLI citation-precision metric (§3.5) parses these markers to decide which (sentence, passage) pairs to entailment-score.

What follows. Below we show one representative example from each benchmark so that the abstract numbers above attach to concrete data.

ALCE-ASQA example (5 passages, mostly relevant). Retrieval here is usually tight—the answer is typically lexically present in one of the top passages, and most passages are topically on-point.

```
Question: Who plays dr hunt on grey's anatomy?

[1] Grey's Anatomy (season 5): episodes as cardiothoracic
```

```
   surgeon Dr. Erica Hahn, Callie's love interest, who
   eventually resigns and moves away. Patrick Dempsey
   portrayed neurosurgeon Dr. Derek Shepherd ...

[2] Kevin McKidd: a strong audience, the show lost about
   half of its viewership throughout its run and suffered
   from the fractious situation in the United States due
   to the writer's strike at the time. ...

[3] Grey's Anatomy: from "Grey's Anatomy" on November 6,
   2008. "E! Online" Kristin Dos Santos asserted that
   Smith's dismissal from the show had been forced by
   the ABC network ...

[4], [5] ... (additional on-topic passages)

Gold short answer: "Kevin McKidd"
```

In this example the answer entity ("Kevin McKidd") is directly present in passage [2]'s title. A baseline model that simply follows the top-retrieved passages usually answers correctly here; the challenge on ALCE is *not* finding the right passage but producing the answer with proper `[cite_N]` attribution that the NLI citation metric can verify.

GaRAGe example (15 passages, mixed relevance). GaRAGe pairs each question with 15 retrieved passages drawn from heterogeneous sources (technical papers, tutorials, news, summaries). Crucially, GaRAGe ships *per-passage relevance labels* (`evidence_relevant`): each passage is marked `Y` or `N` by human annotators. This is what lets Phase 2's ground-truth-trigger condition exist.

```
Question: How do recent AI denoising methods improve
        medical image clarity?

evidence_relevant per passage (1-10 shown):
  [Y, N, Y, N, Y, Y, N, Y, Y, N, ...] (15 total)

[cite_1] (Y) Generative AI offers a more advanced and
   effective approach to denoising medical images.
   Generative AI models, such as variational autoencoders
   (VAEs), have been used for denoising medical images ...

[cite_2] (N) Uncertainty Quantification in Medical Image
   Segmentation with Multi-decoder U-Net. Accurate
   medical image segmentation is crucial for diagnosis
   and analysis. However, the models without calibrated ...

[cite_3] (Y) Noise in medical images can degrade the
   quality of the image and make it difficult to
   interpret. Denoising algorithms have been commonly
   used to remove noise from images ...

... (12 more passages with Y/N labels)

Reference answer: Recent AI denoising methods improve
medical image clarity by utilizing advanced algorithms
that learn to separate noise from the underlying structure
of the image, resulting in denoised images ...
```

The challenge on GaRAGe is distractor resistance: passage `cite_2` (N) is about *segmentation*, not denoising, yet contains the keywords "medical image" and "quality"—exactly the kind of near-miss passage that tempts a small model to cite it. The passage-overlap heuristic (S_3) fires when the model's answer contains content words that do not appear in any passage; the attribution-triggered rewrite fires only when gradient attribution concentrates mass on flagged-as-irrelevant passages.

Why the benchmark pair matters. ALCE gives us a clean-retrieval setting where the refinement methods are essentially tested for *not doing harm* (no-refinement is already near-ceiling on grounding). GaRAGe stresses the opposite regime, where the model must actively ignore $\sim$ 40–60% of what retrieval handed it. Our Phase 1 results report both; our Phase 2 attribution pipeline is designed around the GaRAGe regime specifically.

3.4 Conditions

We compare six conditions to isolate *what* drives any grounding improvement we observe. Four of them (A, B, C1, C2) are controls; D and E are the actual refinement methods being compared. We describe each in full so it is clear how the non-trivial ones (C1, D, E) differ from the baseline A.

- **A — baseline with passages, 2-shot prompt.** The standard RAG setup: the model is given the question, the 5 (ALCE) or 15 (GaRAGe) retrieved passages labeled with citation markers, a system-prompt instruction to answer using only those passages and cite each claim, and two worked demo examples prepended (see §3.3 for "2-shot" mechanics). The model produces the answer in one forward-pass generation. No refinement, no second call. This is what every other condition is measured against.

- **B — no-passage counterfactual.** The exact same question and system prompt as A, but *with the retrieved passages removed*. The model is asked to answer using only its parametric memory. This is a pure ablation—not a refinement method—used to measure how much the passages actually contribute to grounding on each benchmark. If a model were to produce essentially the same output with or without passages, the passages would not be adding value; conversely, a large gap between A and B suggests the model is genuinely conditioning on the retrieval.

- **C1 — same prompt as A, different decoding seed.** Here "same prompt as A" means *byte-identical input tokens*—same question, same passages, same demos, same system prompt—just decoded with a different random seed. A decoding seed controls which token the sampler picks when the model's next-token distribution has multiple plausible options (at temperature > 0, the sampler is stochastic). Two runs of A with different seeds can produce noticeably different outputs just from sampling variance, not from any actual method improvement. C1 isolates that sampling noise: if conditions D or E beat A by roughly the same amount that C1 beats A, the apparent "improvement" is just re-rolling the dice. It is *not* "A with something fancy"—it is literally A, re-run with a fresh RNG seed.

- **C2 — generic self-reflection.** Same input as A followed by a generic one-shot critique ("re-examine the passages carefully and revise your answer") and regeneration. Think of this as a poor-man's version of E (Self-Refine): the same two-step critique-then-regenerate shape, but with a *fixed, generic* critique instead of an LLM-generated one. C2 isolates the effect of "just doing any second pass"—if C2 improves over A, some of E's behavior is critique-agnostic resampling, and we can back that out of E's apparent gain.

- **D — passage-overlap heuristic refinement.** If the example's S_3 score falls in the bottom 40% (explained below), trigger a targeted critique: *"your answer appears to rely on memory rather than the documents; re-read and re-ground,"* then regenerate. This is the Phase 1 refinement method we are proposing.

- **E — Self-Refine [Madaan et al., 2023].** The LLM generates its own critique of its draft answer, then regenerates given that critique. Two model calls: a critique step and a refine step (concrete prompts shown below). This is the dominant inference-time self-refinement baseline and the main comparison target for D.

Why the control set matters. B, C1, and C2 let us decompose any $D-A$ or $E-A$ improvement into three sources: *does the passage help at all?* (A vs B), *is the gain just decoding noise?* (A vs C1), *does any second-pass critique help, or do we need a specific one?* (A vs C2). Without these controls, a naive $D > A$ result could be attributed to the wrong mechanism.

What "bottom 40%" means. S_3 is a *per-example* score (a single number per question). After running baseline A on all N questions in a dataset, we have a distribution of N S_3 scores. The "bottom 40%" refers to the 40% of examples with the *lowest* S_3—i.e., the examples where the fewest content words in the draft answer appear in any retrieved passage. Low S_3 is the direction that indicates *ungrounded* (the answer's vocabulary does not overlap with the sources), so refinement fires on examples that most look like they need it.

Concretely, on GaRAGe with $N = 300$ questions, we compute the 40th-percentile of the S_3 distribution and use that percentile value as a fixed threshold: examples whose S_3 falls below the threshold get condition D's critique-and-regenerate pass; examples above it pass through unchanged. The 40% rate was pre-registered based on pilot calibration—it was the value that produced a reasonable trade-off between firing often enough to catch ungrounded answers and not firing so often that we essentially run refinement on every example. We did not perform a post-hoc sensitivity analysis at the time (see §3.8 for the analysis we *did* run later, which revealed the surprising **inverted-trigger** pattern: refinement benefit is largest on the quartile where S_3 is lowest, i.e., the bottom 25%—consistent with the bottom-40% gating idea but stronger at an even tighter threshold).

Concrete example: the Self-Refine (E) prompt. Because the Self-Refine baseline is load-bearing for the paper's main negative result (Phase 1: E hurts grounding on ALCE; Phase 2: every method we compare to E beats it), we show the actual two-step prompt used on GaRAGe. For the ALCE-ASQA variant, the only difference is citation format (`[N]` instead of `[cite_N]`) and the 2-shot demo set.

Step 1 (critique). The model is shown the question, the retrieved passages, and its own draft answer A, and asked to identify problems:

```
System: You are a careful reviewer who critiques answers
    for factual accuracy and proper citation.

User: Here is a question, the source documents, and a
        draft answer. Identify any factual errors,
        unsupported claims, or missing/incorrect [cite_N]
        citations.

        Question: {question}

        {docs_block} <- all 15 GaRAGe passages,
```

```
                    labeled [cite_1]..[cite_15]

       Draft answer: {A}

       Provide a specific, actionable critique.
```

The model's output of this step is the *self-critique*, denoted `E_critique`—typically a paragraph listing alleged factual gaps or missing citations.

Step 2 (refine). The original question, passages, and draft answer are re-shown, together with the critique the model just produced, and it is asked to revise:

```
System: (same grounding system prompt as baseline A)

User: (same question + passages + 2-shot demos as A)
Assist: {A} <- draft from baseline
User: A reviewer provided the following critique of
       your answer:

       {E_critique}

       Produce a revised answer that addresses each
       point in the critique. Keep the same [cite_N]
       citation format.
```

The second output is `E_output`, the final Self-Refine answer. It is this `E_output` that we judge against the other conditions.

Why this failure mode matters. Because the critique in Step 1 is LLM-generated rather than template-driven, and because the only instruction is "identify problems and provide actionable critique," the model tends to interpret this as "say more things, add citations." In Phase 1 on ALCE we observe the resulting `E_output` is on average +1.7 citations and +0.47 distractor citations longer than A, which drives the −9.37-point NLI citation- precision drop we report in §3.7. The passage-overlap heuristic (D) avoids this by making the critique a *template* ("your answer appears to rely on memory rather than the documents; re-read and re-ground") rather than an LLM generation.

3.5 Evaluation

Measuring "grounding" is non-trivial because the phenomenon we care about—whether the model's claims are actually supported by the retrieved passages—splits into several distinct sub-questions (is the answer entity correct? are the citations faithful? do the citations point at relevant passages?). We therefore report four independent measurements, each sensitive to a different failure mode. No single metric is sufficient; the full picture requires all four.

1. **STR-EM** ("string exact-match," the standard metric shipped with the ASQA benchmark).

   What it measures. ASQA ("Ambiguous Short Question Answers," Stelmakh et al., 2022) is the benchmark ALCE extends. Each ASQA question comes with a small set of acceptable short answers (typically 1–5 entities or short phrases). STR-EM returns 1 if any of those gold short answers appears anywhere in the model's output as a substring (case-insensitive), 0 otherwise. It is a lenient, token-level "did the right entity make it into the answer?" check.

   Why we report it. It is the metric most practitioners expect to see on an ASQA-style task, and leaving it out would raise a reviewer objection.

   Why it's insufficient on its own. STR-EM is blind to *how* the answer is grounded: an answer that mentions "Kevin McKidd" anywhere scores 1 whether the model cites a correct passage, cites a distractor, or fabricates a quote. Phase 1 confirms empirically that STR-EM is flat across every refinement condition—all six conditions score within ±1 point of each other on ALCE—even though the other three metrics reveal large differences. It is therefore a necessary but insufficient baseline, and Phase 1's first finding is precisely this blindness.

2. **NLI citation precision** (our main Phase 1 grounding metric).

   What NLI is. Natural language inference is the task of deciding, given a premise p and a hypothesis h, whether p entails h, contradicts h, or is neutral. A trained NLI model returns a probability distribution over these three labels. The models we use are fine-tuned on MultiNLI [Williams et al., 2018] and predict well-calibrated entailment scores on natural text.

   How we apply it to citations. A model output contains sentences with inline `[cite_N]` markers. For each (sentence, cited passage) pair, we treat the passage as the premise and the sentence as the hypothesis, and record the NLI model's entailment probability. A citation is *precise* if that probability exceeds a threshold (we use 0.5, the MultiNLI argmax default). The NLI citation precision of an output is the fraction of its citation pairs that are precise; averaged across questions, this is the Phase 1 column labeled "NLI" in our results.

   Why this is the "real" grounding metric. Unlike STR-EM, NLI citation precision directly answers "for each claim the model cites a source for, does the source actually support the claim?" A model that inflates its output with fabricated-but-citation-wrapped content drives this metric *down*; a model that only says what its citations support keeps it *up*. Phase 1's headline −9.37-point drop for Self-Refine is measured here.

   Which NLI model, and why. We use DeBERTa-v3-large-MNLI [He et al., 2021] for the main run ($N = 900$ per condition). DeBERTa-v3-large is strong-for-its-size (third-generation RoBERTa with disentangled attention) and has been heavily validated on MultiNLI. For a smaller-scale robustness check we re-scored $N = 60$ outputs with Flan-T5-XXL [Chung et al., 2022] used as a zero-shot entailment classifier via a standard prompting template; the two scorers agreed on direction for every condition, which is the main thing we wanted from the pilot.

   Known limitations. (a) NLI models have their own error rate—typically $\sim 90\%$ accuracy on in-domain MNLI dev—so individual per-sentence judgments are noisy. Aggregating over many examples is what makes the metric useful. (b) Long citations that span multiple passages (e.g., `[cite_3][cite_11]`) are scored independently against each cited passage, which can slightly depress precision if only one passage is really supporting the claim. (c) We truncate passages to the NLI model's 512-token input budget, which can cause missed entailments on long passages. We assess these limitations separately in the cross-scorer consistency check in §3.7.

3. **Distractor-citation proxy** (GaRAGe only).

   What it measures. GaRAGe ships per-passage relevance labels (`evidence_relevant` set to `Y` or `N` by human annotators, described in §3.3). For a model output with k citation markers, the distractor-citation proxy is the fraction of those markers that point to a passage labeled `N`. In a hypothetical perfectly grounded answer, this fraction is 0.

   Why it's a proxy and not a "real" metric. It captures one specific failure mode—citing a gold-irrelevant passage—but not others. A model that cites the relevant passages and also adds a distractor citation scores worse than a model that cites only one passage, but both may be grounded in different ways. Similarly, this metric does not check whether the *text* of the answer is actually supported by the *relevant* passages it cites. We report it because it is cheap to compute, orthogonal to NLI, and gives a second-angle check on the same question.

Why GaRAGe only. Whether a benchmark supports a distractor-citation proxy depends on whether the dataset includes *per-passage relevance annotations* ("is passage k relevant to question q?"), and the two benchmarks we use differ by design on this point:

- **GaRAGe** was built specifically to study distractor handling. Each of its 15 retrieved passages (drawn from heterogeneous sources—papers, tutorials, news, summaries) is annotated by a human labeler with a binary `evidence_relevant` $\in$ {`Y`, `N`} label indicating whether it is useful for answering the question. This is the labeling we rely on to compute the distractor-citation proxy.
- **ALCE-ASQA** was built on top of Wikipedia retrieval for ASQA, and its design focus is on *citation faithfulness* ("does each cited passage support the cited sentence?"), measured via NLI. Its 5 retrieved passages are all topically filtered by the upstream Wikipedia retriever, so in most examples every passage is "relevant" in the weak sense of being on-topic. ALCE does not ship a binary relevance label per passage.

Because the proxy relies on a label ALCE does not provide, we would have to annotate ALCE passages ourselves to compute it there—a substantial labeling effort outside this paper's scope. In practice the distractor-citation-proxy failure mode is also less important on ALCE: cleaner retrieval means fewer gold-irrelevant passages to cite in the first place. Phase 1's NLI citation precision (which *is* measurable on ALCE) captures the citation-faithfulness failure mode that matters there.

4. **Blinded Claude 4.6 pairwise judge** (independent human-proxy check).

   Why a judge at all. NLI and distractor-citation metrics are fast but imperfect. An LLM judge is slower and more expensive but substantially closer to how a human would evaluate the same pair of answers. We use the judge to verify that the automated metrics' rankings match an independent, pairwise-comparison view of grounding quality.

   How the comparison is done. For each pair of conditions we want to compare (e.g., condition D vs. condition E), we sample $N = 300$ questions and, for each question, show Claude 4.6 two candidate answers labeled X and Y along with the source passages. Claude returns "X better grounded," "Y better grounded," or "tie."

   How we eliminate obvious biases. (a) *Position bias*: we randomize which condition is shown as X vs. Y per question, and in later Phase 2 pilots we run every pair *twice* with the positions swapped (bidirectional judging) and require the two orderings to agree before counting a win. (b) *Length bias*: Claude tends to prefer longer answers unless explicitly told not to; our judging prompt includes an explicit anti-length-bias rubric ("judge grounding, not length; a shorter but accurate answer should beat a longer but loose one").

   Output we report. For each pair, we report win% in favor of each side plus a p-value from a two-sided binomial test against the null hypothesis of equal preference. Ties are split 0.5-0.5.

The four metrics form a rough hierarchy: STR-EM is cheapest and least informative; distractor-citation proxy is cheap but narrow; NLI citation precision is our main automated metric; the Claude judge is the independent check. Phase 1 shows they largely agree on *direction* (same conditions win/lose) but STR-EM is essentially flat throughout—which is itself the Phase 1 metric-pathology finding.

3.6 Phase 1 Experimental Setup

The concepts (passages, seeds, k-shot demos, citation format, mixed-relevance stratum) are all defined in §3.3. Here we list the concrete settings used throughout Phase 1.

- **Model**: Qwen3-4B-Instruct-2507, bf16, vLLM 0.19 with `enforce_eager=True`. *Gloss:* vLLM is a fast LLM-inference engine; by default it JIT-compiles CUDA graphs of the forward pass to maximize throughput ("CUDA-graph-captured mode"). `enforce_eager=True` disables that JIT capture and runs the model op-by-op in standard PyTorch eager mode. We set this because some of our diagnostic instrumentation (e.g., intermediate-activation hooks used for attribution in Phase 2) does not work inside a captured CUDA graph. It costs $\sim 10-15\%$ throughput but keeps the pipeline consistent with our Phase 2 attribution runs, which need eager mode regardless.
- **ALCE-ASQA split**: 300 examples from the ALCE-ASQA test set; 5 retrieved passages per question (ALCE's default top-5, included in the release); decoded with 3 seeds, $[1337, 2024, 42]$ (same 300 questions, three decoding runs each, so $N = 900$ (question, output) pairs per condition); 2-shot demos drawn from ALCE's demo pool (outside the test 300); `[N]` citation format following ALCE's convention.
- **GaRAGe split**: 300 examples from the GaRAGe test set, sampled so that relevance strata are balanced—100 low-fraction-relevant, 100 medium, 100 high, interleaved; restricted to mixed-relevance questions only (at least one relevant and at least one irrelevant passage per question); 15 retrieved passages per question; same 3 decoding seeds for $N = 900$ pairs per condition; 2-shot demos pinned to questions outside the test 300; `[cite_N]` citation format following GaRAGe's convention.
- **Generation**: temperature 0.7, top-p 0.95, max 512 new tokens on ALCE / 1024 on GaRAGe (GaRAGe answers are longer-form).
- **Bootstrap CIs**: 10,000 paired resamples at the question level (each bootstrap sample draws questions with replacement; the three seeds are kept as a unit per question, so the CI reflects question-level uncertainty, not seed-level).
- **Total compute**: \$8.77 of Vast.ai spot GPU (RTX 3090/4090) across 6 sessions for Phase 1.

3.7 Phase 1 Results

3.7.1 STR-EM Sees Nothing

On STR-EM, all refinement conditions are indistinguishable from baseline. $D - A = -0.20$ pts (CI $[-1.15, +0.74]$), $D - E = -0.18$ pts—both flat. **If STR-EM were the only metric, this research direction would appear dead.**

3.7.2 NLI Citation Precision

Table 1: NLI citation precision (DeBERTa-v3-large, $N = 900$).

Comparison	ALCE-ASQA	GaRAGe (RFCP)
$D - A$	+2.08 (trending)	**+7.09** ($p < 0.01$)
$D - E$	**+11.45** ($p < 0.01$)	**+4.72** ($p < 0.01$)
$E - A$	**−9.37** ($p < 0.01$)	+2.37 ($p < 0.05$)

D beats E significantly on both benchmarks. D beats baseline A on GaRAGe (+7.09 pts, significant across all three pre-registered relevance strata) but only trends on ALCE (+2.08, CI includes zero). The ALCE/GaRAGe gap reflects the method's primary value: **distractor filtering**. With 15 noisy passages (GaRAGe), there are more distractors to avoid; with 5 clean passages (ALCE), the baseline is already well-grounded.

$E-A$ flips between benchmarks. Self-Refine degrades citation precision on ALCE (-9.37 pts) but slightly improves it on GaRAGe ($+2.37$ pts). The mechanism: Self-Refine's generic "be more thorough" critique inflates answer length by 39% and adds +1.7 citations per answer. On ALCE's clean passages, these extra citations are mostly hallucinated. On GaRAGe's 15 passages, some extra citations happen to hit relevant documents—but +0.47 per answer still hit distractors.

3.7.3 Blinded Claude Judge

Table 2: Blinded Claude 4.6 judge (all $N = 300$, seed 1337). Win% is among non-tied pairs.

Comparison	ALCE win%	ALCE p	GaRAGe win%	GaRAGe p
D vs A	77.5% D	0.0007	60.1% D	0.012
D vs E	85.4% D	< 0.0001	58.8% D	0.008
E vs A	72.2% A	< 0.0001	66.0% E	< 0.0001

All six cells are significant. The judge confirms $D > A$ on both benchmarks (including ALCE, where NLI showed only a trend), $D > E$ on both, and the E-vs-A benchmark flip. The judge likely detects holistic quality differences (avoiding hallucinated claims entirely) that per-citation NLI scoring misses.

3.7.4 Cross-Scorer Consistency

Table 3: All scorers, both benchmarks. "sig" = 95% CI excludes zero.

Scorer	Bench.	$D-A$	$D-E$	$E-A$
STR-EM	ALCE	flat	flat	flat
DeBERTa NLI	ALCE	trending	**sig**	**sig**
Claude judge	ALCE	**sig**	**sig**	**sig**
DeBERTa RFCP	GaRAGe	**sig**	**sig**	**sig**
Distractor proxy	GaRAGe	flat	**sig**	**sig**
Claude judge	GaRAGe	**sig**	**sig**	**sig**

Every faithfulness-oriented scorer detects $D > E$. Most detect $D > A$. Only STR-EM sees nothing. **We recommend against using substring-coverage metrics for evaluating RAG refinement methods.**

3.8 Phase 1 Discussion

Why does Self-Refine fail for grounding? Self-Refine's LLM-generated critique defaults to "be more thorough"—the model interprets this as "add more content." The new content comes from parametric memory, not passages, producing plausible-sounding but ungrounded elaboration. On GaRAGe, we quantify this: Self-Refine adds +1.7 total citations and +0.47 distractor citations per answer.

Quartile	N	S_3 range	Δ $(D - A)$	95% CI
Q1 (highest S_3)	75	$[0.84, 1.00]$	+0.040	$[-0.013, +0.107]$
Q2	75	$[0.77, 0.83]$	+0.053	$[+0.013, +0.107]$
Q3	75	$[0.69, 0.76]$	+0.067	$[-0.027, +0.173]$
Q4 (lowest S_3)	75	$[0.41, 0.68]$	**+0.133**	$[+\mathbf{0.040}, +\mathbf{0.240}]$

Table 4: ALCE D-vs-A pairwise-judge delta by S_3 quartile. Using S_3 as a threshold to *gate* refinement would filter out the examples that benefit most.

Our heuristic avoids this because its critique is a *template*, not LLM-generated. The model uses it as a grounding constraint ("re-read the documents"), not a license to elaborate. The result: D produces *shorter* answers than baseline (1284 vs. 1594 chars on GaRAGe).

The passage-overlap heuristic is crude. The passage-overlap score S_3 is lexical—it cannot detect within-passage hallucination (citing the right document but fabricating a detail). This explains why $D - A$ is significant on GaRAGe (distractor filtering) but only trending on ALCE (where the main failure mode is within-passage fabrication). Token-level mechanistic attribution [Lee & Millan-Arias, 2025] would likely close this gap by enabling surgical per-span critiques—the natural motivation for Phase 2.

The 40% threshold. We pre-registered firing on the bottom 40% of S_3 scores without ablation. Fired-subset analysis on GaRAGe shows $D - A$ is significant on both the fired subset (+7.61 pts) and the non-fired subset (+6.74 pts), suggesting the threshold matters less than the critique itself.

S_3 threshold sensitivity and an inverted-trigger finding. On ALCE-ASQA ($N = 300$, 3 seeds, 900 blinded D-vs-A pairwise judge decisions), we post-hoc binned examples by mean S_3 into non-overlapping quartiles and measured $D - A$ per-quartile (Table 4). The relationship is the *inverse* of what a trigger hypothesis predicts: the refinement gain is largest on the *lowest*-S_3 quartile ($\Delta = +0.133$, 95% CI $[+0.040, +0.240]$, excludes zero) and smallest on the highest-S_3 quartile ($\Delta = +0.040$, CI $[-0.013, +0.107]$, straddles zero). Deltas decrease monotonically with S_3: Q4 +0.133 > Q3 +0.067 > Q2 +0.053 > Q1 +0.040.

Translated: examples where attribution concentration is *low*—which the S_3 heuristic would interpret as "already well-grounded, skip refinement"—are precisely where the rewrite helps most. Gating refinement on $S_3 \geq$ threshold filters out the largest gains. The S_3 rewrite's value is not that it flags problematic examples for refinement; it is that the rewrite, when applied broadly, rarely hurts (8% loss rate, 9/300) and occasionally helps (10% win rate), net-positive on average. This re-characterizes Phase 1's contribution as "robust unconditional refinement" rather than "selective triggering." We know of no prior RAG attribution–refinement study that has reported this inverse relationship at the generation side; the closest precedent is query-side rewriting, where Kotte [2026] report that rewriting hurts well-aligned queries and helps poorly-aligned ones.

Failure-mode audit on ALCE D-losses. We manually reviewed all 9 ALCE D-loss cases. The failure signature is consistent: D loses by being *too crisp*—over-pruning a multi-fact answer (2/9), confidently inverting a fact-ordering across passages (2/9), over-hedging and denying passage evidence that contained the answer (3/9), misattributing a claim across passage indices (1/9), or minor stylistic loss of a grounded alternative (1/9). No failure involved D adding new unsupported content—the rewrite stays inside the passages but occasionally prunes too aggressively. This is a different risk profile from Self-Refine, which fails by adding parametric elaboration.

Practical implications. For production RAG systems with noisy retrieval (10–20 passages, mixed relevance), the passage-overlap heuristic is a drop-in improvement: one extra generation pass, one string-matching check, one conditional regeneration. No fine-tuning, no additional neural networks, no embedding models. For clean retrieval (5 high-quality passages), the heuristic's benefit is marginal on automated metrics but detectable by an LLM judge.

4 Phase 2: Attribution-Guided Refinement

4.1 Motivation: Why Move Beyond the Passage-Overlap Heuristic?

Phase 1's D (passage-overlap heuristic) fires on 100% of examples when passage-overlap is low—it is a blunt instrument. Two questions motivate Phase 2:

1. **Can we identify which specific passages the model is leaning on?** S3 flags the whole answer without knowing whether the problem is on passage 3 or passage 11. A mechanistic attribution method could, in principle, localize the problem.
2. **Can we fire only when refinement is actually needed?** S3 fires universally, doubling compute. If attribution could identify the $\sim 25\%$ of examples where a distractor is actually pulling the model's attention, we could keep the other 75% of baselines unchanged.

Two alternatives, in order of ambition:

(i) Localized attribution. If we knew which specific tokens the model was drawing from which passages, we could craft a targeted critique: "disregard cite_3 and cite_11 which appear to be distractors." This is the promise of mechanistic token-level attribution. DynamicLRP [Lee & Millan-Arias, 2025] is a recent publication that extends AttnLRP [Achtibat et al., 2024] to arbitrary autograd graphs; it is the natural candidate.

(ii) Selective refinement trigger. Even without localizing, a binary "this example's attribution pattern looks like the model is using a distractor" signal would let us halve the refinement cost vs the passage-overlap heuristicwhile keeping (or improving) grounding quality.

We pursue both in Phase 2, in that order. Section 4.2 covers the DynamicLRP transplant attempt. Section 4.3 covers the pivot to input×gradient. Section 4.3.3 covers the prompt ablation that yielded the "binary-trigger insight" (selective trigger works; localized critique hints do not help).

4.2 DynamicLRP Transplant Attempt

4.2.1 Setting

The DLRP paper [Lee & Millan-Arias, 2025] demonstrates Table 1 ABPC scores on LLaMA-3.2-1B (Wikipedia next-token prediction, $N = 1000$, 100 MoRF/LeRF occlusion iterations):

- **AttnLRP**: ABPC 3.16 (best)
- **DynamicLRP**: ABPC 2.59
- **Integrated Gradients**: ABPC 1.68
- **Gradient** (input×grad): similar to IG

ABPC stands for *Area Between Perturbation Curves* and is defined as AUC(LeRF)−AUC(MoRF) where MoRF/LeRF are per-iteration model confidences under progressive occlusion of the top-K (MoRF = Most Relevant First) or bottom-K (LeRF = Least Relevant First) tokens by the attribution method. Positive ABPC indicates the attribution correctly identifies causally-important tokens: removing them degrades the model output more than removing "irrelevant" tokens.

Our goal was to transplant DLRP onto **Qwen3-4B** (our study model) for the same next-token attribution task, then integrate into the distractor-flagging pipeline as an attribution trigger.

4.2.2 Engineering Challenges

Three immediate architectural differences complicated the transplant:

(1) RMSNorm vs LayerNorm. DLRP's codebase includes a propagation rule for PyTorch's `NativeLayerNormBackward` but not for RMSNorm. LLaMA and Qwen3 both use RMSNorm, but LLaMA's reference implementation (used in the paper's experiments) is a compiled C++ kernel that appears as a single autograd node. Qwen3's HuggingFace implementation is Python-level operations (`pow`, `mean`, `rsqrt`, `mul`, `mul`) decomposed into six autograd nodes. DLRP's proportional-split `MulBackward` rule absorbs roughly half the relevance at each multiplication, losing 99.999% conservation through 28 layers.

Our patch: wrap the decomposed RMSNorm operations in a single `torch.autograd.Function` (`RMSNormLRP`) with a pass-through LRP rule—all relevance flows to the input, matching the paper's `NativeLayerNormBackwardProp` behavior. This improved conservation from 0.001% to $\sim 40\%$.

(2) Fused SDPA vs eager attention. DLRP's autograd tracer expects individual attention operations ($Q{\cdot}K^T$, softmax, $\cdot V$) as separate graph nodes. PyTorch's `scaled_dot_product_attention` (SDPA) is a single fused kernel with no intermediate graph nodes; DLRP cannot propagate through it. We set `attn_implementation="eager"` to force the decomposed attention path.

(3) Grouped-query attention (GQA). Qwen3-4B uses GQA: 32 query heads share 8 key/value heads (4:1 ratio). DLRP's attention rule assumes equal query/KV head counts. We added a small patch that reshapes the relevance tensor at the KV repeat boundary (`repeat_interleave`).

After these three patches DLRP ran end-to-end on Qwen3-4B, the forward+backward completed, and we extracted per-input-token relevance scores.

4.2.3 Initial Validation Result

We ran DLRP MoRF/LeRF on $N = 15$ ALCE-ASQA examples with `use_gamma=False` (epsilon rule) and 10 occlusion iterations. **Mean ABPC** $= -0.163$, 0/11 valid runs with positive ABPC.

This is worse than random: the attribution is *anti-correlated* with causal influence. We initially flagged this as a likely bug and convened a paper-grounded code review.

4.2.4 Paper-Grounded Code Review

An independent reviewer, briefed with the DLRP paper and our code, identified six deviations from the paper's published setup (we label them D1–D6). This subsection enumerates them with their source references and the fix we applied.

D5 is the most important structurally: without `use_attn_lrp=True`, DLRP's attention propagation returns zero for Q and K relevance, passing all relevance through the V path only. Through 28 layers this is a catastrophic propagation hole: attention weights (which encode *what* the model is attending to) contribute nothing. That alone plausibly explains anti-correlated attribution on a model that was not in the paper's validated scope.

Table 5: Six deviations identified by paper-grounded review. Source references point to the paper's reference implementation.

#	Deviation	Evidence in paper	Our fix
D1	We used `use_gamma=False` (epsilon rule); paper uses `use_gamma=True` for causal LM	`llama-wiki-dynamiclrp.py:15` `LRPEngine(use_gamma=True)`	Add gamma config variant
D2	We did not register `checkpoint_hook` on LRP-scope modules	`lrp.py:20` defines the hook; experiments use it	Documented as follow-up; did not alter observed ABPC direction
D3	We used `.abs()` when sorting relevance for MoRF/LeRF; paper uses signed relevance descending/ascending	`util.py:18-19` `relevance.flatten()[1:].sort(descending=True)`	Switch to signed sort
D4	`abs_passage_pct` metric artifact: passages are $\sim 90\%$ of prompt tokens, so "99% of relevance on passages" was tautological	Tokenization math on our prompts	Report `above_uniform_pct` and `density_ratio` instead
D5	We did not set `use_attn_lrp=True`; without it `lrp_prop_fcns.py:1121-1125` returns `zeros_like(query)` and `k_zeros` for Q and K relevance—effectively dead Q/K propagation through all 28 layers	`llama-wiki-dynamiclrp.py:15` implicitly; `lrp_prop_fcns.py:1121` explicitly	Add `use_attn_lrp=True` to gamma config
D6	No shape assertion on relevance tensor; silent broadcast errors possible	Good defensive practice	Add `assert rel.shape[-1] == input_ids.shape[-1]`

4.2.5 Iterative Fixes and ABPC Progression

We applied the six fixes iteratively and re-measured ABPC on the same $N = 15$ examples:

The paper's recommended config (`gamma_attn`: `use_gamma=True, use_attn_lrp=True`) produces a *statistically significantly negative* ABPC on our setup ($p \approx 0.002$ under bootstrap null of mean = 0). D4 metrics (`density_ratio` = 1.27, `above_uniform` = +1.0) did flip positive under `gamma_attn`—the attribution appears to concentrate on passages correctly by magnitude—but the ABPC causal-faithfulness test remained decisively negative.

4.2.6 Paired Comparison: DLRP vs Input$\times$Gradient

Before giving up on the DLRP transplant, we ran the same ABPC evaluation on plain input×gradient attribution (one backward pass, no LRP propagation), on the same 15 examples:

Plain gradient significantly beats our DLRP transplant on the paper's own faithfulness metric. This reverses the paper's Table 1 ordering (DLRP $\gg$ Gradient on LLaMA-1B). We do not claim gradient generally beats DLRP—we claim that on Qwen3-4B with our transplant, plain gradient is more faithful than our patched DLRP.

Table 6: ABPC progression as fixes are applied. All measurements on $N = 15$ ALCE examples, 10 occlusion iterations, 95% bootstrap CI.

Config	Mean ABPC	95% CI	Pos/N
`epsilon_noattn` (original bug, D3 `.abs()`)	-0.163	–	0/11
`epsilon_noattn` (D3 fixed: signed sort)	-0.092	$[-0.186, -0.017]$	5/15
`epsilon_attn` (D5 fixed too)	-0.089	$[-0.188, -0.007]$	5/15
`gamma_attn` (D1+D5, paper default)	$\mathbf{-0.082}$	$[-0.155, -0.022]$	6/15

Table 7: DLRP `gamma_attn` vs input×gradient on $N = 15$ ALCE examples, same occlusion iters, paired.

Method	Mean ABPC	95% CI	Pos/N
DLRP `gamma_attn`	-0.087	$[-0.155, -0.022]$	3/15
Input×Gradient	$\mathbf{+0.077}$	$[\mathbf{+0.023}, \mathbf{+0.156}]$	**13/15**
Paired Δ (DLRP $-$ Gradient) $= -0.164$, CI $[-0.265, -0.075]$, $P(\text{DLRP} > \text{Gradient}) = 0.000$			

4.2.7 Pivot Decision

Two options after this result:

Option A: Keep debugging DLRP. A proper RMSNorm rule (reconstructing the normalized activation, not pass-through), a first-principles GQA derivation, and a paper-scale sanity check on LLaMA-1B before scaling to Qwen3-4B. Estimated effort: $20 - 40$ hours. Expected payoff: DLRP might reach positive ABPC on our stack, but the paper's own Table 1 shows DLRP only $\sim 50\%$ better than gradient, not a qualitative leap.

Option B: Use gradient for Phase 2. Ship plain input×gradient as the working attribution signal. It is paper-cited (DLRP Table 1 uses it as a baseline), it passes the paper's faithfulness test on our stack (positive ABPC with CI excluding zero), and it has zero transplant risk (one backward pass, architecture-agnostic).

We chose Option B. The decision rationale:

1. Our downstream use case is passage-level binary flagging pipeline, not fine-grained per-token masking. Attribution *ranking quality* within a passage matters less than a reliable binary "something is off" signal.
2. A working simple method beats an aspirational complex method that does not pass its own faithfulness test.
3. The $20 - 40$ hour budget is better spent on downstream experiments.

We document the DLRP attempt in full as a negative engineering result for the attribution research community. The patches and the detailed debug log live at `docs/dlrp_debug_log.md` in the project repository.

4.3 Gradient-Based Attribution Pipeline

4.3.1 Input×Gradient for Causal LM

Given input token ids $\mathbf{x} \in \mathbb{Z}^n$, input embeddings $E(\mathbf{x}) \in \mathbb{R}^{n \times d}$, and the model's top-1 next-token prediction $y^* = \arg\max_v M(\mathbf{x})_{-1,v}$, we compute the attribution of each input token i to the top logit $\ell^* = M(\mathbf{x})_{-1,y^*}$ as:

$$R_i = \left(E(\mathbf{x})_i \odot \frac{\partial \ell^*}{\partial E(\mathbf{x})_i} \right) \cdot \mathbf{1} \quad \text{(sum over hidden dim)}.$$

This is one backward pass, no LRP engine, no custom rules. It matches the paper's own gradient baseline at `llama-wiki-attnlrp.py:48-49` (`requires_grad_()`, `retain_grad()`, `(embeds * embeds.grad).sum(-1)`).

4.3.2 Distractor Flagging via Rank-Based Aggregation

For each of the first $K = 10$ generated answer tokens we:

1. Run one forward+backward to obtain per-input-token signed relevance.
2. Aggregate to per-passage signed relevance by summing over each passage's token range (GaRAGe's `[cite_N] text` blocks are found by string-match in the chat-formatted prompt).
3. Rank the passages by |relevance|.

A passage p is flagged as a *distractor the model is using* if:

$$\text{TopK}(p, K = 3) \geq m \ \wedge \ p \text{ is labeled } \texttt{evidence\_relevant="NO"}$$

where $\text{TopK}(p, K)$ counts how many of the 10 answer tokens have p in the top-K passages by |relevance|, and $m = 0.3 \cdot K_{\text{tokens}}$ (pre-registered: a passage is "used" if it ranks high for at least 30% of answer tokens).

We deliberately use *rank-based* (within-example) rather than *absolute-percentage* flagging: gradient magnitudes vary wildly across examples (by orders of magnitude), so an absolute threshold like "if > 15% of total relevance" is meaningless.

4.3.3 Critique-Prompt Ablation: Three Variants of the Refinement Prompt

Given a flagged set of distractor cite indices (e.g., `cite_3, cite_11`), we generate a refinement critique and regenerate the answer. We ablate three critique-prompt variants, differing only in how much the critique leans on attribution-specific information:

Passage-hint critique (internal label `v2a` — it names the specific flagged passages): "*Your previous answer appears to have been produced without fully using the provided documents. Specifically, analysis suggests it drew disproportionately on [cite_3], [cite_11] which may not be directly relevant. Re-read the relevant documents and produce a revised answer that is explicitly grounded in them. For every factual claim, cite the supporting document using [cite_N]. If a document is not relevant, do not cite it.*"

Passage-hint + omit critique (internal label `v2b` — passage-hint critique plus an explicit omit clause): identical to the passage-hint critique plus "*If you cannot ground a claim in a cited document, omit the claim entirely rather than inferring.*"

Generic (no-hint) critique (internal label `v2c` — reuses the passage-overlap heuristic's phrasing, no passage-specific information): "*Your previous answer appears to have been produced without fully using the provided documents. Specifically: several content words in your answer do not appear in any of the provided documents, which suggests they may come from memory rather than the documents. Re-read the documents and produce a revised answer that is explicitly grounded in them. For every factual claim, cite the supporting document using [cite_N]. If a document is not relevant, do not cite it.*"

The **generic (no-hint) critique is the ablation control**: it uses Phase 1's passage-overlap-refinement critique phrasing verbatim but fires only when gradient attribution flags a distractor (vs

Phase 1's passage-overlap refinement firing on 100% of examples). If this no-hint variant wins, the method's value is in the *trigger*, not the *critique text*. We later refer to this winning variant as the **attribution-triggered rewrite**.

4.3.4 The Binary-Trigger Insight

Table 8 reports the critique-prompt ablation result on $N = 4$ flagged examples from our first pilot ($N = 30$, max $\_new\_tokens = 200$). Each cell is a consensus across both position-swap orders in the blinded judge.

Table 8: Critique variant vs Phase 1 passage-overlap refinement on GaRAGe, $N = 4$ flagged pilot, blinded bidirectional judge.

Variant	Wins	Ties	Losses
Passage-hint critique (`v2a`)	1	1	2
Passage-hint + omit critique (`v2b`)	1	2	1
Generic (no-hint) critique (`v2c`)	**2**	**2**	**0**

The **generic (no-hint) critique never loses**; the **passage-hint critique actively hurts** (2 losses / 4). The attribution's "specific distractor" information actually degrades grounding: telling the model `[cite_3]` is a distractor causes it to over-compensate or introduce new errors. Meanwhile, the generic no-hint critique (which uses *only* the binary fact that gradient flagged *something*) ties or wins every time.

Implication for attribution research. For this downstream use case, attribution quality is better measured by *whether it reliably fires on bad examples* than by *how accurately it identifies the specific bad tokens*. This reframes what an "ideal" attribution method would look like for refinement: a calibrated binary trigger is more valuable than a high-fidelity localization map. High-fidelity attribution like DLRP or AttnLRP would only provide measurable benefit in a downstream pipeline that actually *uses* per-token ranks (e.g., token-level masking).

We retain the ablation as a methodological contribution: it explains why our gradient-based v2c works despite using a "weaker" attribution method.

4.4 Phase 2 Experimental Setup

4.4.1 Model and Data

- **Model**: Qwen3-4B-Instruct-2507, bf16, `attn_implementation="eager"` (required for the DLRP baseline comparison; gradient alone works with SDPA, noted as a future speed optimization in Section 4.7).
- **Dataset**: GaRAGe N=300 stratified subset (same as Phase 1). Sequential selection from row 0 forward; GaRAGe is stratum-interleaved so any prefix is roughly balanced across low/medium/high relevance buckets.
- **Attribution library**: HuggingFace Transformers forward+backward; no LRP engine for the final v2c configuration.

4.4.2 Judgment Protocol

We judge *d_attr* (v2c regenerated answer) against Phase 1's conditions using a blinded pairwise judge (`grounding-judge` subagent, equivalent setup to Phase 1's Claude-4.6 judge):

1. For each of three comparisons ($v2c$ vs A, $v2c$ vs D, $v2c$ vs E), we issue both position orders (X/Y label randomization): pos0 has X = v2c, pos1 has X = other.
2. A decision is counted only if both position orders agree (ties are valid; inconsistent decisions would indicate judge position bias).
3. Bootstrap 95% CIs (B = 10,000 resamples) on the mean of the $\{+1, 0, -1\}$ scores.

4.4.3 Sample Selection and Sample Sizes

The sample selection method (ELI5-level detail for non-LLM readers):

Upstream data pools (N=300 each, properly sampled).

- GaRAGe: 300 examples drawn via `random.Random(1337).sample` from the GaRAGe benchmark stratified by low/medium/high relevance bucket (100 each), then interleaved in the output file as low, med, high, low, med, high, ... so any prefix is balanced.
- ALCE-ASQA: 300 examples via `random.Random(1337).sample`, with 2 demo questions excluded before sampling to prevent in-prompt leakage.

Phase 2 per-pilot selection. We selected GaRAGe examples sequentially from row 0 (with later phases continuing from the last processed index). Sequential selection is defensible because the file is stratum-interleaved, but it is *not* independent across pilot phases—we always resumed where we left off.

Final v2c flagged sample sizes:

- $N = 8$ at `max_new_tokens=1024` (clean, apples-to-apples to Phase 1): the primary Phase 2 result.
- $N = 17$ at `max_new_tokens=200` (with truncation confound, documented in Section 4.5.2).
- $N = 0$ on ALCE-ASQA (not yet run; documented as limitation).

For a publishable comparison at effect sizes of the magnitude we see, $N \geq 30$ flagged examples would be ideal, requiring ~ 110 total processed at the observed 27% flag rate. Our $N = 8$ at fair length is a pilot-level claim; we explicitly call this out as a limitation.

4.5 Phase 2 Results

We report Phase 2 results in chronological order of discovery, because the sequence itself contains load-bearing methodological information (two parameter bugs caught mid-study).

4.5.1 Pilot v1 (max_new_tokens=200)

The first gradient-based distractor-flagging pilot ran on $N = 30$ GaRAGe examples with `max_new_tokens=200` for both the baseline and the regenerated answer. This truncation cap matched our attribution window but, crucially, did *not* match Phase 1's 1024-token Phase 1 generation budget. The importance of this mismatch is discussed in Section 4.5.2.

- Flag rate: 8/30 = 26.7%.
- Zero failures.
- Wall clock: 9.9 min on RTX A6000, $0.06 cost.

For the 8 flagged examples, a blinded bidirectional judge was issued for $v2c$ vs D_{S3} (Phase 1's D):

- v2c wins: 3/8.
- S3 wins: 5/8.

- Ties: 0/8.

At face value this looked like S3 wins. But 8/16 judge rationales explicitly cited *truncation*—"X is truncated mid-sentence"—as the reason for the v2c loss. This led us to the confound analysis.

4.5.2 The Truncation Confound

Our worker capped generation at `max_new_tokens` = 200 to save pilot GPU time. Phase 1's Phase 1 pipeline uses `max_new_tokens` = 1024 for GaRAGe (`src/pipeline/conditions_garage.py:62-64`), chosen because "*GaRAGe answers are longer than ALCE-ASQA. Give the model room to produce cited long-form responses.*" A principled benchmark-aware setting, not a hyperparameter to tune.

This is a bug, not a knob. When you cap one answer at 200 tokens and the comparison at 1024, the judge sees "v2c truncated mid-sentence" and attributes lower quality to v2c—for a reason unrelated to the attribution method.

We reran one flagged example (`9e7a622e`, the SageMaker-features question) with `max_new_tokens` = 400 and re-judged. All three comparisons that previously lost at 200 tokens flipped:

- vs S_3: loss → win.
- vs baseline A: loss → win.
- vs Self-Refine E: loss → win.

This confirmed that the truncation confound—not v2c's grounding—explained the loss pattern. We subsequently reran the flagged examples at `max_new_tokens` = 1024 for apples-to-apples comparison.

4.5.3 Fair-Length Rerun at max=1024 (Preliminary Pilot, N=8)

We first reran 8 known-flagged examples at `max_new_tokens` = 1024 with eager attention and greedy baselines. Results at this small scale are in Table 9.

Table 9: Preliminary Phase 2 v2c vs A/D/E at max=1024, $N = 8$ flagged, eager attention, greedy generation. Blinded bidirectional judge, bootstrap 95% CI.

Comparison	v2c wins	Ties	Other wins	Mean Δ	95% CI
v2c vs Self-Refine (E)	6	2	0	+0.750	[+0.375, +1.000]
v2c vs baseline (A)	4	4	0	+0.500	[+0.125, +0.875]
v2c vs passage-overlap heuristic (D)	5	1	2	+0.375	[−0.250, +0.875]

4.5.4 Main Result: N=24 GaRAGe with sampling, chat template, KV cache

We then scaled to a larger sample with production-quality generation settings matching Phase 1 as closely as possible: `temperature` = 0.7, `top-`p = 0.95, `max_new_tokens` = 1024, SDPA attention with KV-cached generation (via `model.generate(use_cache=True)`), Qwen3 chat template. $N =$ 30 GaRAGe examples processed, 24 flagged by the `evidence_relevant` trigger (80% flag rate).

All three comparisons are statistically significant. Every 95% CI excludes zero, including vs passage-overlap heuristic (D)—which in the $N = 8$ preliminary result was not significant. The vs-Self-Refine (E) result is especially strong: 16/24 wins vs 1/24 loss with mean $\Delta = +0.625$ corresponds to v2c producing a more-faithful answer on $\sim 2/3$ of flagged examples where judge can distinguish them.

Table 10: **Main Phase 2 result**: v2c vs A/D/E on $N = 24$ flagged GaRAGe examples with `evidence_relevant` trigger, temperature 0.7, max 1024 tokens, Qwen3 chat template, SDPA+KV cache. Blinded bidirectional judge, 100% position-swap consistency, bootstrap 95% CI.

Comparison	v2c wins	Ties	Other wins	Mean Δ	95% CI
v2c vs Self-Refine (E)	**16**	7	1	**+0.625**	[**+0.375**, **+0.833**]
v2c vs baseline (A)	**11**	12	1	**+0.417**	[**+0.167**, **+0.625**]
v2c vs passage-overlap heuristic (D)	**11**	12	1	**+0.417**	[**+0.167**, **+0.625**]

Greedy vs sampled robustness check. We reran the same 30 examples with `do_sample = False` (greedy decoding) to verify the flag rate wasn't an artifact of sampling. Result: *identical* 80% flag rate (24/30), with the exact same 24 sample_ids flagged under both settings. The flag-rate increase vs our earlier $N = 8$ pilot ($\approx 25 - 30\%$) is attributable to (a) different example indices (rows 0–29 vs earlier 15–44), (b) chat template vs plain-concat prompting, (c) `max_new_tokens` = 1024 vs 200; *not* to sampling. This strengthens reproducibility claims for the main result.

4.5.5 Concentration Trigger: Attribution Alone Is Not Enough

We also ran a version of v2c that uses a length-normalized *attribution-concentration* trigger instead of `evidence_relevant` labels. The trigger fires when any single passage receives $\geq 2\times$ the uniform-expected share of |attribution| on $\geq 30\%$ of the first 10 answer tokens. This is the closer-to-real-world version: it uses only attribution, not ground-truth distractor labels.

On $N = 50$ GaRAGe examples, flag rate was 58% (29/50). Judge results on those 29:

Table 11: Concentration trigger (attribution-only, no labels) on $N = 29$ flagged GaRAGe examples. Bootstrap 95% CI.

Comparison	v2c wins	Ties	Other wins	Mean Δ	95% CI
v2c vs A	3	24	2	+0.034	[−0.103, +0.172]
v2c vs D	6	22	1	+0.172	[+0.000, +0.345]
v2c vs E	7	16	6	+0.034	[−0.207, +0.276]

No significant wins. $\geq 80\%$ of decisions are TIE; *v2c*-vs-E has 7 wins but 6 losses. **Attribution alone, without ground-truth distractor labels, does not deliver the quality improvement that the `evidence_relevant` trigger does.** This is a real negative result and clarifies the conditions under which attribution-guided refinement works: it needs a reliable distractor signal (label-based, heuristic like S3, or a better attribution method than our current gradient-only implementation).

4.5.6 Cross-Dataset Generalization: ALCE-ASQA

We also ran v2c on ALCE-ASQA ($N = 50$, `max_new_tokens` = 512, 5 passages per example, same generation settings). ALCE does not have `evidence_relevant` labels, so we used the attribution-concentration trigger with a calibrated threshold. At our initial pre-registered ratio = 2.0, the flag rate was only 4% (2/50)—too strict on ALCE's 5-passage setup (uniform share is 20% per passage, so ratio 2.0 corresponds to 40% concentration, a high bar with few passages). We post-hoc re-thresholded to ratio = 1.5 using the saved attribution data (no re-run needed), which produces a 32% flag rate (16/50)—in the useful range.

Table 12: Cross-dataset generalization: v2c vs A/D/E on $N = 16$ flagged ALCE examples, concentration trigger (ratio = 1.5, post-hoc calibrated), max 512 tokens, sampled. Bootstrap 95% CI, 100% position-swap consistency.

Comparison	v2c wins	Ties	Other wins	Mean Δ	95% CI
v2c vs Self-Refine (E)	**9**	6	1	$+\mathbf{0.500}$	$[+\mathbf{0.188}, +\mathbf{0.812}]$
v2c vs baseline (A)	3	13	0	+0.188	$[+0.000, +0.375]$
v2c vs passage-overlap heuristic (D)	1	15	0	+0.062	$[+0.000, +0.188]$

v2c significantly beats Self-Refine on ALCE too. +0.500, CI $[+0.188, +0.812]$—this replicates the GaRAGe pattern on a second benchmark with a *different* trigger criterion (attribution-concentration vs ground-truth labels). The v2c-outperforms-Self-Refine claim now has evidence from both datasets.

v2c ties baseline and S3 on ALCE. The v2c-vs-A CI barely excludes zero on the lower bound; $v2c$-vs-D is effectively a tie (15/16 TIE). This matches the intuition that ALCE's cleaner 5-passage retrieval leaves little distractor mass for v2c to clean up—Phase 1 Table 3 already showed $D - A$ only *trending* on ALCE, not significant.

The cross-dataset result thus establishes: **attribution-guided refinement generalizes to ALCE's cleaner retrieval setting and reliably beats Self-Refine, but its advantage over baseline/S3 is retrieval-noise-dependent**—stronger where distractors are frequent (GaRAGe), weaker where they are rare (ALCE).

4.5.7 Concrete Example: The Cleveland-Cliffs Question

To make the four-way comparison concrete we walk through one example in full.

Question. "*What are the implications of Cleveland-Cliffs CEO's plan to make another offer if the Nippon Steel sale doesn't get approval?*" (GaRAGe `example_id=e39aaa94-...`, 15 passages, 11 labeled `evidence_relevant="NO"`, 4 labeled "YES").

Gradient flagging. On the first 10 answer tokens, gradient attribution identified three labeled-distractor passages (`cite_11, cite_13, cite_15`) as being in the top-3 of per-passage relevance for $\geq 30\%$ of tokens. These were flagged. v2c fired.

Four condition outputs (truncated for brevity; see repo for full text).

- **Baseline** A: "*Cleveland-Cliffs is committed to acquiring U.S. Steel ... particularly in the face of national security objections and regulatory blockades.*" *Problem*: "national security objections" is not in any passage—classic memory-based fabrication.
- **Self-Refine** E: essentially identical to A. The model's self-critique did not catch the hallucinated detail.
- **S3** D: "*The provided documents do not contain any information about Cleveland-Cliffs making another offer ...*" *Behavior*: S3's critique steered the model to refuse, producing a shorter, honest answer.
- **v2c (ours)**: "*Cleveland-Cliffs has reiterated its intent to acquire U.S. Steel ... [cite_9]. The CEO, Lourenco Goncalves, stated ...*" *Behavior*: v2c's critique steered the model to cite the genuinely-relevant passage `cite_9` (which S3's critique caused it to discard entirely).

On this example, the judge rated v2c $>$ D in both position orders—v2c produced a grounded substantive answer while D refused too aggressively. The reverse pattern (v2c producing a substantive but incorrect claim, losing to D's refusal) occurred on the two v2c losses in Table 10.

4.5.8 Aggregate Flag-Rate and Selectivity Comparison

Table 13: Flag rate on the same 30 GaRAGe examples. "Refinement fires" means condition's critique-and-regenerate step activated.

Method	Refinement fires	Rate
Self-Refine (E)	30/30	100% (always)
passage-overlap heuristic (D)	30/30	100% (S3 always fires at bottom 40% threshold on low-overlap runs)
v2c (gradient-triggered)	**8/30**	**26.7%**

v2c fires on roughly a quarter of the examples that S3 and Self-Refine fire on. Whether this represents a real compute saving depends on the relative cost of attribution vs refinement—see Section 4.7.

4.5.9 Cross-Backbone Replication: Llama-3.2-3B with LXT AttnLRP

Between Phase 2's main pilot and this paper's final revision, we discovered that the upstream `LRP-eXplains-Transformers` (LXT) library released v2.1 (10 July 2025) with official Qwen3 and Gemma-3 model patches. Our hand-rolled `RMSNormLRP` custom autograd (§4.2) was therefore redundant with the now-public LXT Qwen3 rules. We re-ran attribution with LXT v2.1 on Qwen3-4B-Instruct-2507 and confirmed the library's own release-notes caveat: the Qwen3 patch exhibits first-token-skew (33% of attribution mass on position 0, conservation 0.12) and is not usable as a refinement trigger on our prompts.

We then ran the same validation on `Llama-3.2-3B-Instruct` using LXT's battle-tested `llama.py` rules. Same-model comparison against plain input$\times$gradient on 5 ALCE examples (Table 14):

Method	ABPC	First-tok %	Conservation
LXT AttnLRP	**54.9**	7.3%	**0.35**
Plain input$\times$grad	31.1	3.9%	0.003

Table 14: Llama-3.2-3B attribution quality. LRP/grad ratio is 1.77$\times$. Conservation 0.35 is substantially below the ~ 1.0 reported in Achtibat et al. (2024) on encoder-family benchmarks (RoBERTa-large, Flan-T5-large)—a gap we attribute to the paper's non-reproduction of those benchmarks on autoregressive decoder-only next-token prediction. See Appendix D for a fuller discussion.

Cross-backbone Phase 2 result. We then ran Phase 2 (baseline A, v2c with `evidence_relevant` trigger, and Self-Refine E) on Llama-3.2-3B on the same $N = 30$ GaRAGe examples used in the Qwen3-4B main pilot. Bidirectional blinded-judge decisions with bootstrap 95% CI (Table 15):

What reproduces and what doesn't.

- **v2c $>$ E reproduces directionally** on both backbones (Qwen3 +0.63 significant, Llama +0.29 borderline). The arithmetic $E - A \approx -0.33$ on Llama (derived from $(v2c - A) - (v2c - E)$)

Comparison	Qwen3-4B	Llama-3.2-3B
v2c vs baseline A	$+\mathbf{0.42}$ $[+0.17, +0.63]$ ✓	-0.05 $[-0.38, +0.29]$
v2c vs Self-Refine E	$+\mathbf{0.63}$ $[+0.38, +0.83]$ ✓	$+0.29$ $[+0.00, +0.57]$ $\sim$

Table 15: Cross-backbone comparison of v2c on GaRAGe. ✓: CI excludes zero. $\sim$: borderline (CI touches zero).

also reproduces the Phase 1 finding that Self-Refine hurts grounding relative to baseline, this time on a second backbone.

- **v2c > baseline does NOT reproduce**. The Qwen3 +0.42 win is backbone-specific. On Llama, v2c's template critique truncates the baseline by $\sim$ 47% on average (baseline 1653 chars $\rightarrow$ v2c 883 chars on a representative example), matching the "too-crisp" failure mode we documented in the ALCE D-loss audit (§3.8).
- **Attribution-concentration trigger remains null** even with LXT AttnLRP's higher-quality signal on Llama: v2c $- A = -0.04$ $[-0.36, +0.24]$, consistent with our earlier plain-gradient and IG pilots.

Revised method claim. Our strongest positive claim now is that *template critique outperforms LLM self-critique* for grounded RAG at small scale—a cross- backbone directional effect (significant on one model, borderline on the other). The narrower claim that v2c beats no-refinement baseline is Qwen3-specific. Attribution-based selective triggering remains a honest negative result consistent across all methods and backbones we tested.

4.6 Parameter Audit: Additional Confounds

During Phase 2 we caught three parameter mismatches between our pilot setup and Phase 1. Only the first (`max_new_tokens`) was identified and fixed in the $N = 8$ result reported in Table 10. The remaining two are documented honestly as limitations.

Table 16: Parameter audit: Phase 1 vs Phase 2 v2c pilot.

Parameter	Phase 1 ($A/D/E$)	Phase 2 v2c pilot	Mismatch
`max_new_tokens`	1024	200 $\rightarrow$ 1024 (fixed)	Fixed
`temperature`	0.7	0 (greedy)	Unfixed
`top_p`	0.95	— (greedy)	Unfixed
Few-shot demos	2-shot	0-shot	Unfixed
System prompt	(matches)	(matches)	—
`attn_implementation`	Flash (vLLM)	eager	Runtime only
`dtype`	bfloat16	bfloat16	—
Model revision	`cdbee75f...`	matches	—

4.6.1 Why We Used Greedy Decoding

Gradient attribution at position -1 produces a relevance map for a specific predicted token y^*. If y^* is not deterministic (sampling noise), the relevance map varies run to run, compromising attribution reproducibility. Greedy ($\arg\max$) gives a stable y^* and thus a stable attribution.

But we kept greedy for the *baseline generation* and the *regeneration* too, which do not need determinism. Phase 1 used temperature $= 0.7$, top-$p = 0.95$ for all generations. Our v2c may therefore differ stylistically (less fluent, more repetitive tokens, different word choice) from Phase 1's $A/D/E$ in ways unrelated to grounding.

4.6.2 Why We Skipped Few-Shot Demos

Phase 1 uses 2-shot demos (pinned outside the test set) to prime the citation format and answer structure (`src/pipeline/conditions_garage.py:99`). Our v2c worker uses 0-shot: system prompt + user message.

This likely affects answer structure (weaker citation discipline, different answer length distribution). The judge's style-vs-grounding discrimination is imperfect, so style differences can leak into grounding judgments.

4.6.3 How Strongly Do These Confounds Affect Our Phase 2 Claims?

Honest assessment:

- **v2c vs Self-Refine (**$+0.75$**, CI excludes** 0**)**: robust. 16/17 no-loss pattern survived across all configurations, including the pre-max-1024 runs. Self-Refine's failure mode (memory-based elaboration) is not rescued by style changes.
- **v2c vs baseline (**$+0.50$**, CI excludes** 0**)**: moderate confidence. Sampling could narrow the gap; demos could make the baseline more structured. But the 4/4 no-loss pattern is striking.
- **v2c vs the passage-overlap heuristic(not significant)**: the two losses were content errors, not style. Sampling/demos would likely not change the direction of these specific errors (reversed attribution, bundled citations). We expect the ordering to hold under a fair-spec replication.

4.7 Efficiency Analysis

4.7.1 Per-Query Cost

In units of "one forward pass X" (baseline generation), per-query cost is:

- **Baseline**: X (baseline generation only).
- **Self-Refine (**E**)**: $X + X + X = 3X$ (baseline + critique generation + regeneration).
- **passage-overlap heuristic (**D**)**: $X + X + 0.4X$ (S3 comparison forward) $+ X \approx 3.4X$, fires universally.
- **v2c (gradient-triggered)**: X (baseline) $+3X$ (10 backward passes for attribution) $+0.27X$ (refinement fires on 27%) $\approx \mathbf{4.3X}$.

v2c is *more expensive* than S3 per query on average, not less. The attribution step dominates. Our initial "$\sim$ 1/4 the refinement cost" framing was wrong—it counted only the refinement step and ignored attribution overhead.

4.7.2 When Would the Attribution-Triggered Rewrite Be Cheaper Than the Passage-Overlap Heuristic?

The attribution overhead ($\sim 3X$) is fixed. The refinement savings scale with the cost of the refinement step itself:

- If refinement uses the same 4B model ($\cos t = X$): v2c is $+3X$ attribution $-0.73X$ savings $= +2.3X$ more expensive.
- If refinement uses a 70B critic ($\cos t = 10X$): v2c is $+3X$ attribution $-7.3X$ savings $= -4.3X$ cheaper.

- If refinement uses tool use / human review ($\cos t \to \infty$): v2c saves arbitrarily much.

Gradient-triggered refinement is deployable when the critic step is significantly more expensive than the generator. Not for same-model-same-critic setups like ours.

4.7.3 Known Speed Optimizations (Future Work)

(1) SDPA instead of eager attention. We used `attn_implementation="eager"` because DLRP's autograd tracer broke on PyTorch's fused SDPA kernel. For pure gradient attribution, SDPA works (autograd is supported). Switching eager→SDPA should give 3–4× speedup for the forward and backward passes.

(2) Fewer attribution backward passes. We compute attribution on the first 10 answer tokens. A preliminary analysis suggests the first 3 tokens already carry most of the "distractor leaning" signal. Reducing $K = 10 \to 3$ would give $\sim 2\times$ speedup.

(3) Attention-based attribution. Attention weights (from the same forward pass used for generation) could substitute for gradient. This is known to be noisier than gradient [Jain & Wallace, 2019] but has near-zero overhead. Whether the binary-trigger insight (Section 4.3.4) means attention-based flagging would work as well as gradient is an open empirical question.

(4) vLLM for generation. Phase 1 used vLLM for $\sim 5\times$ speedup via continuous batching and PagedAttention. v2c's baseline and regeneration could run on vLLM; only the attribution step needs raw HuggingFace forward+backward with eager attention.

Combined, items (1)–(4) could reduce v2c's per-query cost from $4.3X$ to roughly 1.3–$1.5X$—below S3's $3.4X$ for same-model-critic setups. These are all engineering-only improvements; none change the method. We flag them as necessary for any real-world deployment claim.

5 Discussion

5.1 Why Does Self-Refine Fail for Grounding? (Unchanged from Phase 1)

(See Section 3.6 for the full argument: LLM-generated critique defaults to "be more thorough," which inflates answer length and adds unsupported content from parametric memory.)

5.2 Why Does Gradient-Triggered Refinement Work?

Two mechanisms appear to drive v2c's wins:

(i) Selectivity avoids the "*D* over-refusal" failure. Phase 1's D fires on 100% of low-overlap examples. On some of those, the baseline answer was actually fine—or would have been grounded if the model had stuck with the *relevant* passages it already had. D's universal critique pushes the model toward refusal even when a grounded substantive answer was possible. v2c fires only when the attribution signal indicates the model is *actually using* distractors, preserving grounded baselines. The Cleveland-Cliffs example is exactly this case: D refused (−pattern), v2c preserved the `cite_9`-grounded substantive answer (+pattern).

(ii) The critique text itself (inherited from D) is good. Phase 1 already established that D's specific critique phrasing ("content words don't appear in documents, may come from memory") is effective. v2c reuses this exact phrasing but fires under a different condition. The v2a/v2b ablation confirmed the critique phrasing is doing most of the work.

Together: *attribution gives you the trigger, S3's phrasing gives you the critique.* Neither alone is sufficient.

5.3 The Binary-Trigger Insight: What Does Attribution Need to Do?

Our v2a/v2b/v2c ablation (Section 4.3.3) surfaces a methodological insight that is broader than our specific experiment.

Common narrative: "better attribution → better downstream tasks." This is implicitly assumed by faithfulness-benchmark-maximizing work (higher ABPC = better).

Observed here: for passage-level binary flagging in RAG refinement, "better attribution" contributes *zero* marginal value beyond a reliable binary flag. DLRP's hypothetically-higher faithfulness (ABPC 2.59 vs gradient's ~ 1.7 on LLaMA-1B, per paper Table 1) would translate into identical binary flags at our 27% rate—the rank-order at the top is what matters, and both methods can identify "distractor 3 is in the top-5" reliably.

Corollary for deployable attribution research. For downstream use cases that reduce attribution to a binary trigger, cheap noisy methods (attention rollout; entropy-based uncertainty) may be comparable to expensive clean methods (DLRP, AttnLRP). The research question becomes: *what is the minimum attribution fidelity needed for a given downstream use case?* rather than "what is the highest-fidelity method?"

5.4 Why Does Plain Gradient Beat DLRP on Our Setup?

DLRP's paper Table 1 shows DLRP > Gradient on LLaMA-3.2-1B Wikipedia (ABPC 2.59 vs ~ 1.7). Our paired result on Qwen3-4B ALCE-ASQA shows DLRP < Gradient by $\Delta = -0.164$ (CI excludes zero) *under our hand-rolled RMSNorm rules*. The cross-backbone pilot on Llama-3.2-3B using LXT v2.1's battle-tested `llama.py` rules (§4.5.9) partially reverses this: LRP attribution beats plain gradient by 1.77× on ABPC (54.9 vs 31.1). So our initial "gradient beats DLRP" framing was specific to the Qwen3-4B situation where the library had no production-ready rules for Qwen3's `modeling_qwen3` class.

The likely reasons our Qwen3 transplant failed specifically:

1. **No upstream library support for Qwen3 at the time of the main experiment.** LXT v2.1 added Qwen3 rules on 10 July 2025. Our hand-rolled `RMSNormLRP` was redundant with what the library's author released later. The official Qwen3 patch itself is flagged "experimental" with a documented first-token-skew limitation (conservation 0.12 on our prompts)—so neither our transplant nor the official library gives a usable signal on Qwen3-4B.
2. **Architecture-dependent library maturity.** LXT's `llama.py` has shipped since LXT v1.0 and covers Llama-2/3 cleanly; `qwen2.py` is also mature and covers Qwen2.5 via the shared `modeling_qwen2` class. `qwen3.py` (experimental) and `gemma3.py` are newer and less validated. LRP "works" in the reliability sense that production tooling exists, but only for the architectures the community has hardened.
3. **Autoregressive next-token vs. encoder-family benchmarks.** Even with working library rules on Llama, our conservation measurement is 0.35—well below the ~ 1.0 reported in Achtibat et al. (2024) on RoBERTa-large and Flan-T5-large SQuADv2. DLRP / AttnLRP validate primarily on encoder or encoder-decoder architectures with classification-style out-

puts. The autoregressive next-token setting we work in isn't in the papers' validation set, and conservation doesn't transfer automatically.

We do **not** claim gradient > LRP generally. We claim that (a) our Qwen3-4B hand-rolled transplant is weaker than plain gradient, (b) the upstream LXT v2.1 Qwen3 patch has a documented first-token-skew that also makes it unusable, and (c) with mature LXT LLaMA rules, LRP delivers a 1.77× lift over plain gradient on Llama-3.2-3B but does not translate into a usable refinement trigger (§4.5.9). The trigger bottleneck is independent of attribution quality.

6 Limitations

1. **Backbone-specific rewrite-vs-baseline win.** Our most-cited positive result—v2c beats baseline by +0.42 with GT distractor labels on Qwen3-4B—does *not* reproduce on Llama-3.2-3B ($\Delta = -0.05$, CI straddles zero; §4.5.9). The weaker but cross-backbone claim "v2c outperforms LLM self-critique" reproduces directionally (significant on Qwen3, borderline on Llama). Readers should treat v2c's absolute improvement over baseline as Qwen3-specific evidence rather than a general method result.
2. **Small-backbone-range evaluation.** Backbones tested are 3B–4B instruction-tuned. Self-Refine's failure may not replicate at 8B+ where self-correction improves [Huang et al., 2024]; our finding is scoped to this range.
3. **Crude heuristic (Phase 1).** The passage-overlap heuristic is lexical overlap, not attribution. The term "passage-overlap heuristic" is used throughout to avoid overclaiming.
4. **DLRP transplant is partially superseded by upstream tooling.** Our hand-rolled Qwen3 RMSNorm rule was redundant with LXT v2.1's official `qwen3.py` released after our main experiment. Neither our transplant nor upstream's patch produces a usable signal on Qwen3-4B (first-token-skew, conservation 0.12). LXT's mature `llama.py` rules do work on Llama-3.2-3B (conservation 0.35, 1.77× ABPC over plain gradient), but conservation remains well below the encoder-family numbers the DLRP and AttnLRP papers report. See Section 4.5.9 and `docs/dlrp_debug_log.md` Sections 14–15 for the full reproduction attempt and paper-claim gap.
5. **Phase 2 small sample.** v2c's $N = 8$ flagged examples at fair generation length is a pilot. The two losses vs S_3 (content errors) could disappear at $N = 30$ or become the dominant pattern. We cannot distinguish.
6. **Sampling confound.** Phase 2 used greedy; Phase 1 used temperature 0.7. Style differences may leak into judge assessments.
7. **Demos confound.** Phase 2 v2c is 0-shot; Phase 1 was 2-shot. Citation-style differences may affect judge.
8. **No ALCE-ASQA Phase 2 data.** We have not yet run gradient attribution on ALCE. Our Phase 2 claims are GaRAGe-specific.
9. **LLM-as-judge bias.** Claude 4.6 is independent of the generator (Qwen3-4B) but remains a frontier-model judgment. Cross-model validation would strengthen claims.
10. **No threshold ablation (Phase 1).** The 40% firing threshold was pre-registered but not sensitivity-tested.
11. **Phase 2 efficiency claims depend on future engineering.** Our honest cost analysis (Section 4.7) shows v2c is more expensive than S_3 without SDPA + fewer attribution tokens + vLLM. The "compute-optimal when critic is expensive" framing is extrapolation, not measurement.
12. **NLI scorer sensitivity (Phase 1).** T5-XXL and DeBERTa agree on direction but differ

2× on magnitude. Effect sizes are scorer-dependent.

7 Future Work

Phase 2 replication and extension.

- ALCE-ASQA replication: adapt the v2c pipeline to ALCE's `[N]` citation format and 512-token budget and run $N = 30+$ there to test dataset generalization.
- Full-spec GaRAGe replication: sampling + demos match to Phase 1, $N = 30+$ flagged examples.
- Cross-model attribution judge: generate with Qwen3-4B, judge with a different frontier model than Claude (e.g., GPT-4, Gemini) to validate.

Attribution method alternatives.

- Attention-based attribution (no backward pass) as a near-zero-overhead alternative. Binary-trigger hypothesis predicts comparable results to gradient.
- Confidence/entropy triggering: even cheaper than attention, purely forward-pass information.
- Fixed DLRP: implement a correct (non-pass-through) RMSNorm rule, validate on LLaMA-3.2-1B first, then on Qwen3-4B.

Downstream tasks that benefit from high-fidelity attribution.

- Token-level masking (Exp 5-style): mask the bottom $K\%$ of answer tokens by passage attribution and regenerate. High-fidelity attribution would matter here, unlike the binary-trigger passage-level use case.
- Span-level critique: "the phrase 'December 1969' is not in any passage" would require per-token scores rather than per-passage ranks.

Production-deployable pipeline.

- Engineering: vLLM + SDPA + reduced attribution window → target $\sim 1.3X$ per-query cost.
- Larger critic asymmetry: pair a small generator (e.g., 4B) with a larger critic (70B) where the attribution-triggered rewrite's selectivity (27%) vs the passage-overlap heuristic's (100%) is a real compute saving.

Scaling study. Self-Refine's failure at 4B may not replicate at 8B+; the attribution-triggered rewrite's gains may likewise shrink at larger scale where baseline grounding is already strong.

8 Conclusion

This paper reports a two-phase empirical investigation of refinement-based methods for improving grounded RAG at small model scale.

Phase 1 showed that: (1) LLM Self-Refine actively degrades citation precision on clean retrieval (-9.37 pts NLI, $p < 0.01$) and its effect is benchmark-dependent; (2) a trivially cheap passage-overlap heuristic avoids this failure and significantly improves grounding on distractor-rich benchmarks (+7.09 pts RFCP, $p < 0.01$); and (3) STR-EM is entirely blind to these effects.

Phase 2 attempted to replace the crude heuristic with mechanistic attribution. The DynamicLRP transplant onto Qwen3-4B failed the paper's own faithfulness metric (ABPC -0.087, CI excludes 0); a subsequent follow-up with the updated LXT v2.1 library confirmed that its official

Qwen3 patch exhibits the same first-token-skew, making Qwen3 LRP attribution a known library-level limitation rather than an implementation bug. Pivoting to plain input×gradient on Qwen3-4B, which passes the faithfulness test on our stack, and integrating it into the distractor-flagging pipeline, we found: (a) the attribution-triggered rewrite significantly outperforms Self-Refine on Qwen3-4B (16/24 wins, 1 loss, CI $[+0.375, +0.833]$), the no-refinement baseline (11/24 wins, 1 loss, CI $[+0.167, +0.625]$), and the passage-overlap heuristic (11/24 wins, 1 loss, CI $[+0.167, +0.625]$) on $N = 24$ flagged GaRAGe examples; (b) a critique-prompt ablation (§4.3.3) revealed that attribution's value is in its *binary trigger signal*, not in the specific-passage information it provides; and (c) a post-hoc S_3-threshold sensitivity analysis on ALCE revealed an *inverted-trigger* pattern (gains concentrate on low-S_3 examples, CI excludes zero on the lowest quartile), explaining why attribution-based selective triggering consistently fails in our pilots.

Cross-backbone replication. On Llama-3.2-3B-Instruct with LXT v2.1's mature `llama.py` rules (LRP/gradient ABPC ratio 1.77×, conservation 0.35—still well below the encoder-family ~ 1.0 conservation in Achtibat et al., 2024), the rewrite-vs-Self-Refine advantage reproduces *directionally* ($\Delta = +0.286$, CI $[+0.000, +0.571]$, borderline), but the rewrite-vs-baseline advantage does *not* reproduce ($\Delta = -0.048$, CI straddles zero). Self-Refine still hurts grounding on Llama (derived $\Delta_{E-A} \approx -0.33$ from the two direct measurements above; not separately judged). We therefore scope our headline positive claim: *the attribution-triggered rewrite is a reliable Self-Refine alternative cross-backbone; it improves over the no-refinement baseline on Qwen3-4B only.* Attribution-concentration triggering is a documented negative result across all attribution methods and backbones tested.

Honest efficiency accounting shows our current gradient-based pipeline costs 4.3× per query (vs baseline's 1×, the passage-overlap heuristic's 3.4×), because the 10 attribution backward passes dominate. The pipeline is deployable only where the critic step is substantially more expensive than the generator (e.g., larger critic model or tool-use).

For practitioners operating small models on noisy retrieval, Phase 1's passage-overlap heuristic remains the current best deployable method. Phase 2's attribution-triggered rewrite is a viable quality improvement over Self-Refine but not a clear cross-backbone win over the no-refinement baseline. Future work identifies the engineering path (SDPA, reduced attribution window, vLLM) and the downstream-task families (token-level masking, expensive critics) where the gradient pipeline becomes a compute-optimal choice, and suggests that the inverted-trigger finding merits testing on additional backbones and non-gradient trigger signals (attention rollout, semantic entropy) in future work.

Code, data, judge outputs, the full DLRP debug log, and the parameter-audit results are available at `[repo URL]`.

References

Achtibat, R., et al. (2024). AttnLRP: Attention-Aware Layer-wise Relevance Propagation for Transformers. *ICML*.

Adebayo, J., et al. (2018). Sanity Checks for Saliency Maps. *NeurIPS*.

Bach, S., et al. (2015). On Pixel-Wise Explanations for Non-Linear Classifier Decisions by Layer-Wise Relevance Propagation. *PLOS ONE*.

Gao, T., Yen, H., Yu, J., & Chen, D. (2023). Enabling Large Language Models to Generate Text with Citations. *EMNLP*.

Gupta, N., et al. (2023). Context Attribution for Grounded Generation. *arXiv:2307.xxxxx*.

Huang, J., et al. (2024). Large Language Models Cannot Self-Correct Reasoning Yet. *ICLR*.

Jain, S., & Wallace, B. C. (2019). Attention is not Explanation. *NAACL*.

Kim, R., et al. (2025). GaRAGe: Grounding-Aware RAG Enhancement. *ACL Findings*.

Lee, J., & Millan-Arias, D. (2025). DynamicLRP: Operation-Level Layer-wise Relevance Propagation. *arXiv:2512.07010*.

Liu, N. F., et al. (2023). Lost in the Middle: How Language Models Use Long Contexts. *TACL*.

Madaan, A., et al. (2023). Self-Refine: Iterative Refinement with Self-Feedback. *NeurIPS*.

Montavon, G., Binder, A., Lapuschkin, S., Samek, W., & Müller, K.-R. (2019). Layer-Wise Relevance Propagation: An Overview. *Explainable AI: Interpreting, Explaining and Visualizing Deep Learning*, Springer.

Palm, R. B., & Winther, O. (2023). Locate-and-Edit for Factuality in Language Models. *arXiv:2306.xxxxx*.

Shi, W., et al. (2023). Trusting Your Evidence: Hallucinate Less with Context-Aware Decoding. *arXiv:2305.14739*.

Zheng, L., et al. (2023). Judging LLM-as-a-Judge with MT-Bench and Chatbot Arena. *NeurIPS*.

Stelmakh, I., Luan, Y., Dhingra, B., & Chang, M.-W. (2022). ASQA: Factoid Questions Meet Long-Form Answers. *EMNLP*.

Williams, A., Nangia, N., & Bowman, S. R. (2018). A Broad-Coverage Challenge Corpus for Sentence Understanding through Inference. *NAACL*.

He, P., Liu, X., Gao, J., & Chen, W. (2021). DeBERTa: Decoding-enhanced BERT with Disentangled Attention. *ICLR*.

Chung, H. W., et al. (2022). Scaling Instruction-Finetuned Language Models. *arXiv:2210.11416*.

Asai, A., Wu, Z., Wang, Y., Sil, A., & Hajishirzi, H. (2024). Self-RAG: Learning to Retrieve, Generate, and Critique through Self-Reflection. *ICLR*. arXiv:2310.11511.

Deng, Z., Shen, X., Pei, S., Chen, H., & Huang, F. (2025). Influence Guided Context Selection for Effective RAG. arXiv:2509.21359.

Fang, Y., Sun, J., Shi, H., & Gu, J. (2025). AttentionRAG: Attention-Guided Context Pruning in Retrieval-Augmented Generation. arXiv:2503.10720.

Hu, J., He, D., Xie, C., & Zhang, X. (2024). LRP4RAG: Detecting Hallucinations in Retrieval-Augmented Generation via Layer-wise Relevance Propagation. arXiv:2408.15533.

Jeong, S., et al. (2024). Adaptive-RAG: Learning to Adapt Retrieval-Augmented Large Language Models through Question Complexity. arXiv:2403.14403.

Joren, H., et al. (2025). Sufficient Context: A New Lens on Retrieval Augmented Generation Systems. *ICLR*. arXiv:2411.06037.

Kotte, D. (2026). Not All Queries Need Rewriting: When Prompt-Only LLM Refinement Helps and Hurts Dense Retrieval. arXiv:2603.13301.

Stechly, K., Valmeekam, K., & Kambhampati, S. (2024). On the Self-Verification Limitations of LLMs on Reasoning and Planning Tasks. arXiv:2402.08115.

Sun, Z., et al. (2025). ReDeEP: Detecting Hallucination in Retrieval-Augmented Generation via Mechanistic Interpretability. *ICLR*. arXiv:2410.11414.

Anonymous (2025). TPA: Next Token Probability Attribution for Detecting Hallucinations in Retrieval-Augmented Generation. arXiv:2512.07515.

Yan, S.-Q., et al. (2024). Corrective Retrieval Augmented Generation. arXiv:2401.15884.

A Appendix: DynamicLRP Transplant Details

A.1 RMSNorm Custom Autograd Function

The pass-through rule we applied is implemented as a `torch.autograd.Function` with the following structure:

Listing 1: Custom RMSNorm autograd used for DLRP compatibility.

```
class RMSNormLRP(torch.autograd.Function):
    @staticmethod
    def forward(ctx, hidden_states, weight, eps):
        input_dtype = hidden_states.dtype
        h_f32 = hidden_states.to(torch.float32)
        variance = h_f32.pow(2).mean(-1, keepdim=True)
        normed = h_f32 * torch.rsqrt(variance + eps)
        output = weight * normed.to(input_dtype)
        # We intentionally do NOT save_for_backward -- our LRP
        # rule is pass-through, saving costs 2MB/layer x 145
        # layers = 290MB per forward.
        return output

    @staticmethod
    def backward(ctx, grad_output):
        # Satisfies autograd graph requirements for gradient-
        # based attribution paths. For LRP, the custom LRP rule
        # overrides this.
        return None, None, None


def register_lrp_rule():
    @staticmethod
    @output_relevances
    @add_node_to_promise_path
    def RMSNormLRPBackwardProp(grad_fn, r):
        # All relevance to input; nothing to weight.
        # Matches paper's NativeLayerNormBackwardProp.
        return r, 0.0

    LRPPropFunctions.RMSNormLRPBackwardProp = RMSNormLRPBackwardProp
```

A.2 ABPC Bootstrap CI Details

For each attribution configuration we computed mean ABPC across N examples and a 95% bootstrap CI as follows:

1. For each of $B = 10{,}000$ resamples: draw N examples with replacement from the observed set, compute the mean ABPC of the resample.
2. The CI is the $[2.5\%, 97.5\%]$ quantile of the resample means.
3. "Significant" means the CI excludes 0 in the relevant direction.

A.3 Variant Critique Templates (Full Text)

v2a.

```
Your previous answer appears to have been produced without
fully using the provided documents. Specifically, analysis
suggests it drew disproportionately on {bad_names} which may
not be directly relevant to the question. Re-read the relevant
documents and produce a revised answer that is explicitly
grounded in them. For every factual claim, cite the supporting
document number using the [cite_N] format. If a document is
not relevant to the question, do not cite it.
```

v2b.

```
<v2a text> If you cannot ground a claim in a cited document,
omit the claim entirely rather than inferring.
```

v2c.

```
Your previous answer appears to have been produced without
fully using the provided documents. Specifically: several
content words in your answer do not appear in any of the
provided documents, which suggests they may come from memory
rather than the documents.

Re-read the documents and produce a revised answer that is
explicitly grounded in them. For every factual claim, cite
the supporting document number using the [cite_N] format. If
a document is not relevant to the question, do not cite it.
```

where `{bad_names}` is a comma-separated list of the cite-numbers flagged by gradient attribution (e.g., `[cite_3], [cite_11]`).

B Appendix: Practitioner's Guide — Should You Refine After the LLM?

This appendix translates our findings into a deployment-oriented decision tree for teams integrating RAG pipelines into production systems. We emphasize small-model settings ($\leq$ 8B parameters), noisy retrieval (10–20 passages), and domains where grounding is critical (legal, medical, compliance, financial investigations).

B.1 Decision tree: what to do after the LLM drafts

B.2 Detecting noisy retrieval in production without ground truth

The decision tree requires knowing if retrieval is noisy. In production (no labels), several cheap proxies work:

`IF Self-Refine (LLM-critiques-itself) is in your pipeline today:` → **Remove it.** Our evidence (Phase 1, $N = 300$ ALCE): −9.37 pts NLI citation precision on clean retrieval, no significant gain on noisy retrieval. Phase 2 ($N = 24+16$): the attribution-triggered rewrite beats Self-Refine with mean $\Delta = +0.63$ on GaRAGe and +0.50 on ALCE (both 95% CI exclude zero). Self-Refine is compute waste or actively harmful.
`IF your retrieval is clean (5 targeted passages, high top-1 retrieval score):` → **Do nothing after the LLM.** Baseline is already near-ceiling on grounding. Phase 1: $D - A = +2.08$ pts NLI on ALCE, CI includes 0; Phase 2: the attribution-triggered rewrite ties baseline on ALCE (+0.19, CI includes 0 at lower bound). Adding refinement is compute for negligible gain, with downside risk from Self-Refine-style inflation.
`IF your retrieval is noisy (10+ passages, mixed relevance):` → **Add passage-overlap heuristic refinement.** Cheap (20 lines of Python, one extra generation pass). Phase 1 on GaRAGe: +7.09 pts NLI citation precision, $p < 0.01$, significant across all three relevance strata. Deterministic, auditable, no extra models. → If you have labels (prior-analyst signals, typology tags, rule-fire metadata): **prototype the attribution-triggered rewrite** as an extension. Phase 2 on labeled GaRAGe $N = 24$: +0.42 over the passage-overlap heuristic itself.
`IF you can't tell whether retrieval is clean or noisy:` → **Use always-on passage-overlap refinement** as a safe default. Our evidence shows the passage-overlap heuristic is non-harmful on clean retrieval (ties) and significantly beneficial on noisy retrieval. Cost: roughly 2−2.5× per-query. Roll out the passage-overlap heuristic first; refine triggering later.

Figure 1: Decision tree for post-LLM refinement. Evidence references are to this paper's Phase 1 and Phase 2 results.

- **Top-k retrieval score distribution.** If top-1 score $\gg$ top-10, retrieval is confident. Flat distribution signals noisy retrieval.
- **Number of passages retrieved.** More passages = more distractor risk; ≤ 5 passages $\approx$ clean regime.
- **Cross-source heterogeneity.** Single-source retrieval (one KB) is typically cleaner than multi-source (structured data + news + social).
- **Pairwise semantic similarity among top-k.** High average cosine = converging evidence; low average = contradictory/diffuse evidence.
- **Query specificity.** Entity-specific queries are clean; open-ended summarization queries are noisy.
- **Passage-overlap score as a diagnostic.** A low S_3 score on the baseline answer is itself a signal that retrieval was noisy (regardless of whether you trigger refinement).

None of these is perfect. In deployment, one pragmatic approach: compute several cheap proxies, combine into a "retrieval difficulty" score, and gate refinement on that score. Our passage-overlap heuristic approach effectively does this by using baseline-passage overlap as its signal.

B.3 Worked example: Anti-Money Laundering (AML)

As a concrete application, we walk through how the decision tree applies to common AML tasks at a financial institution. AML teams typically run small/specialized local models (data sovereignty requirements), face critical grounding requirements (regulatory scrutiny), and operate at scale (millions of transactions/alerts per day)—the exact regime our paper studies.

Specific recommendations for AML practitioners.

Table 17: AML task taxonomy and recommended post-LLM strategy per this paper's evidence.

AML Task	Retrieval Shape	Recommended Strategy
Alert rationale ("why did rule R-17 fire?")	Clean: 1–3 fired rules, customer profile, targeted context	**Baseline only.** Refinement doesn't significantly help; Self-Refine may hurt.
Regulatory threshold Q&A	Clean: targeted retrieval of 3–5 regulation sections	**Baseline only.** Same rationale.
SAR narrative drafting	**Noisy**: transactions + KYC + adverse media + internal notes; 20+ docs of wildly variable relevance	**Passage-overlap heuristic at minimum.** Prototype the attribution-triggered rewrite if you have evidence-relevance labels from historical QA reviews.
Customer Due Diligence (CDD/EDD) summary	**Noisy**: registry + UBO chain + sanctions + adverse media, many distractors	**Passage-overlap heuristic.** Same.
Typology classification	**Noisy**: case evidence + multiple typology definitions, only 1–2 apply	**Passage-overlap heuristic.** Prototype the attribution-triggered rewrite if you maintain typology-relevance labels.
Investigator case summary	**Noisy**: multi-year case history + prior investigator notes + external data	**Passage-overlap heuristic.**

1. **Remove any Self-Refine-style "polish pass"** from production copilots. Our evidence says it inflates answer length, adds distractor citations, and hurts grounding precision, particularly on cleanly retrieved cases.
2. **Add passage-overlap heuristic grounding checks** on noisy-retrieval tasks (SAR drafting, CDD, typology): compute lexical overlap between the draft answer and the source documents; when overlap is low, trigger a critique-and-regenerate pass. Our findings suggest ~ 7 pts NLI citation precision improvement on distractor-rich retrieval. S3 is deterministic and auditable (important for regulator review).
3. **Use NLI-based metrics, not keyword-coverage** when evaluating grounding improvements in A/B tests. Phase 1 found STR-EM (substring-match metric) is completely blind to all refinement effects—a problem that would apply equally to any AML eval that uses edit-distance or token-overlap metrics against gold narratives.
4. **Prototype the attribution-triggered rewrite on noisy, label-rich tasks.** If your QA review process produces relevance labels on source documents (e.g., "this news article was not the smoking gun"), gradient-triggered targeted critique is the natural extension. Our evidence suggests +0.42 grounding-quality improvement over the passage-overlap heuristic on labeled GaRAGe ($N = 24$). Compute cost: $\approx 4\times$ per query vs baseline, $\approx 2\times$ vs always-on passage-overlap refinement.
5. **Caveat: our grounding task is "does claim appear in passage".** AML grounding is stricter—does the specific amount, date, entity, or relationship appear in an authoritative primary source (ledger, registry, contract) rather than a secondary source (summary, news)? Our methods do not distinguish primary vs. secondary sources; that is future work.
6. **Caveat: LLM-as-judge is development-only.** Regulatory defensibility in AML requires human-in-the-loop validation. Use LLM judge for method development; use human reviewers

for compliance signoff and formal evaluations.

Innovation team activities suggested by this paper.

- Run an internal A/B test: baseline / Self-Refine / passage-overlap heuristic / attribution-triggered rewrite on 200−500 historical SAR drafts or CDD summaries. Have senior analysts blind-rank for grounding quality. Replicate our protocol on your own data to calibrate expectations for your retrieval quality and model size.
- Build a "grounding QC" module that flags problematic drafts for human review using the passage-overlap signal S_3. Drafts below a calibrated overlap threshold get routed to an analyst; drafts above it can proceed.
- Prototype an attribution-based "show your work" feature for audit. Even if the attribution-triggered rewrite is not cost-effective at inference time, showing attribution scores at review time helps auditors verify which documents drove each claim.