The Trigger Is in the Embedding: Supervision-Free Discovery and Diagnosis of Universal Adversarial Triggers in Fact Verification

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Abstract

Despite their widespread adoption, the robustness of fact-checking models to adversarial perturbations remains underexplored. Existing approaches are model-specific and require gradient access or dataset-dependent optimization, with implications for generalization, efficiency, and semantic validity. We introduce FACTFLIP, a framework for analyzing robustness in claim verification models via universal adversarial triggers. FACTFLIP identifies perturbative trigger words through a lightweight, model-only procedure that ranks candidates by the alignment of their non-contextualized embeddings with class-specific decision directions, without labeled data or gradient access. Trigger discovery is decoupled from claim perturbation via an LLM-based perturb-and-verify pipeline that preserves semantic validity. Experiments show that FACTFLIP discriminates between strong and weak triggers, composes them costructively, and rivals fully supervised gradient-based baselines. We further show that FACTFLIP doubles as a diagnostic lens, surfacing socially salient model biases without any task-specific supervision.

1 Introduction

An increasing share of the population relies on online sources as a primary means of accessing information (Reuters Institute, 2024). At the same time, online information environments are characterized by a growing prevalence of false or misleading content, amplified by the scale and speed of digital dissemination (WEF, 2024; Vosoughi et al., 2018). Ensuring the correctness of publicly accessible information is a critical societal challenge, motivating the development of tools to support fact-checking at scale (IFCN, 2025; Mirza et al., 2023).

Several approaches in the fact-checking literature evaluate robustness by modifying claims or evidence through input-specific transformations.

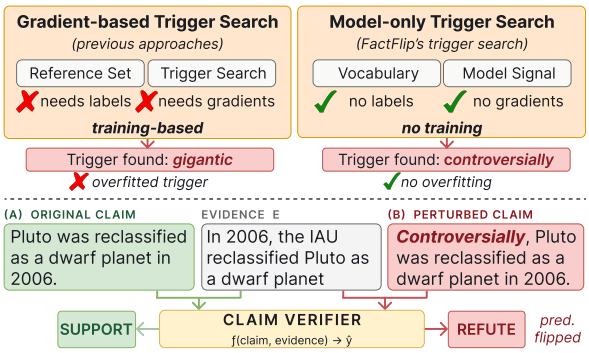

Figure 1: Motivating comparison between trigger-search with gradient-based approaches and with FACTFLIP. The discovered triggers can be inserted inside claims to flip a target model prediction.

These transformations operate at different levels, including character-level noise (Mamta and Cocarascu, 2025), word-level substitutions (Kim and Allan, 2019; Mamta and Cocarascu, 2025; Hidey et al., 2020), and syntactic changes (Thorne et al., 2019; Mamta and Cocarascu, 2025; Hidey et al., 2020), as well as content reformulations generated by LLMs, such as paraphrasing (Thorne et al., 2019; Atanasova et al., 2020; Niewinski et al., 2019; Magomere et al., 2025), colloquialization (Kim et al., 2021), and dialectal variation (Magomere et al., 2025).

Other approaches instead study robustness by inducing prediction flips through the insertion of *adversarial triggers*, referred to as *universal* when they are input-agnostic, as illustrated in Figure 1. In this setting, triggers are tokens that systematically bias model predictions toward a target class when injected into otherwise unchanged inputs (Wallace et al., 2019). While this paradigm aims to reveal model-level vulnerabilities and global decision biases, existing methods (Shin et al., 2020; Atanasova et al., 2020; Ebrahimi et al., 2018) rely on gradient-based optimization over a reference dataset.

In practice, this requirement is a substantial ob-

stacle. Probing a fact-checking model with existing methods requires first collecting or manually annotating a set of claim–evidence–label examples in the target domain, and then running an iterative gradient-based search over those examples to discover triggers. This process is costly in both annotation effort and computation, and it is not applicable when training data are proprietary or when gradient backpropagation is too expensive. Even when feasible, existing approaches raise two additional concerns, namely *generalization* and *semantic validity*. First, the resulting triggers may capture artifacts of the specific optimization set rather than intrinsic vulnerabilities of the model itself, thereby limiting their ability to generalize beyond the reference data. Second, direct token insertion often produces unnatural or meaning-altering claims, making it difficult to disentangle successful attacks from semantic drift (Morris et al., 2020; Zhou et al., 2024; Atanasova et al., 2020). A reliable diagnostic of lexical vulnerabilities in claim verification models therefore requires a method capable of deriving triggers directly from the model, without dependence on annotated datasets.

To address these limitations, we introduce FACTFLIP[1], a framework for the discovery of Universal Adversarial Triggers in claim verification models. FACTFLIP identifies perturbative candidate words directly from the target model. Specifically, the framework feeds the model with individual candidate tokens and ranks them, for each class, according to the alignment between their non-contextualized embeddings and the corresponding class-specific decision direction. Trigger discovery therefore depends exclusively on the target model, reducing the computational cost of trigger search while mitigating the risk of overfitting to dataset-specific artifacts. The proposed approach is grounded in the *distributional hypothesis* (Harris, 1954), the observation that the meaning of a word is reflected in the contexts in which it typically occurs. Under this perspective, non-contextualized word embeddings can be interpreted as aggregate representations of the distribution of contexts associated with a token, often dominated by its most frequent semantic usage. We hypothesize that, if a word embedding already exhibits a strong alignment with a target class along the model's decision direction in isolation, this signal will largely persist when the word is injected *universally* across arbitrary claims. Universal triggers can therefore be interpreted as tokens whose embeddings are intrinsically aligned with class-specific decision regions of the model, reducing trigger discovery to a lightweight model-centric computation that requires neither annotated data nor gradient optimization.

To ensure semantic validity, FACTFLIP incorporates an LLM-based framework that integrates selected tokens coherently into claims (*LLM as a Perturber*) and verifies entailment constraints between the original and perturbed claims (*LLM as a Verifier*). While similar generate-and-verify paradigms have been explored in prior work (Magomere et al., 2025), we employ this framework to explicitly enforce entailment preservation in adversarial fact-checking (Atanasova et al., 2020), ensuring that perturbations preserve the original claim meaning while remaining linguistically natural and realistic.

To assess the quality of our approach, we evaluate FACTFLIP on six fine-tuned versions of `RoBERTa` (Liu et al., 2019) and on `Qwen2.5-14B-Instruct` (Qwen, 2024) used as a zero-shot classifier. We obtain three main principled insights into the robustness and failure modes of claim verification models. In particular, our analysis shows that (**RQ1**) the triggers discovered by FACTFLIP are effective: they discriminate between high- and low-impact perturbations, compose constructively when injected jointly and, despite using no annotated data, nearly match gradient-based supervised baselines (§4.1); (**RQ2**) enforcing linguistic plausibility and entailment preservation during trigger injection introduces a clear trade-off between semantic validity and attack strength, with lexically sensitive models remaining vulnerable even under strict constraints (§4.2); and (**RQ3**) universal adversarial triggers provide a powerful diagnostic lens, uncovering systematic socially salient biases rooted in both pre-training and fine-tuning rather than dataset-specific artifacts (§4.3).

2 Related Work

Adversarial Attacks in NLP. Adversarial text generation is widely used to assess the robustness of NLP models. Early approaches such as SEAR (Ribeiro et al., 2018) relied on rule-based perturbations to generate grammatically altered but semantically equivalent texts. Subsequent work extended this paradigm through controlled modifications of text properties, such as sentiment

[1] https://anonymous.4open.science/r/FactFlip-8DCE

changes (Ribeiro et al., 2020; Alzantot et al., 2018; Jia et al., 2019), or through character-level (Gao et al., 2018; Li et al., 2019; Boucher et al., 2021) and synonym-based lexical substitutions guided by cosine similarity (Jin et al., 2020; Li et al., 2019), masked language modeling (Garg and Ramakrishnan, 2020; Li et al., 2020, 2021), or external knowledge bases (Zang et al., 2020; Ren et al., 2019).

More recent work leverages gradient information in supervised settings to identify target input elements and perform perturbations (Ebrahimi et al., 2018; Yoo and Qi, 2021; Yoo et al., 2020; Hou et al., 2023; Yuan et al., 2023). In particular, HotFlip (Ebrahimi et al., 2018) is an instance-based gradient attack that identifies character- or word-level substitutions maximizing the loss for a given input via a first-order approximation. The same first-order optimization principle has also been adopted beyond instance-level perturbations to search for universal adversarial triggers, i.e., short token sequences that induce misclassification when inserted into arbitrary inputs (Wallace et al., 2019). Representative approaches in this category include AutoPrompt (Shin et al., 2020), which performs an iterative, gradient-guided search over trigger tokens and serves as a gradient-based baseline for universal trigger discovery.

Although several recent robustness-analysis methods introduce additional constraints on the generated perturbations, such as semantic similarity, plausibility, or fluency preservation (Song et al., 2021; Guo et al., 2021; Xu and Wang, 2024), many of them retain the same underlying gradient-guided trigger-search paradigm. The same optimization principle has also remained influential beyond conventional adversarial attacks, notably in modern prompt-optimization methods for LLM jailbreaking (Zou et al., 2023; Jia et al., 2025; Wang et al., 2025). In contrast to these approaches, which typically rely on data-dependent and gradient-based optimization to discover discrete trigger tokens, FACTFLIP identifies perturbative triggers through a lightweight model-only scoring mechanism that requires neither training data nor gradient access.

Model Robustness for Claim Verification. Several approaches adapt adversarial attack techniques to assess the robustness of claim verification models. Thorne et al. (2019) evaluate robustness through rephrasing strategies that introduce linguistic patterns absent from the training data, including synonym substitutions derived from WordNet (Miller, 1994) and rule-based transformations. Other work examines robustness under more realistic perturbation scenarios, such as discursive claims (Kim et al., 2021), misinformation edits (Magomere et al., 2025), i.e., perturbations that frequently occur on social media, character-level or grammatical-structure modifications (Mamta and Cocarascu, 2025; Hidey et al., 2020), and evidence omission (Atanasova et al., 2022; Abdelnabi and Fritz, 2023). Schuster et al. (2019, 2021) show that negation is strongly associated with the *refute* label in the FEVER (Thorne et al., 2018) dataset, and that models trained on FEVER learn to exploit such correlations, which can be leveraged to mislead their predictions. More recent work generates adversarial claims through LLM-based paraphrasing, using either unconstrained rewriting followed by post-hoc label validation (Magomere et al., 2025), or supervised, gradient-based optimization to promote label cohesion (Niewinski et al., 2019). While these methods improve fluency, they do not explicitly search for maximally perturbative universal triggers. Among adversarial approaches to claim verification (Liu et al., 2025), Atanasova et al. (2020) remains the only work explicitly targeting universal triggers. Atanasova et al. (2020) builds on the gradient-based trigger-discovery paradigm underlying AutoPrompt and jointly fine-tunes a generative model to discover triggers and apply perturbations under semantic-similarity constraints. In contrast, FACTFLIP identifies triggers directly from the target model without fine-tuning, and decouples trigger discovery from claim generation, enabling zero-shot discovery and application of universal triggers. In §4.1, we therefore compare FACTFLIP against AutoPrompt while using our perturb-and-verify pipeline for claim generation.

3 Approach

FACTFLIP follows a two-stage pipeline (see Figure 2), consisting of an *extraction* stage and a *generation* stage.

3.1 Extraction Stage

Given a model M, a vocabulary V, and a set of claim–evidence–label tuples (c_i, e_i, l_i), with $l_i \in \{\textit{Support}, \textit{Refute}, \textit{NEI}\}$, our goal is to identify universal trigger words $p \in V$ that, when inserted into a claim c_i, increase the likelihood of misclassification toward a target class $t \neq l_i$. To identify the most perturbative triggers, we leverage the class-

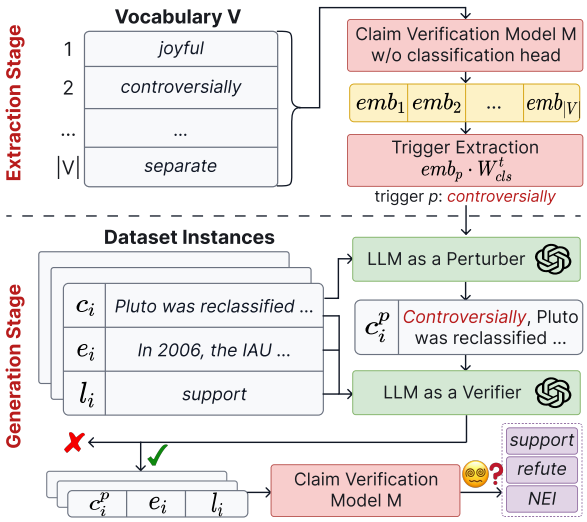

Figure 2: FACTFLIP's pipeline. FACTFLIP identifies the most perturbing triggers (*Extraction*), then integrates and validates the trigger inside the claim (*Generation*).

specific weight vectors of the model's classification head as reference decision directions. In particular, for the *Support* and *Refute* labels, given the classification head h_{cls}, we compute the inner product between the non-contextualized embedding emb_p of each candidate trigger word $p \in V$ and the corresponding class-specific weight vector W^t_{cls}. For the *NEI* label, we instead score p by averaging the *NEI* score obtained from the templates "*The statement "p" is true*" and "*The statement "p" is false*". This controls a known bias toward *NEI*: tokens with weak semantics can spuriously increase the *NEI* logit. The templates add informative context, so high-scoring words are more likely to be genuinely perturbative toward *NEI* rather than merely uninformative. Finally, the embedding emb_p is computed using the embedding of the first token.

The resulting score $emb_p \cdot W^t_{\text{cls}}$ measures the alignment of a word with the decision direction associated with class t: higher values indicate words that, in isolation, bias the model toward that class. We exclude the bias term of h_{cls} to focus on directional alignment rather than global class priors. We hypothesize that if a word's non-contextualized embedding already carries strong evidence for a target class, this evidence should, on average, largely persist when inserted *universally* into any claim, thus increasing the probability of misclassification. This trigger extraction procedure depends only on the target model M and does not require access to training data, as well as being applicable to both encoder-based classifiers and zero-shot generative models. In the latter case, the labels correspond to verbalized output tokens (*"support"*, *"refute"*, *"not enough information"*), and trigger alignment is computed with respect to the scores obtained from the label-specific initial tokens, yielding an equivalent trigger ranking procedure.

In FACTFLIP, V is a vocabulary of 6,621 elements consisting of words and their antonyms extracted from WordNet. This design is motivated by three considerations: (i) excluding words, such as negations, that can alter the validity of a claim and invalidate its label (Morris et al., 2020; Zhou et al., 2024; Atanasova et al., 2020); (ii) including antonyms ensures coverage of both semantic polarities of the same concept, enabling a controlled analysis of sensitivity to meaning variations; and (iii) operating at the word level facilitates coherent integration into claims and avoids the interpretability issues associated with token-level perturbations (Ebrahimi et al., 2018; Shin et al., 2020).

3.2 Generation Stage

Once the trigger word p_{chosen} is identified, we use `gpt-4o-mini` as an *LLM as a Perturber* to integrate the trigger p into the original claim c_i, producing a perturbed claim c_i^p. A critical requirement of adversarial evaluation in fact-checking is that the ground-truth label must remain unchanged. If a perturbed claim alters the original entailment relation with the evidence, it becomes impossible to distinguish between a genuine model error and a correct prediction on a semantically different claim. For this reason, the goal of the generation stage is not merely to produce fluent adversarial claims, but to ensure that the semantic relation between the claim and the evidence, corresponding to the original label l_i, is preserved. This setting also reflects a realistic adversarial scenario, in which an attacker aims to maintain the intended meaning of a claim while inducing an incorrect model prediction. To enforce these constraints, we use the same `gpt-4o-mini` model as an *LLM as a Verifier* to check whether the perturbed claim c_i^p satisfies entailment relations with respect to c_i and e_i. We define the binary relation $\Rrightarrow$ to represent the claim verification relation, where $e_i \Rrightarrow c_i$ indicates support and $e_i \Rrightarrow \neg c_i$ indicates refutation, and we use $\models$ for logical entailment.

(i) If l_i = *Support*, the original claim must logically entail the perturbed claim. Since $e_i \Rrightarrow c_i$ and $c_i \models c_{i,\text{perturb}}$, it follows that $e_i \Rrightarrow c_{i,\text{perturb}}$.

(ii) If l_i = *Refute*, the perturbed claim must logi-

cally entail the original claim. Since $e_i \Rrightarrow \neg c_i$ and $c_{i,\text{perturb}} \vDash c_i$, by contraposition it follows that $e_i \Rrightarrow \neg c_{i,\text{perturb}}$.

(iii) If $l_i = \textit{NEI}$, the original and perturbed claims must logically entail each other, ensuring that no new supporting or refuting information is introduced with respect to the evidence.

Perturbed claims that violate these entailment constraints are discarded. While logical entailment is stricter than the standard *Support*, *Refute* and *NEI* relations used in claim verification, this choice guarantees that the resulting adversarial examples preserve the original ground-truth label, at the cost of an higher number of claims being discarded. Indeed, evaluations that do not enforce label preservation can inadvertently count semantically invalid perturbations as successful attacks, inflating attack success (Morris et al., 2020; Zhou et al., 2024; Atanasova et al., 2020). Starting from a dataset of claim verification tuples (c_i, e_i, l_i), this process yields a new dataset (c_i^p, e_i, l_i) composed of semantically valid adversarial claims that include the selected trigger p. Details about the prompts used and `gpt-4o-mini` parameters are reported in §F.

4 Experimental evaluation

The experimental evaluation addresses three key research questions. **RQ1** (Trigger Discrimination, Composition and Effectiveness) assesses the general effectiveness of FACTFLIP in discovering universal adversarial triggers, by analyzing whether FACTFLIP can distinguish between highly and weakly perturbative triggers, integrate multiple triggers to increase attack effectiveness, and be on par with gradient-based approaches (§4.1). **RQ2** (Plausibility vs. Attack Strength) investigates the trade-off between linguistic plausibility and attack success rate by evaluating performance under increasing constraints on the naturalness of perturbed claims (§4.2). **RQ3** (Model Diagnosis) analyzes how FACTFLIP can be used as a diagnostic tool to reveal sources of bias in fact-checking models, including biases stemming from pre-training and fine-tuning (§4.3). For the *LLM as a Verifier* and *Perturber*, we have qualitatively checked the reliability of the generation and verification step and found `gpt-4o-mini` to be consistently satisfactory, thus we use it as both the perturber and verifier.

Datasets. We consider six datasets commonly used in the literature to evaluate claim veracity: *AVERITEC* (Schlichtkrull et al., 2023), *SciFact* (Wadden et al., 2020), *HoVer* (Jiang et al., 2020), *FM2* (Eisenschlos et al., 2021), *PolitiHop* (Ostrowski et al., 2021), and *VitaminC* (Schuster et al., 2021). The *NEI* label appears only in *AVERITEC*, *SciFact*, and *VitaminC*. We drop dataset-specific labels such as *Conflicting Evidence* (*AVERITEC*) and *half-true* (*PolitiHop*) to keep the label space consistent across datasets. Moreover, we keep only instances the model predicts correctly, up to 500 per dataset to reduce API costs. Additional dataset statistics are provided in §F.

Models. We test FACTFLIP on two pre-trained models: `roberta-base` (Liu et al., 2019) and `Qwen-2.5-14B-Instruct` (Qwen, 2024). For each dataset, we fine-tune a separate `roberta-base` model, whereas `Qwen-2.5-14B-Instruct` is used in a zero-shot setting following the experimental setup of Lin et al. (2025), in which Qwen models achieved the best performance among open-source alternatives. Thus, we avoid using Chain-of-Thought prompting (Kojima et al., 2022), as it has been shown to provide limited gains in fact verification (Zhang and Gao, 2023; Pan et al., 2023). Detailed model performance results are in §B.4.

Metrics. We evaluate our method using the Attack Success Rate (ASR), which measures the effectiveness of an attack in flipping model predictions towards a target class t. ASR is computed over input samples that are initially correctly classified and whose ground-truth label is not t.

4.1 RQ1: Trigger Discrimination, Composition and Effectiveness

This section evaluates the central hypothesis underlying FACTFLIP: non-contextualized word representations carry enough class-specific evidence to identify perturbative triggers without supervision. Three complementary analyses are conducted to test this hypothesis: (i) **discrimination** tests whether triggers ranked higher by FACTFLIP are systematically more perturbative than triggers ranked lower; (ii) **compositionality** tests whether the perturbative effect of individual triggers compounds when they are combined; and (iii) **effectiveness** tests whether the resulting attacks remain competitive with supervised, gradient-based baselines that have direct access to labeled data.

Adversarial impact of ranked triggers. The discriminative power of FACTFLIP's ranking is assessed by quantifying the difference in attack success between the top- and bottom-ranked triggers.

	roberta-base				Qwen-2.5-14B			
Dataset	*Sup.*	*Ref.*	*NEI*	***Avg***	*Sup.*	*Ref.*	*NEI*	***Avg***
AVERITEC	+1.8	+2.9	0.0	+1.6	-0.1	+0.2	+1.1	+0.4
SciFact	+1.2	+12.1	+2.2	+5.2	0.0	-1.5	-0.1	-0.5
HoVer	+0.7	+58.2	–	+29.5	-1.3	+24.7	–	+11.7
FM2	-1.6	+4.9	–	+1.7	+0.4	+24.9	–	+12.7
PolitiHop	+1.2	+6.7	–	+4.0	-0.6	+14.1	–	+6.8
VitaminC	-0.5	+0.5	+15.1	+5.0	-2.8	+9.1	+4.5	+3.6
Average	+0.5	+14.2	+5.8	**+7.8**	-0.7	+11.9	+1.8	**+5.8**

Table 1: Difference in ASR between the top-5 high and low perturbing words identified by FACTFLIP.

	roberta-base				Qwen-2.5-14B			
Dataset	*Sup.*	*Ref.*	*NEI*	***Avg***	*Sup.*	*Ref.*	*NEI*	***Avg***
AVERITEC	+0.3	+66.7	+0.1	+22.3	+1.1	-2.4	+0.3	-0.3
SciFact	+6.8	+35.5	+14.2	+18.8	+0.0	0.0	+5.5	+1.8
HoVer	-1.0	+19.1	–	+9.1	+1.4	+12.4	–	+6.9
FM2	+1.1	+45.5	–	+23.3	+0.5	+5.9	–	+3.2
PolitiHop	-0.5	+43.3	–	+21.4	+1.4	-20.0	–	-9.3
VitaminC	-0.0	-0.8	+12.1	+3.8	-0.8	+14.2	+6.3	+6.6
Average	+1.1	+34.9	+8.8	**+16.5**	+0.6	+1.7	+4.0	**+1.5**
	↓21.9%	↓87.4%	↓29.5%	↓49.7%	↓32.0%	↓80.3%	↓18.2%	↓48.5%

Table 2: Compositional effect of high-perturbing triggers. Each cell reports the ΔASR between injecting three triggers and injecting one trigger. The bottom value on the **Average** row reports the relative change in the number of samples passing the verification step.

For each (model, dataset, class) configuration, the top-5 highest-ranked and top-5 lowest-ranked triggers identified by FACTFLIP are individually injected into the original claims, and the resulting ASRs are compared. Table 1 reports the average Δ ASR between high- and low-ranked triggers; complete per-dataset results are in §C.1. Top-ranked triggers yield substantially higher ASR than bottom-ranked ones across both models (average Δ ASR: $+7.81$ for RoBERTa, $+5.77$ for Qwen), with the strongest separation for *Refute* ($+14.23$ and $+11.92$, respectively) and a smaller but consistent separation for *NEI* ($+3.21$ and $+1.83$). For *Support*, the gap is small and occasionally negative, reflecting an intrinsic asymmetry of the claim verification task: support requires the joint presence of multiple aligned cues, while refutation and uncertainty can be triggered by individual lexical signals (Atanasova et al., 2020; Schuster et al., 2019), making the corresponding decision boundaries more sensitive to lexical perturbations. These results establish that the alignment between a word's non-contextualized embedding and a class-specific decision direction is a reliable predictor of its in-context perturbative effect, providing empirical support for the hypothesis underlying FACTFLIP.

Multi-trigger compositionality. A further consequence is that the perturbative effect of triggers should accumulate when multiple high-ranked triggers are combined within a single claim. To test this, three top-ranked triggers are jointly injected into each claim, and both the resulting ASR and the proportion of claims passing the entailment-based verification step are measured. Table 2 reports the average ΔASR between injecting three and one trigger; full results are reported in §C.2. Composition consistently amplifies attack effectiveness, confirming that perturbative signals identified by FACTFLIP combine constructively. This gain, however, comes at the cost of semantic plausibility: the proportion of verified claims drops by 49.1% on average compared to single-trigger perturbations, as denser perturbations are more likely to introduce semantic drift that the verifier rejects.

Effectiveness. Discriminative and compositional behaviour establish that FACTFLIP's ranking captures a meaningful perturbative signal. The remaining question is whether this signal is competitive with supervised, gradient-based methods that have access to labeled data. To answer it, FACTFLIP is compared against AutoPrompt (Shin et al., 2020), the gradient-based trigger discovery method underlying Atanasova et al. (2020), and against FACTFLIP-DS, a dataset-specific variant of FACTFLIP that retains the proposed extraction strategy but selects the top-5 triggers maximising ASR on a dev set among the top-50 candidates returned by FACTFLIP. Implementation details are in §B.1. After discovery, all baselines use our perturb-and-verify pipeline to integrate triggers into claims.

The three methods sit at different points along a supervision axis: FACTFLIP requires no labeled data, FACTFLIP-DS uses a development set for selection only, and AutoPrompt additionally performs gradient-based training. The comparison is therefore biased *against* FACTFLIP and, to a lesser extent, against FACTFLIP-DS. The Qwen setting compounds this asymmetry. Unlike the roberta-base setting, where each baseline operates against a separately fine-tuned model per dataset, the Qwen configuration uses a single shared

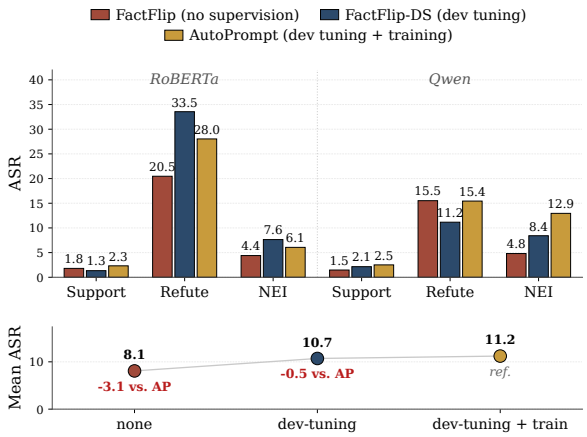

Figure 3: ASR comparison across attack methods. Top: class-wise ASR for FACTFLIP, FACTFLIP-DS, and AutoPrompt across RoBERTa and Qwen. Bottom: mean ASR by supervision level.

model across all datasets: FACTFLIP then operates on a fixed set of triggers and FACTFLIP-DS can only re-rank within the top-50 unsupervised candidates, whereas AutoPrompt remains free to re-optimise per dataset. The results are shown in Figure 3. Despite this asymmetry, no method consistently dominates the others: averaging across classes, FACTFLIP-DS outperforms AutoPrompt on `RoBERTa` by 1.77 ASR points, while AutoPrompt outperforms FACTFLIP-DS on `Qwen` by 2.89 points. Aggregating over all six (model, class) cells, the overall ASR gap between FACTFLIP-DS and AutoPrompt narrows to -0.51 points, and FACTFLIP itself lags AutoPrompt by only -3.14 points despite using no labeled data.

These results substantiate the central claim of this work. The ranking produced by FACTFLIP is not only internally meaningful (discriminative and compositional) but also externally competitive. The implication is twofold. From a methodological standpoint, costly supervised search procedures are not strictly required to expose the lexical vulnerabilities of claim verification models, simplifying the deployment of robustness analyses in settings where labeled data are unavailable. From a diagnostic standpoint, the non-contextualized alignment between vocabulary and decision direction emerges as a model-intrinsic property: a property that, as shown in §4.3, can be probed to reveal sources of bias that lie beyond dataset-specific artefacts.

4.2 RQ2: Plausibility vs. Attack Strength

Realistic attacks must preserve claim meaning, but enforcing plausibility restricts the trigger space

	FF-RAW		FF-SIM	
Model	Δ_{ASR}	$\Delta_{BERTScore}$	Δ_{ASR}	$\Delta_{BERTScore}$
`roberta-base`	+2.97	-3.33	+0.96	+2.50
`Qwen2.5-14B`	+0.20	-3.83	-1.68	+2.16

Table 3: Impact of `FF-RAW` and `FF-SIM` trigger injection on ASR and semantic similarity ($\Delta_{BERTScore}$), averaged across datasets. Positive Δ_{ASR} indicates stronger attacks than FACTFLIP, while negative $\Delta_{BERTScore}$ reflects reduced semantic plausibility.

and can blunt effectiveness. In Table 3, we isolate this trade-off through two ablations: raw injection (`FF-RAW`), which disables claim rewriting as an upper bound on attack strength, and similarity-constrained selection (`FF-SIM`), which restricts triggers to those semantically aligned with the claim. The full analysis is reported in §D.1. The trade-off is model-dependent rather than universal. For RoBERTa, `FF-RAW` raises ASR by $+2.97$ on average at a clear cost in semantic similarity (-3.33 BERTScore), and `FF-SIM` recovers plausibility ($+2.50$ BERTScore) while still gaining slightly in effectiveness ($+0.96$ ASR). Fine-tuned RoBERTa absorbs lexical correlations and remains vulnerable even when perturbations are forced to preserve meaning, suggesting that robustness cannot be reduced to filtering implausible inputs. For Qwen, the same interventions yield marginal ASR changes ($+0.20$), at a comparable plausibility cost (-1.68), indicating that its predictions rely on deeper claim–evidence semantic alignment. FACTFLIP therefore provides not only a method for discovering perturbative triggers but also a knob to probe *which* aspect of a model's representation, surface or semantic, drives its predictions.

4.3 RQ3: Model Diagnosis

The preceding subsections established FACTFLIP as an effective and controllable method for discovering universal adversarial triggers. The remaining question is whether the resulting triggers carry diagnostic value, that is, whether the structure of their adversarial impact reveals systematic properties of the target model. Two complementary analyses are conducted: a probe for sensitivity to socially salient lexical cues, and an attribution analysis that traces the origin of adversarial sensitivity to either pre-training, fine-tuning, or their interaction.

Sensitivity to lexical cues from demographic groups. FACTFLIP can be used as a diagnostic

	Mean ASR				ΔASR (pert.−unpert.)			
Bias	*Sup.*	*Ref.*	*NEI*	*Avg*	*Sup.*	*Ref.*	*NEI*	*Avg*
Religion	1.7	24.9	11.1	12.6	+0.8	+6.3	+7.2	+4.8
Politics	1.0	23.2	8.4	10.9	+0.1	+3.5	+3.7	+2.5
Gender	1.8	21.5	5.7	9.7	+1.2	+0.3	+3.1	+1.6
Race	1.1	20.6	5.0	8.9	0.0	+0.8	+1.4	+0.7
Sexual	1.7	17.0	1.2	6.6	+0.7	+6.2	-0.3	+2.2
Average	1.5	21.4	6.3	**9.7**	+0.6	+3.4	+3.0	**+2.4**

Table 4: Bias on RoBERTa, averaged across datasets. **Mean ASR** (left) measures *robustness*, i.e. how strongly each bias category flips predictions; **ΔASR** (right) measures *discrimination*, i.e. the gap between top- and bottom-ranked triggers within each category.

	Support	*Refute*	*NEI*	*Avg*
AVERITEC	-3.70	-9.20	-1.59	-4.83
SciFact	+7.43	+1.62	+1.51	+3.52
HoVer	-37.44	-3.21	–	-20.33
FM2	+23.64	-2.11	–	+10.77
PolitiHop	+26.08	+18.84	–	+22.46
VitaminC	-0.02	-1.55	+8.92	+2.45
Average	+2.66	+0.73	+2.95	+2.34

Table 5: Mean difference in occurrence frequency between the top and bottom 5 perturbing triggers.

tool to probe two complementary properties of a target model's bias profile: *robustness*, i.e. whether terms associated with specific demographic groups systematically flip the model's predictions (representational harm Blodgett et al., 2020); and *discrimination*, i.e. whether FACTFLIP correctly separates strongly perturbative from weakly perturbative terms within the same bias category. The analyses are conducted on RoBERTa with claim rewriting disabled, using dedicated vocabularies for five bias dimensions: *gender*, *religion*, *sexual orientation*, *politics*, and *race* (vocabularies in §E.1). The resulting mean ASRs per bias group and ΔASR between top- and bottom-ranked triggers are reported in Table 4. Religion- and politics-related terms induce the strongest effects (mean ASR 12.6 and 10.9, respectively), followed by gender and race (9.7 and 8.9), with sexual-orientation cues producing the weakest effects (6.6). The flip rates are substantial across all dimensions, indicating that socially salient terms reliably function as universal triggers. Moreover, within each dimension, the ranking itself remains informative, as the top-ranked terms identified by FACTFLIP systematically outperform the bottom-ranked ones across all five categories. Together, the two findings show that a claim verification model's bias profile can be surfaced without any task-specific supervision, using only the non-contextual trigger search underlying FACTFLIP.

Origin of adversarial sensitivity. A natural concern is that the discovered triggers reflect spurious correlations specific to the fine-tuning data rather than intrinsic model vulnerabilities. To test this directly, the occurrence frequency of high- and low-ranked FACTFLIP triggers is compared in the training claims of each target class (Table 5). If the strongest triggers were dataset artefacts, they should appear systematically more often in target-class training examples than the weakest ones; instead, the association is weak and inconsistent. Highly perturbing words are only marginally more frequent on average (mean difference $+2.34$), and no systematic pattern emerges even in settings where ASR is high (e.g., *Refute* on HoVer and FM2). Adversarial sensitivity therefore cannot be reduced to dataset-specific lexical artefacts.

To further isolate the contribution of pre-training, the perturbing triggers discovered for a standard fine-tuned RoBERTa are compared against those obtained after re-initialising the pre-training weights and fine-tuning from scratch. As shown in §E.2, the lexical overlap between the two sets is negligible for both high- and low-impact triggers, indicating that neither pre-training alone nor fine-tuning alone determines which words function as universal triggers. The vulnerability emerges, instead, from the *interaction* between the lexical statistics absorbed during pre-training and the decision boundary shaped by fine-tuning. Thus, biased behaviour cannot be resolved by isolating "culprit" fine-tuning examples. By surfacing these biases directly through their adversarial triggers and integrating them into novel claims, FACTFLIP offers a natural starting point for future interventions aimed at improving bias robustness (Schuster et al., 2019).

5 Conclusion

We introduced FACTFLIP, a framework for analyzing the robustness of fact-checking models via universal adversarial triggers. FACTFLIP relies on a lightweight, model-only analysis with an LLM-based perturb-and-verify pipeline. Across multiple datasets and models, FACTFLIP effectively exposes systematic vulnerabilities and different sources of bias in claim verification models.

6 Limitations

Our trigger extraction assumes access to model internals (e.g., classifier-head parameters) or, in generative zero-shot settings, access to label-token logits. As a result, FACTFLIP is not applicable to black-box APIs.

Because triggers are ranked using non-contextual alignment, FACTFLIP may under-rank triggers whose adversarial effect is highly context-dependent, which can reduce ASR in some cases. This behavior is expected: FACTFLIP targets universal triggers intended to expose general vulnerabilities rather than instance-specific weaknesses. However, incorporating more contextualized representations (e.g. with different word senses) to improve ASR is a promising direction, which we leave to future work.

Finally, the generation stage depends on an external LLM (perturber/verifier), which incurs additional cost and latency and may introduce variability across prompts and model versions.

7 Ethical considerations

Our framework could be exploited by malicious actors to craft well-engineered claims aimed at fooling existing automated claim verification systems. However, our goal is strictly diagnostic: we introduce FACTFLIP to systematically expose model weaknesses and failure modes, enabling future work to develop more robust and reliable claim verification models. This work is not intended for, nor do we endorse, adversarial misuse.

The data we generate with the perturb-and-verify pipeline exclusively integrates the found triggers with the original claims of the datasets we tested. These datasets may contain texts that refer to individual people. Although the integration of the triggers may inadvertently alter the meaning of the original texts, we do not perform any steps to protect/anonymize the data we generate, as (i) the introduced perturbation is minimal by definition; (ii) it would be impossible to guarantee reproducibility in real world scenarios, as personally identifiable information is crucial for the claim verification task; and (iii) it would be difficult to evaluate FACTFLIP and compare it with other approaches, as the datasets we used have been published with personally identifiable information.

8 Use of AI assistants

When writing this paper, we used ChatGPT to improve the flow of writing and the vocabulary of the initial drafts we manually wrote. Each suggestion has been manually validated by the authors.

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A Appendix overview

This appendix provides extended experimental results and methodological details complementing

the analyses presented in the main paper. For clarity and consistency, the appendix is structured to mirror the four research questions (RQ1–RQ4) investigated in the experimental section, with additional preliminary material covering experimental setup and baselines.

Appendix B details the experimental foundations shared across all research questions. It describes the adaptation of AutoPrompt to the fact verification setting and reports the baseline performance of the evaluated fact-checking models on the original test sets.

Appendix C extends the analysis of RQ1 (Trigger discrimination, composition and effectiveness) by reporting additional results related to the adversarial impact of ranked triggers, their compositionality, and further results for AutoPrompt under increased gradient accumulation.

Appendix D supports RQ2 (Plausibility vs. Attack Strength) with a detailed study of the LLM-as-a-verifier component. It quantifies the proportion of perturbed claims filtered out by the semantic consistency check and analyzes its differential impact across target classes and models.

Appendix E complements RQ3 (Model Diagnosis) with extended case studies on demographic bias and adversarial sensitivity. It includes detailed breakdowns of bias-related triggers across datasets and classes, examples of bias vocabularies, and an analysis of lexical overlap aimed at disentangling the effects of pre-training and fine-tuning.

Appendix F illustrates the prompts used for the perturb-and-verify pipeline, as well as the prompt used to perform claim verification with `Qwen2.5-14B-Instruct`. Moreover, this section provides additional statistics on the composition of the generated datasets.

B Experimental setup and baselines

B.1 AutoPrompt adaptation

This section describes the adaptation of AutoPrompt (Shin et al., 2020) to the fact verification setting considered in this work. AutoPrompt automatically constructs prompts (Wallace et al., 2019) through a Hotflip-style gradient search (Ebrahimi et al., 2018). The method introduces n universal trigger tokens initialized as [MASK] at the beginning of the input, which are iteratively updated to maximize the likelihood of a target class t, that is, the class we intend to perturb.

For each input x_i^{train} in a dataset D^{train}, where x_i^{train} is the prompt containing c_i^{train} and e_i^{train} and c_i^{train} is initialized with n [MASK] trigger tokens, AutoPrompt computes the gradient with respect to the j-th trigger token w_{trig}^j and identifies a set of candidate tokens $w \in V_{cand}$ from the model vocabulary that maximize the probability of t:

$$V_{cand} = \underset{w \in V}{\text{top-}k} \, [w_{emb}^T \nabla \log p(t \mid x_i^{train})] \quad (1)$$

where w_{emb}^T denotes the embedding of token w, and the gradient is taken with respect to w_{trig}^j. Then, each token $w \in V_{cand}$ replaces the j-th trigger token inside each $x_i^{dev} \in D^{dev}$. The perturbation performance of each candidate w is then evaluated on D^{dev}, and the best-performing tokens are retained for the next iteration

$$V_{trig} = \underset{w \in V_{cand}}{\text{top-}k} \, [\text{PScore}(w; D^{dev})] \quad (2)$$

with PScore being the *ASR* of trigger token w on D^{dev}.

Unlike AutoPrompt, our objective is to identify words, possibly composed of multiple tokens. Let $\mathcal{W}$ be the set of words, where each word $w \in \mathcal{W}$ is composed of $|w|$ tokens with embeddings emb_{w_i}. Since AutoPrompt's search requires a fixed number of trigger tokens, we divide the vocabulary into m bins B_m according to token length. For each bin B_m, we define the word-level gradient score as

$$s(w) = \frac{1}{|w|} \sum_{i=1}^{|w|} emb_{w_i}^T \nabla \log p(t|x)$$

and select the top-k candidate words as

$$V_{cand}^{(m)} = \text{top-}k_{w \in B_m} [s(w)]$$

retaining the candidate words with the strongest perturbation on D^{dev} for the next iteration (Equation 2). Finally, we unify all the candidate words across the B_m bins $V_{cand} = \bigcup_{m=1}^{M} V_{cand}^{(m)}$ and compute V_{trig} with Equation 2.

B.2 RoBERTa fine-tuning parameters

The `roberta-base` models have been fine-tuned for 30 epochs with learning rate equal to $1e-5$, maximum sentence length equal to 512, weight decay equal to $1e-2$, and class weight to handle label imbalance. We select and report the model checkpoint corresponding to the epoch achieving the highest F1 score on the validation set.

	Dataset	Accuracy	Precision	Recall	F1 Score
roberta-base	AVERITEC	84.42	86.36	85.04	85.66
	SciFact	75.00	69.66	69.73	69.50
	HoVer	79.68	80.02	79.68	79.62
	FM2	81.45	81.51	81.42	81.43
	PolitiHop	87.13	70.53	72.19	71.31
	VitaminC	89.26	85.78	86.15	85.92
Qwen2.5-14B	AVERITEC	62.99	69.74	75.08	60.73
	SciFact	82.33	85.41	80.96	81.57
	HoVer	73.60	73.61	73.60	73.60
	FM2	87.20	87.57	87.37	87.19
	PolitiHop	97.66	93.14	96.62	94.78
	VitaminC	77.80	71.10	70.40	70.66

Table 6: Performance of tested fact checker models on the original test sets. The `roberta-base` model is fine-tuned on each dataset, while `Qwen2.5-14B-Instruct` is tested in zero-shot.

B.3 `gpt-4o-mini` parameters

Every `gpt-4o-mini` generation uses temperature equal to 0 and top_p equal to 1. We set a number of retries equal to 50 to handle OpenAI API failures.

B.4 Fact-checking accuracy of the tested models

We report the baseline performance of the evaluated fact-checking models on the original, unperturbed test sets. This analysis serves two purposes: (i) to verify that all models achieve competitive performance in the clean setting, and (ii) to contextualize the impact of adversarial perturbations reported in subsequent sections. Baseline accuracy varies across datasets and target classes, reflecting differences in dataset size, label distribution, and evidence complexity. The results, reported in Table 6, confirm that both *RoBERTa* and *Qwen* exhibit strong verification capabilities prior to adversarial intervention, ensuring that observed attack effects cannot be attributed to weak baseline performance.

B.5 Computational infrastructure

We report the model sizes and the computational infrastructure used to run the experiments. The `roberta-base` model contains 0.1 billion parameters, while `Qwen2.5-14B-Instruct` contains 14 billion parameters. The experiments with `roberta-base` have been run on a NVIDIA L40S with 46GB. For `Qwen2.5-14B-Instruct`, we loaded the model in half precision (float16) and ran it on one NVIDIA Ampere A100 with 64GB.

C RQ1 - Trigger discrimination, composition, and effectiveness

C.1 Adversarial impact of ranked triggers

This section provides detailed results supporting the trigger ranking experiment discussed in the main paper. In addition to reporting ASR differences between highly perturbing and minimally perturbing triggers, in Table 7 we include absolute ASR values for each dataset and class. The results confirm that FACTFLIP consistently assigns higher adversarial impact to triggers that induce larger prediction shifts, while minimally perturbing words yield substantially weaker effects. This separation holds across datasets and target labels, supporting the reliability of the learned trigger ranking.

C.2 Multi-trigger compositionality

This section provides a detailed analysis of the compositional effects arising from simultaneously injecting multiple triggers into the same claim. In particular, we study whether the adversarial impact of individual triggers compounds when combined, and how this interaction affects both attack success and semantic validity.

Table 9 reports ASR obtained by injecting, for each claim, either the three most perturbing triggers or the three least perturbing ones according to FACTFLIP 's ranking. Across both models, combining high-impact triggers generally leads to a substantial amplification of adversarial effectiveness. This effect is especially pronounced for the *Refute* class, where RoBERTa exhibits an average ASR increase of $+37.60$ points when using perturbing combinations compared to unperturbing ones, while Qwen shows a corresponding gain of $+10.33$. For *NEI*, the gains are more moderate but consistent across models ($+6.93$ for RoBERTa and $+4.97$ for Qwen), whereas *Support* remains largely stable, indicating lower sensitivity to trigger composition.

At the same time, multi-trigger injection has a marked impact on claim validity. As shown in Table 8, injecting three triggers instead of a single one leads to a sharp reduction in the number of claims passing the verification step enforced by the LLM-as-a-verifier. Averaged across datasets and labels, the proportion of verified claims drops by $46.29\% \pm 20.88$ for RoBERTa and by $43.49\% \pm 22.51$ for Qwen. This reduction is particularly severe for the *Refute* class, where over 80% of claims are filtered out on average, indicating that composi-

		Support			*Refute*			*NEI*			*Avg* Δ
	Dataset	pert.	unpert.	Δ	pert.	unpert.	Δ	pert.	unpert.	Δ	
roberta-base	AVERITEC	1.81	0	+1.81	8.33	5.41	+2.92	0.16	0.00	+0.16	+1.63
	SciFact	1.23	0.00	+1.23	14.52	2.38	+12.14	3.95	0.00	+3.95	+5.77
	HoVer	1.02	0.34	+0.68	80.88	22.66	+58.22	–	–	–	+29.45
	FM2	5.26	6.83	-1.57	11.68	6.78	+4.90	–	–	–	+1.67
	PolitiHop	1.24	0	+1.24	6.67	0	+6.67	–	–	–	+3.96
	VitaminC	0.22	0.75	-0.53	0.75	0.23	+0.52	9.08	3.56	+5.52	+1.84
	Average	1.80	1.32	+0.48	20.47	6.24	+14.23	4.40	1.19	+3.21	**+7.39**
Qwen-2.5-14B	AVERITEC	1.14	1.21	-0.07	2.38	2.20	+0.18	5.24	4.18	+1.06	+0.39
	SciFact	1.65	1.65	0.00	0.00	1.52	-1.52	1.28	1.34	-0.06	-0.53
	HoVer	1.40	2.71	-1.31	28.8	4.12	+24.68	–	–	–	+11.69
	FM2	0.80	0.40	+0.40	29.41	4.47	+24.94	–	–	–	+12.67
	PolitiHop	0.49	1.10	-0.61	20.00	5.88	+14.12	–	–	–	+6.76
	VitaminC	3.31	6.09	-2.78	12.5	3.39	+9.11	7.91	3.42	+4.49	+3.61
	Average	1.47	2.19	-0.72	15.52	3.60	+11.92	4.81	2.98	+1.83	**+5.77**

Table 7: Effectiveness of FACTFLIP in distinguishing highly perturbing and less perturbing words across different models, datasets, and target class. The delta column reports the difference in ASR between perturbing and unperturbing words, with positive values indicating high discriminative power of the ranking.

Label	**RoBERTa**	**Qwen**
Support	21.91 ± 26.43	32.00 ± 17.28
Refute	87.44 ± 9.45	80.33 ± 28.73
NEI	29.53 ± 26.76	18.15 ± 21.52
Average	46.29 ± 20.88	43.49 ± 22.51

Table 8: Percentage reduction in the number of claims passing the verification step when injecting three triggers instead of a single trigger. Results are reported as mean ± standard deviation across datasets for each label.

tional perturbations frequently introduce semantic distortions that violate entailment consistency with the evidence.

Taken together, these results highlight a clear trade-off induced by trigger composition. While combining multiple high-impact triggers significantly strengthens adversarial attacks, it also increases semantic drift, causing a large fraction of perturbed claims to be rejected by the verifier. Importantly, the remaining claims that do pass verification still achieve non-negligible ASR, showing that compositional attacks can remain effective even under strict semantic constraints. Overall, this analysis further supports the reliability of FACTFLIP 's trigger ranking and illustrates how adversarial strength and linguistic plausibility interact under multi-trigger perturbations.

C.3 Effectiveness

This section reports more in detail the comparison between FACTFLIP and AutoPrompt described in §4.1. The complete per-dataset results are reported in Table 10.

Additionally, we report additional AutoPrompt results obtained by increasing the number of gradient accumulation steps during trigger optimization (from 10 to 500). As shown in Table 11, increasing gradient accumulation does not consistently improve attack success rates. In several cases, performance deteriorates, particularly on datasets with limited lexical diversity. This behavior suggests that prolonged optimization encourages overfitting to dataset-specific artifacts rather than discovering robust universal triggers. For this reason, these configurations are excluded from the main experimental comparison and reported here solely for completeness.

D RQ2 - Plausibility vs. Attack strength

D.1 Similarity-based trigger selection.

This section extends the analysis presented in the main paper by providing a more fine-grained, class-wise view of attack success rates across datasets and trigger selection strategies. While the main paper focuses on aggregated trends, these extended results make explicit how such trends emerge from systematic shifts in specific decision classes. The results of this evaluation are reported in Table 12

	Dataset	*Support*			*Refute*			*NEI*			*Avg* Δ
		pert.	unpert.	Δ	pert.	unpert.	Δ	pert.	unpert.	Δ	
roberta-base	AVERITEC	2.09	0.23	+1.86	75	0.00	+75.00	0.23	0.00	+0.23	+25.70
	SciFact	8.00	2.60	+5.40	50.00	0.00	+50.00	18.18	5.00	+13.18	+22.86
	HoVer	0.00	0.00	0.00	100.00	87.18	+12.82	–	–	–	+6.41
	FM2	6.40	8.57	-2.17	57.14	19.35	+37.79	–	–	–	+17.81
	PolitiHop	0.72	0.00	+0.72	50.00	0.00	+50.00	–	–	–	+25.36
	VitaminC	0.20	1.51	-1.31	0.00	0.00	0.00	21.21	13.83	+7.38	+2.02
	Average	2.90	2.15	+0.75	55.36	17.76	+37.60	13.21	6.28	+6.93	**+16.69**
Qwen2.5-14B	AVERITEC	2.26	0.63	+1.63	0.00	6.25	-6.25	5.56	4.33	+1.23	-1.13
	SciFact	1.67	4.26	-2.59	0.00	1.75	-1.75	6.76	2.76	+4.00	-0.11
	HoVer	2.75	4.32	-1.57	41.18	8.33	+32.85	–	–	–	+15.64
	FM2	1.26	1.99	-0.73	35.29	8.54	+26.75	–	–	–	+13.01
	PolitiHop	1.84	2.68	-0.84	0.00	8.33	-8.33	–	–	–	-4.59
	VitaminC	2.52	7.53	-5.01	26.67	7.96	+18.71	14.18	4.40	+9.78	+7.83
	Average	2.05	3.57	-1.52	17.19	6.86	+10.33	8.83	3.86	+4.97	**+5.11**

Table 9: ASR for the set of three most perturbing (*pert.*) and three least perturbing words (*unpert.*) across models, datasets and target classes.

	Dataset	*Support*				*Refute*				*NEI*				*Avg* Δ
		FF	FF-DS	Auto-Prompt	Δ	FF	FF-DS	Auto-Prompt	Δ	FF	FF-DS	Auto-Prompt	Δ	
roberta-base	AVERITEC	1.81	0.41	1.44	-1.03	8.33	44.00	5.00	+39.00	0.16	1.43	0.72	+0.71	+12.89
	SciFact	1.23	0.00	2.67	-2.67	14.52	28.57	22.58	+5.99	3.95	5.66	2.17	+3.49	+2.27
	HoVer	1.02	1.55	1.11	+0.44	80.88	88.74	93.58	-4.84	–	–	–	–	-2.20
	FM2	5.26	4.47	8.72	-4.25	11.68	19.05	28.26	-9.21	–	–	–	–	-6.73
	PolitiHop	1.24	0.93	0.00	+0.93	6.67	16.67	0.00	+16.67	–	–	–	–	+8.80
	VitaminC	0.22	0.66	0.00	+0.66	0.75	4.24	18.73	-14.49	9.08	15.84	15.28	+0.56	-4.42
	Average	1.80	1.34	2.32	-0.98	20.47	33.55	28.03	+5.52	4.40	7.64	6.06	+1.58	**+1.77**
	Std. Dev.	1.62	1.48	3.00	1.91	27.35	27.49	30.9	18.12	3.66	6.05	6.55	1.35	7.07
Qwen-2.5-14B	AVERITEC	1.14	0.56	0.96	-0.40	2.38	6.06	1.43	+4.63	5.24	9.29	8.44	+0.85	+1.69
	SciFact	1.65	2.38	0.00	+2.38	0.00	0.00	2.17	-2.17	1.28	8.05	13.92	-5.87	-1.89
	HoVer	1.40	3.41	6.01	-2.60	28.80	11.71	19.05	-7.34	–	–	–	–	-4.97
	FM2	0.80	1.20	0.54	+0.66	29.41	34.88	41.67	-6.79	–	–	–	–	-3.07
	PolitiHop	0.49	1.25	0.31	+0.94	20.00	14.29	20.00	-5.71	–	–	–	–	-2.39
	VitaminC	3.31	4.07	7.22	-3.15	12.50	0.00	8.33	-8.33	7.91	7.92	16.50	-8.58	-6.69
	Average	1.47	2.15	2.51	-0.36	15.52	11.16	15.44	-4.28	4.81	8.42	12.95	-4.53	**-2.89**
	Std. Dev.	0.91	1.26	2.94	1.96	11.63	11.89	13.81	4.43	2.72	0.62	3.36	3.96	2.61

Table 10: ASR comparison between FACTFLIP (FF) and competitors across models and datasets. Values are reported per target class (*Support*, *Refute*, *NEI*), corresponding to the attack target label. Results are averaged over the top-5 most perturbing words. The Δ column reports the ASR difference between FF-DS and AutoPrompt, the last column shows the average ASR change across classes, and "–" indicates the absence of the *NEI* class.

and Table 13. Across both models, the detailed breakdown confirms that most ASR gains, both for FF-RAW and FF-SIM, are driven primarily by changes in the *Refute* class, while effects on *Support* and *NEI* are generally smaller and less consistent. This pattern is particularly pronounced for *RoBERTa*, reinforcing the interpretation that refutation decisions are more susceptible to lexical and stylistic cues introduced by adversarial triggers. Moreover, the class-wise results clarify the trade-offs observed in the main paper. For FF-RAW, large ASR increases are often associated with substantial semantic degradation, whereas FF-SIM achieves a more balanced profile, combining moderate attack effectiveness with improved semantic preservation. The consistency between aggregate metrics

		Support			*Refute*			*NEI*			*Avg Δ*
	Dataset	FactFlip	Autoprompt	Δ	FactFlip	Autoprompt	Δ	FactFlip	Autoprompt	Δ	
roberta-base	AVERITEC	1.81	1.45	+0.36	8.33	9.09	-0.76	0.16	0.51	-0.35	-0.25
	SciFact	1.23	3.28	-2.05	14.52	22.45	-7.93	3.95	6.12	-2.17	-4.05
	HoVer	1.02	0.59	+0.43	80.88	81.08	-0.20	–	–	–	+0.12
	FM2	5.26	8.36	-3.10	11.68	12.5	-0.82	–	–	–	-1.96
	PolitiHop	1.24	0	+1.24	6.67	0	+6.67	–	–	–	+3.96
	VitaminC	0.22	0	+0.22	0.75	7.14	-6.39	9.08	14.91	-5.83	-4.00
	Average	1.80	2.28	-0.48	20.47	22.04	-1.57	4.40	7.18	-2.78	**-1.03**
Qwen-2.5-14B	AVERITEC	1.14	0.10	+1.04	2.38	7.41	-5.03	5.24	10.50	-5.26	-3.08
	SciFact	1.65	0.00	+1.65	0.00	5.26	-5.26	1.28	17.86	-16.58	-6.73
	HoVer	1.40	2.40	-1.00	28.8	21.62	+7.18	–	–	–	+3.09
	FM2	0.80	1.04	-0.24	29.41	16.67	+12.74	–	–	–	+6.25
	PolitiHop	0.49	0.57	-0.08	20.00	10.00	+10.00	–	–	–	+4.96
	VitaminC	3.31	7.21	-3.9	12.5	7.15	+5.35	7.91	13.19	-5.28	-1.28
	Average	1.47	1.89	-0.42	15.52	11.35	+4.17	4.81	13.85	-9.04	**+0.54**

Table 11: Attack Success Rate comparison between FACTFLIP and AutoPrompt with 500 accumulation steps.

and class-level behavior supports the robustness of the main conclusions: adversarial effectiveness is strongly shaped by the interaction between trigger plausibility, model sensitivity to surface patterns, and the biases induced by fine-tuning.

D.2 Impact of the LLM-as-a-verifier.

This experiment evaluates the selectivity of the LLM-as-a-verifier by measuring how many perturbed claims are discarded by the semantic filtering step. After trigger injection and claim rewriting, the verifier retains only those claims that preserve the original entailment relation with the evidence, allowing us to quantify the extent to which unconstrained perturbations introduce semantic drift.

The statistics reported in Table 14 show that the verifier filters out a large fraction of generated claims. On average, *RoBERTa* rejects 55.8% of Support claims, 86.4% of Refute claims, and 71.6% of NEI claims, while *Qwen* discards 58.4%, 92.8%, and 70.3%, respectively. The particularly high rejection rates for the *Refute* class suggest that, although effective attacks exist, most refutation-oriented perturbations induce semantic inconsistencies that are detected by the entailment check. Overall, these results indicate that the LLM-as-a-verifier acts as a strong semantic regularizer, pruning perturbations that violate entailment consistency and substantially constraining the space of admissible adversarial claims.

E RQ3 - Model diagnosis

E.1 Assessing biases affecting minority demographic groups

This section provides a detailed breakdown of the bias analysis introduced in the main paper, reporting fine-grained results across datasets, target classes, and bias categories. The goal of this analysis is to better characterize how socially salient lexical cues influence model predictions when injected into claims in isolation.

For each bias dimension, *gender*, *religion*, *sexual orientation*, *politics*, and *race*, we construct a dedicated vocabulary of 50 terms, covering both majority and minority identifiers as well as neutral and less frequent descriptors. Table 15 reports a representative subset of 10 words per category. These vocabularies are used to systematically probe lexical sensitivity by inserting a single bias-related term at the beginning of each claim, followed by a punctuation mark, without enabling claim rewriting. This setup ensures that observed effects are attributable to the lexical cue alone, rather than to semantic reformulation.

Table 16 reports the attack success rate obtained when injecting the top-ranked perturbing terms (`pert.`) and the least perturbing ones (`unpert.`), as identified by FACTFLIP, across all datasets and target classes. Results reveal substantial variability across datasets and labels. In `HoVer` and `FM2`, which contain a large proportion of *Refute* instances, several bias categories lead to pronounced increases in ASR when perturbing words are injected. In

	Dataset	FF-RAW				FF-SIM			
		Support	*Refute*	*NEI*	Δ	*Support*	*Refute*	*NEI*	Δ
roberta-base	AVERITEC	$1.2^{-0.6}$	$25.9^{+17.6}$	$0.6^{+0.4}$	+5.80	$0.6^{-1.2}$	$10.6^{+2.3}$	$0.2^{0.0}$	+0.37
	SciFact	$0.3^{-0.9}$	$3.8^{-10.7}$	$6.0^{+2.0}$	-3.20	$1.0^{-0.2}$	$5.8^{-8.7}$	$0.0^{-4.0}$	-4.30
	HoVer	$3.4^{+2.4}$	$80.2^{-0.7}$	–	+0.85	$0.3^{-0.7}$	$85.0^{+4.1}$	–	+1.70
	FM2	$9.8^{+4.5}$	$12.5^{+0.8}$	–	+2.65	$4.7^{-0.6}$	$28.1^{+16.4}$	–	+7.90
	PolitiHop	$0.0^{-1.2}$	$29.1^{+22.4}$	–	+10.60	$0.7^{-0.5}$	$6.3^{-0.4}$	–	-0.45
	VitaminC	$0.8^{+0.6}$	$3.6^{+2.8}$	$9.0^{-0.1}$	+1.10	$0.8^{+0.6}$	$2.4^{+1.6}$	$8.6^{-0.5}$	+0.57
	Average	$2.6^{+0.8}$	$25.9^{+5.4}$	$5.2^{+0.8}$	**+2.97**	$1.4^{-0.4}$	$23.0^{+2.5}$	$2.9^{-1.5}$	**+0.96**
Qwen2.5-14B	AVERITEC	$0.4^{-0.7}$	$4.2^{+1.8}$	$5.4^{+0.2}$	+0.43	$0.5^{-0.6}$	$5.8^{+3.4}$	$6.1^{+0.9}$	+1.23
	SciFact	$0.0^{-1.7}$	$1.6^{+1.6}$	$8.8^{+7.5}$	+2.47	$0.0^{-1.7}$	$0.0^{0.0}$	$4.6^{+3.3}$	+0.53
	HoVer	$1.6^{+0.2}$	$27.6^{-1.2}$	–	-0.50	$2.4^{+1.0}$	$22.9^{-5.9}$	–	-2.45
	FM2	$0.4^{-0.4}$	$35.4^{+6.0}$	–	+2.80	$0.8^{0.0}$	$26.9^{-2.5}$	–	-1.25
	PolitiHop	$0.4^{-0.1}$	$18.0^{-2.0}$	–	-1.05	$0.3^{-0.2}$	$5.9^{-14.1}$	–	-7.15
	VitaminC	$2.6^{-0.7}$	$7.4^{-5.1}$	$4.8^{-3.1}$	-2.97	$6.6^{+3.3}$	$10.0^{-2.5}$	$4.1^{-3.8}$	-1.00
	Average	$0.9^{-0.6}$	$15.7^{+0.2}$	$6.3^{+1.5}$	**+0.20**	$1.8^{+0.3}$	$11.9^{-3.6}$	$4.9^{+0.1}$	**-1.68**

Table 12: Attack success rates under FF-RAW and FF-SIM settings. Superscripts indicate absolute changes from FACTFLIP, and Δ denotes the average variation across labels.

	Dataset	FF-RAW				FF				FF-SIM			
		Support	*Refute*	*NEI*	*Avg*	*Support*	*Refute*	*NEI*	*Avg*	*Support*	*Refute*	*NEI*	*Avg*
roberta-base	AVERITEC	83.36	83.24	84.58	83.73	88.76	89.50	91.21	89.82	90.80	88.75	91.73	90.43
	SciFact	80.42	80.95	80.43	80.60	85.32	85.90	83.93	85.05	91.22	90.11	89.17	90.17
	HoVer	83.47	85.29	–	84.38	88.33	92.22	–	90.28	90.31	92.51	–	91.41
	FM2	82.94	80.23	–	81.59	87.59	86.38	–	86.99	88.70	87.43	–	88.07
	PolitiHop	83.06	84.53	–	83.80	82.10	84.51	–	83.31	86.88	86.48	–	86.68
	VitaminC	82.71	82.09	77.69	80.83	81.97	81.27	75.06	79.43	83.47	82.74	83.08	83.10
	Average	**82.66**	**82.72**	**80.90**	**82.49**	**85.68**	**86.63**	**83.40**	**85.81**	**88.56**	**88.00**	**87.99**	**88.31**
Qwen2.5-14B	AVERITEC	82.09	82.79	82.21	82.36	85.59	89.75	87.53	87.62	89.46	89.93	90.45	89.95
	SciFact	81.26	82.67	81.53	81.82	85.12	88.72	87.89	87.24	90.66	93.23	90.01	91.30
	HoVer	83.71	84.55	–	84.13	88.82	92.33	–	90.58	89.95	90.78	–	90.37
	FM2	77.84	79.16	–	78.50	82.24	87.47	–	84.86	86.22	88.97	–	87.60
	PolitiHop	83.68	86.69	–	85.19	82.97	85.19	–	84.08	86.72	84.20	–	85.46
	VitaminC	77.86	79.16	78.26	78.43	76.60	82.62	77.92	79.05	81.55	81.32	82.27	81.71
	Average	**81.07**	**82.50**	**80.67**	**81.74**	**83.56**	**87.68**	**84.45**	**85.57**	**87.43**	**88.07**	**87.58**	**87.73**

Table 13: BERTScore under FF-RAW, FACTFLIP, and FF-SIM settings, reported per label and averaged across classes.

particular, political- and religion-related terms exhibit large positive deltas, exceeding +20 points in some cases (e.g., political cues on HoVer and religion cues on PolitiHop). These effects indicate a strong interaction between bias-related lexical cues and contradiction detection. In contrast, effects on the *Support* class are generally smaller and more heterogeneous, with both positive and negative deltas depending on the dataset and bias category. For datasets that include the *NEI* class (e.g., AVERITEC, SciFact, and VitaminC), we observe that bias-related triggers often induce non-trivial ASR increases, particularly for religion, political, and racial cues. Notably, in VitaminC, several bias categories yield double-digit deltas for *NEI*, indicating that such lexical insertions can substantially alter uncertainty-related predictions. Aggregating results across datasets and labels, the final row of Table 16 reports an overall average delta of +2.49, confirming that perturbing bias-related terms systematically exerts a stronger adversarial effect than unperturbing ones. As reported in the main paper, religion and politics emerge as the most impactful categories overall, followed by gender and

Class	RoBERTa	Qwen
Support	55.80 ± 7.26	58.42 ± 14.02
Refute	86.41 ± 8.19	92.79 ± 2.12
NEI	71.61 ± 13.13	70.31 ± 16.51
Average	71.27 ± 9.53	73.84 ± 10.89

Table 14: Percentage of claims filtered out by the LLM-as-a-verifier, reported as mean $\pm$ standard deviation across all the datasets.

Religion	Politics	Gender	Race	Sexual
christian	democrat	man	african	heterosexual
catholic	republican	woman	kurdish	gay
protestant	libertarian	boy	arab	lesbian
anglican	socialist	girl	asian	bisexual
baptist	communist	transgender	caucasian	pansexual
lutheran	anarchist	nonbinary	european	asexual
hindu	monarchist	agender	slavic	queer
jewish	conservative	bigender	mulatto	polysexual
muslim	progressive	pangender	indigenous	sapiosexual
buddhist	capitalist	cisgender	yoruba	aceflux

Table 15: Example of 10 words per bias category (from a total of 50 words per category).

race, while sexual orientation cues tend to produce weaker and less consistent effects. Importantly, the direction and magnitude of the deltas vary considerably across datasets, indicating that these biases are not driven by isolated artifacts but reflect dataset-specific interactions between lexical cues, label distributions, and learned representations. This analysis further supports the use of FACTFLIP as a diagnostic tool for uncovering fine-grained sources of adversarial sensitivity and socially grounded biases in claim verification models.

E.2 Discovering sources of adversarial sensitivity

This analysis examines the extent to which adversarial sensitivity to universal triggers arises from task-specific fine-tuning data versus biases inherited from pre-training. To this end, we compare the perturbing and unperturbing triggers identified by FACTFLIP for two models with identical architectures and downstream training data: a standard RoBERTa initialized with pre-trained weights, and a variant in which the pre-training weights are randomly re-initialized prior to fine-tuning.

Table 17 reports the percentage of word overlap between the top-50 most perturbing and least perturbing triggers extracted from the two models across datasets. The results show near-zero overlap in almost all cases. For most datasets, there is no shared vocabulary between perturbing (or unperturbing) triggers identified in the pre-trained and re-initialized models, with only negligible overlap observed in isolated cases (e.g., FM2 and PolitiHop). This lack of overlap indicates that adversarial triggers are not primarily explained by dataset-specific lexical artifacts acquired during fine-tuning. If fine-tuning data were the dominant source of adversarial sensitivity, one would expect a substantially higher overlap between the two models, given that they are trained on the same downstream data. Instead, the observed divergence suggests that pre-training plays a central role in shaping the lexical vulnerabilities exploited by universal adversarial triggers.

Taking this insight together with the results reported in the main paper, we claim that adversarial sensitivity to universal triggers cannot be attributed exclusively to either pre-training or fine-tuning dynamics. Instead, it emerges from their interaction: biases encoded during pre-training interact with task-specific supervision to produce stable yet model- and dataset-dependent behaviors.

F Prompts and dataset statistics

Prompts. Figure 4 and Figure 5 show the prompts used for the *LLM as a Perturber* and *LLM as a Verifier* respectively. To both the perturber and verifier we specify the entailment constraints that the new generated claim must satisfy, depending on the label between the original claim and evidence. The entailment constraints are specified in §3.2, and their textualized version in Table 18. Inside Figure 4, we specify that the trigger must be inserted inside the original claim without any modification, such as stemming or lemmatization, to make sure that the added trigger is the same as the one found by FACTFLIP. Furthermore, we specify to minimize the number of edits to the original claim, so to better isolate the trigger's contribution to the adversarial performance. Additionally, Figure 6 shows the prompt used by `Qwen` to perform zero-shot claim verification.

Dataset statistics. We report the resulting dataset sizes related to the experiments reported inside this paper. Table 19, Table 20, Table 21 and Table 22 report the number of generated samples associated with the experiments in Table 10, Table 3, Table 1 and Table 9 respectively. From the number of claims that satisfy the verification step, three main patterns can be identified. In particular, (i) generat-

	Dataset	Bias category	*Support*			*Refute*			*NEI*			*Avg* Δ
			pert.	unpert.	Δ	pert.	unpert.	Δ	pert.	unpert.	Δ	
roberta-base	AVERITEC	gender	1.09	0.65	+0.44	4.42	6.23	-1.81	0.55	0.42	+0.13	-0.41
		political	0.94	0.29	+0.65	9.18	12.46	-3.28	0.61	0.49	+0.12	-0.84
		racial	1.09	0.80	+0.29	4.92	6.89	-1.97	0.61	0.67	-0.06	-0.58
		religion	0.58	0.79	-0.21	9.51	7.05	+2.46	0.73	0.61	+0.12	+0.79
		sexual orientation	1.81	0.43	+1.38	5.90	5.08	+0.82	0.49	0.36	+0.13	+0.78
	SciFact	gender	1.63	1.63	0	0.80	4.63	-3.83	1.93	1.23	+0.70	-1.04
		political	0.89	1.48	-0.59	1.20	1.39	-0.19	0.70	0.35	+0.35	-0.14
		racial	2.22	2.52	-0.30	1.29	1.29	0	2.63	0.53	+2.10	+0.60
		religion	2.07	2.81	-0.74	1.39	1.10	+0.29	4.39	0.35	+4.04	+1.20
		sexual orientation	1.18	1.78	-0.60	1.39	1.69	-0.30	1.58	2.10	-0.52	-0.47
	HoVer	gender	0.64	0.06	+0.58	82.72	92.28	-9.56	–	–	–	-4.49
		political	1.12	0.07	+1.05	91.62	71.57	+20.05	–	–	–	+10.55
		racial	0.30	0.22	+0.08	73.45	93.09	-19.64	–	–	–	-9.78
		religion	0.59	0.19	+0.40	90.72	77.15	+13.57	–	–	–	+6.99
		sexual orientation	0.99	0.65	+0.34	59.43	37.86	+21.57	–	–	–	+10.96
	FM2	gender	7.13	0.89	+6.24	12.60	2.57	+10.03	–	–	–	+8.14
		political	2.32	2.70	-0.38	6.13	4.61	+1.52	–	–	–	+0.57
		racial	2.08	1.92	+0.16	9.44	4.94	+4.50	–	–	–	+2.33
		religion	6.18	1.12	+5.06	12.71	6.21	+5.50	–	–	–	+5.28
		sexual orientation	5.43	2.36	+3.07	10.03	4.02	+6.01	–	–	–	+4.54
	PolitiHop	gender	0	0	0	21.82	14.54	+7.28	–	–	–	+3.64
		political	0	0	0	20.00	18.18	+1.82	–	–	–	+0.91
		racial	0	0	0	20.00	10.91	+9.09	–	–	–	+4.55
		religion	0	0	0	27.27	9.09	+18.18	–	–	–	+9.09
		sexual orientation	0	0	0	14.54	5.45	+9.09	–	–	–	+4.55
	VitaminC	gender	0.50	0.57	-0.07	6.55	6.61	-0.06	14.53	5.94	+8.59	+2.82
		political	0.50	0.47	+0.03	11.30	10.12	+1.18	23.79	13.08	+10.71	+3.97
		racial	0.59	0.57	+0.02	14.37	1.39	+12.98	11.89	9.87	+2.02	+5.01
		religion	0.59	0.35	+0.24	7.91	10.21	-2.30	28.09	10.62	+17.47	+5.14
		sexual orientation	0.56	0.44	+0.12	10.47	10.20	+0.27	1.58	2.10	-0.52	-0.04
	Average											**+2.49**

Table 16: Difference in attack success rate between perturbing and non-perturbing lexical cues across bias categories and target classes (Support, Refute, NEI). Results are reported for each dataset by inserting a single bias-related word at the beginning of the claim, followed by a punctuation mark. The last column reports the overall average delta per model.

ing claims targeting the *refute* class is consistently more difficult, as it becomes easier to use trigger words that inadvertently flip the meaning of the original claim, thus not satisfying the entailment constraints; (ii) choosing trigger words based on similarity with the claim (FF-SIM in Table 20) alleviates this issue, while (iii) using multiple triggers does the opposite (Table 22), with almost all generated claim candidates being discarded due to the entailment constraints. The number of instances of the FF-RAW variant in Table 20 represents the upper bound on the amount of instances that can be generated in our experiments, as FF-RAW does not use the perturb-and-verify pipeline.

Dataset	% of word overlap	
	pert.	unpert.
AVERITEC	0.01	0.01
SciFact	0.00	0.00
HoVer	0.05	0.00
FM2	0.01	0.01
PolitiHop	0.01	0.01
VitaminC	0.01	0.00

Table 17: Word overlap between the 50 most perturbing and least perturbing triggers identified by FACTFLIP for a fine-tuned RoBERTa model and a RoBERTa model with re-initialized pre-training weights, both fine-tuned on the same dataset. Scores are averaged across the different target classes.

Perturber prompt

You will be given a claim and a (series of) word(s). You must add all the provided words inside the given claim. The words added must be exactly like the one provided: any sort of stemming, lemmatization or similar is not allowed.
Also, the rest of the claim must be exactly like the original: no existing word must be removed or modified, and only the provided words must be added. The word(s) must be added inside the claim, not at the beginning. In case of multiple words, you can add them in different positions: you are not forced to add them consecutively.
The novel claim must exclusively satisfy the following rule: "{label}"
First reason step-by-step. Then write "Final answer: " followed exclusively by the generated claim. Do not write anything else after "Final answer: ".
Ensure that the final claim is exactly like the original, except for the added word and other stopwords.
Claim: {claim}
Word: {word}
Let's think step-by-step.

Figure 4: Prompt to generate the novel perturbing claims. The *{label}* is filled with the corresponding label instruction shown in Table 18.

Verifier prompt

You will be given an original claim and a novel claim derived from the original. You must determine if the following rule is satisfied or not: "{label}"
First think step-by-step, then write "Final answer:" followed exclusively by "yes" if the rule is satisfied, "no" otherwise. Do not write anything else after "Final answer:".
Original claim: {claim1}
Novel claim: {claim2}
Let's think step-by-step.

Figure 5: Prompt used by the verifier to check the entailment constraints between the original claim and the generated claim. The *{label}* is filled with the corresponding label instruction shown in Table 18.

Label	Instruction
Support	*the factual content of the original claim must entail the factual content of the new claim. Inside the claims, subjective statements are not factual content, and thus they must not be considered in the final evaluation.*
Refute	*the factual content of the new claim must entail the factual content of the original claim. Inside the claims, subjective statements are not factual content, and thus they must not be considered in the final evaluation.*
NEI	*the factual content of the original claim must entail the factual content of the new claim and viceversa. Inside the claims, subjective statements are not factual content, and thus they must not be considered in the final evaluation.*

Table 18: Textual label descriptions used inside the Figure 4 and Figure 5 prompting templates.

Verifier prompt

You are a fact checking system. You must indicate whether the claim is supported, refuted or "not enough information" based on the given evidence.
After "Answer: ", write exclusively "support", "refute" or "not enough information". Do not write anything else.
{input}
Answer:

Figure 6: Prompt used by `Qwen2.5-14B-Instruct` to perform claim verification.

		Support			*Refute*			*NEI*		
	Dataset	FF	FF-DS	AutoPrompt	FF	FF-DS	AutoPrompt	FF	FF-DS	AutoPrompt
roberta-base	AVERITEC	551	490	486	60	50	20	622	421	416
	SciFact	244	57	75	62	14	93	76	53	46
	HoVer	683	711	539	204	151	109	–	–	
	FM2	570	515	585	137	42	46	–	–	
	PolitiHop	322	215	329	15	6	4	–	–	
	VitaminC	455	454	397	134	165	105	749	606	658
Qwen2.5-14B	AVERITEC	438	356	314	42	33	140	420	323	225
	SciFact	121	84	61	94	85	46	78	87	79
	HoVer	358	469	383	59	111	63	–	–	
	FM2	551	521	373	51	43	24	–	–	
	PolitiHop	406	319	324	10	7	10	–	–	
	VitaminC	393	270	263	64	28	24	785	341	206

Table 19: Number of instances produced by the perturb-and-verify pipeline for the experiment in Table 10.

		Support		*Refute*		*NEI*	
	Dataset	FF-RAW	FF-SIM	FF-RAW	FF-SIM	FF-RAW	FF-SIM
roberta-base	AVERITEC	1380	532	610	123	1640	522
	SciFact	675	99	1005	104	570	61
	HoVer	1250	609	1250	474	–	–
	FM2	1270	633	1230	242	–	–
	PolitiHop	690	282	55	16	–	–
	VitaminC	1175	361	1510	372	2315	655
Qwen2.5-14B	AVERITEC	1035	417	595	120	1280	394
	SciFact	785	111	1010	59	675	109
	HoVer	910	461	930	249	–	–
	FM2	1120	496	1060	182	–	–
	PolitiHop	735	346	100	34	–	–
	VitaminC	885	274	1205	261	1800	462

Table 20: Number of samples produced for `FF-RAW` and `FF-SIM` in Table 3.

	Dataset	*Support*		*Refute*		*NEI*	
		pert.	unpert.	pert.	unpert.	pert.	unpert.
roberta-base	AVERITEC	551	603	60	37	622	542
	SciFact	244	41	62	42	76	58
	HoVer	683	890	204	256	–	–
	FM2	570	600	137	118	–	–
	PolitiHop	322	366	15	8	–	–
	VitaminC	455	536	134	174	749	342
Qwen2.5-14B	AVERITEC	438	331	42	227	420	407
	SciFact	121	121	94	264	78	149
	HoVer	358	431	59	291	–	–
	FM2	551	535	51	380	–	–
	PolitiHop	406	273	10	51	–	–
	VitaminC	393	345	64	472	785	507

Table 21: Number of perturbing and unperturbing samples produced by the perturb-and-verify pipeline when one trigger is inserted inside the claim (Table 1).

	Dataset	*Support*		*Refute*		*NEI*	
		pert.	unpert.	pert.	unpert.	pert.	unpert.
roberta-base	AVERITEC	431	427	4	4	442	337
	SciFact	75	38	4	2	33	20
	HoVer	490	725	62	39	–	–
	FM2	531	455	7	31	–	–
	PolitiHop	278	213	2	0	–	–
	VitaminC	493	464	18	15	726	347
Qwen2.5-14B	AVERITEC	265	319	3	64	414	416
	SciFact	60	47	5	57	74	181
	HoVer	364	278	17	108	–	–
	FM2	397	402	17	82	–	–
	PolitiHop	217	149	2	12	–	–
	VitaminC	278	239	15	113	409	589

Table 22: Number of perturbing and unperturbing samples produced by the perturb-and-verify pipeline when 3 triggers are inserted inside the claim (Table 9).