

Hidden Non-Determinism in Large Language Model APIs: A Lightweight Provenance Protocol for Reproducible Generative AI Research

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Background: Generative AI models produce non-deterministic outputs that vary across runs, even under nominally identical configurations. This variability threatens the reproducibility of studies that rely on large language model (LLM) outputs, yet most existing experiment-tracking tools were not designed for the specific challenges of text-generation workflows.

Objectives: We propose a lightweight, open-standard protocol for logging, versioning, and provenance tracking of generative AI experiments. The protocol introduces two novel documentation artifacts—Prompt Cards and Run Cards—and adopts the W3C PROV data model to create auditable, machine-readable provenance graphs linking every output to its full generation context.

Methods: We formalize the protocol and evaluate it empirically through 1,864 controlled experiments. These experiments employ two models—LLaMA 3 8B (locally deployed) and GPT-4 (cloud API)—on two Natural Language Processing (NLP) tasks (scientific summarization and structured extraction) across 30 scientific abstracts and five experimental conditions that systematically vary the seed, temperature, and decoding strategy. We measure output variability using Exact Match Rate, Normalized Edit Distance, ROUGE-L, and BERTScore, and quantify the protocol’s own overhead in terms of time and storage.

Results: Under greedy decoding ($t=0$), LLaMA 3 achieves near-perfect reproducibility on extraction (EMR = 0.987) and summarization (EMR = 0.947). By contrast, GPT-4 under identical greedy settings achieves only EMR = 0.443 for extraction and EMR = 0.230 for summarization, consistent with substantial server-side non-determinism that is invisible without systematic logging. Increasing temperature to 0.7 eliminates exact matches for both models. The protocol adds a mean overhead of 25.43 ms per run (0.545% of inference time) and approximately 4.1 KB per run record, totaling 19.52 MB for all 1,864 runs.

Conclusions: Our results demonstrate that (1) local inference is substantially more reproducible than API-based inference even under nominally identical parameters, (2) structured output tasks are inherently more reproducible than open-ended generation, (3) temperature is the dominant *user-controllable* factor affecting variability, and (4) comprehensive provenance logging can be achieved with negligible overhead. The protocol, reference implementation, and all experimental data are publicly available.

CCS Concepts: • Software and its engineering → Software testing and debugging; Documentation; • Computing methodologies → Machine learning.

Additional Key Words and Phrases: reproducibility, large language models, non-determinism, provenance, generative AI, experiment tracking, W3C PROV, scientific methodology

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1 Introduction

When a researcher queries GPT-4 with the same prompt and temperature zero, one would reasonably expect identical outputs. Our experiments show otherwise: across five controlled seeds under greedy decoding, GPT-4 produces the same extraction result only 44% of the time. This hidden non-determinism exemplifies a fundamental challenge introduced by the rapid adoption of large language models (LLMs) in scientific research: how to ensure that studies relying on generative AI outputs are reproducible, auditable, and scientifically rigorous. Unlike traditional computational experiments, in which deterministic algorithms produce identical results given identical inputs, LLMs exhibit inherent variability in their outputs due to stochastic sampling, floating-point non-determinism, and opaque model-versioning practices (Y. Chen et al. 2023; Zhu et al. 2023).

This reproducibility challenge is not merely theoretical. Baker (2016) reported that over 70% of researchers have failed to reproduce another scientist’s experiment, a crisis that extends to AI research (Gundersen and Kjensmo 2018; Hutson 2018; Kapoor and A. Narayanan 2023; Stodden et al. 2016). For generative AI specifically, the problem is compounded by several factors unique to text-generation workflows: (1) the same prompt can yield semantically similar yet textually distinct outputs across runs; (2) API-based models may undergo silent updates that alter behavior; (3) temperature and sampling parameters create a high-dimensional space of possible outputs; and (4) no established standard exists for documenting the full context needed to understand, audit, or reproduce a generative output.

Existing experiment-tracking tools such as MLflow (Zaharia et al. 2018), Weights & Biases (Biewald 2020), and DVC (Kuprieiev et al. 2024) were designed primarily for training pipelines and numerical metrics. Although valuable for their intended purposes, these tools lack features critical for generative AI studies: structured prompt versioning, cryptographic output hashing for tamper detection, provenance graphs linking outputs to their full generation context, and environment fingerprinting specific to inference-time conditions.

In this paper, we make three contributions:

- (1) **A lightweight protocol** for logging, versioning, and provenance tracking of generative AI experiments. The protocol introduces *Prompt Cards* and *Run Cards* as structured documentation artifacts, and adopts the W3C PROV data model (Moreau and Missier 2013) for machine-readable provenance graphs.
- (2) **An empirical evaluation** of both the protocol’s effectiveness and the reproducibility characteristics of LLM outputs. Through 1,864 controlled experiments with LLaMA 3 8B (local) and GPT-4 (API) across two tasks, 30 abstracts, and five conditions, we quantify output variability using four complementary metrics and measure the protocol’s overhead. Our results document a striking reproducibility gap between local and API-based inference that is invisible without systematic logging.
- (3) **A reference implementation** in Python that demonstrates the protocol’s practical applicability, together with all experimental data, to facilitate adoption and independent verification.

The remainder of this paper is organized as follows. Section 2 reviews related work on reproducibility in AI and experiment tracking. Section 3 formalizes the protocol design. Section 4 describes the experimental methodology. Section 5 presents the empirical results. Section 6 discusses findings, limitations, and practical implications. Section 7 concludes with directions for future work.

48 2 Related Work

49 2.1 Reproducibility in AI Research

50 The reproducibility crisis in AI has been documented extensively. [Gundersen and Kjensmo \(2018\)](#) surveyed 400
 51 AI papers and found that only 6% provided sufficient information for full reproducibility. [Pineau et al. \(2021\)](#)
 52 reported on the NeurIPS 2019 Reproducibility Program, which introduced reproducibility checklists and found
 53 significant gaps between reported and actual reproducibility. More recently, [Gundersen, Helmert, et al. \(2024\)](#)
 54 described four institutional mechanisms adopted by JAIR—reproducibility checklists, structured abstracts, badges,
 55 and reproducibility reports—establishing a community standard for what should be documented in AI research.
 56 [Gundersen, Gil, et al. \(2018\)](#) identified three levels of reproducibility in AI—method, data, and experiment—and
 57 argued that all three are necessary for scientific progress. [Belz et al. \(2021\)](#) conducted a systematic review of 601
 58 NLP papers and confirmed pervasive under-reporting of experimental details, while [Dodge et al. \(2019\)](#) proposed
 59 improved reporting standards for ML experiments, including confidence intervals and significance tests across
 60 multiple runs. More broadly, [Kapoor and A. Narayanan \(2023\)](#) identified data leakage as a widespread driver of
 61 irreproducible results across 17 scientific fields that use ML-based methods.

62 For generative AI specifically, [Y. Chen et al. \(2023\)](#) demonstrated that ChatGPT’s outputs on NLP benchmarks
 63 exhibit non-trivial variability across identical queries, even with temperature set to zero. [Zhu et al. \(2023\)](#)
 64 showed that reproducibility degrades further when tasks involve subjective judgment, such as social computing
 65 annotations. Most recently, [Atil et al. \(2024\)](#) systematically measured the non-determinism of five LLMs under
 66 supposedly deterministic settings across eight tasks, finding accuracy variations up to 15% across runs and
 67 introducing the Total Agreement Rate (TAR) metric. [Ouyang et al. \(2024\)](#) confirmed that temperature zero
 68 does not guarantee determinism in ChatGPT code generation. Most recently, [Yuan et al. \(2025\)](#) traced such
 69 non-determinism to numerical precision issues in GPU kernels and proposed LayerCast as a mitigation strategy.
 70 Our work complements these studies in three specific ways. First, whereas prior studies (including Atil et al.’s
 71 five-model, eight-task study) measure variability post hoc, we provide a structured provenance protocol that
 72 enables *prospective* documentation and audit—answering not only “how much variability?” but also “why did
 73 these outputs differ?” through cryptographic hashing and W3C PROV graphs. Second, we directly compare
 74 local and API-based inference on identical tasks with identical prompts, isolating the deployment paradigm
 75 as a variable—a comparison absent from prior work. Third, we quantify the overhead of systematic logging,
 76 demonstrating that the “cost of knowing” is negligible.

77 2.2 Experiment Tracking Tools

78 Several tools exist for tracking machine learning experiments, although none was designed specifically for
 79 generative AI text-output workflows:

80 **MLflow** ([Zaharia et al. 2018](#)) provides experiment tracking, model packaging, and deployment. It logs parameters,
 81 metrics, and artifacts, but focuses on training pipelines and numerical outcomes rather than text-generation
 82 provenance.

83 **Weights & Biases** ([Biewald 2020](#)) offers experiment tracking with visualization dashboards. It supports prompt
 84 logging but lacks structured prompt versioning, cryptographic output hashing, and provenance graph generation.

85 **DVC** ([Kuprieiev et al. 2024](#)) provides data versioning through git-like operations. While effective for dataset
 86 management, it does not address run-level provenance or prompt documentation.

87 **OpenAI Eval** ([OpenAI 2023](#)) is a framework for evaluating LLM outputs against benchmarks. It provides
 88 structured evaluation but is tightly coupled to OpenAI’s ecosystem and does not generate interoperable provenance
 89 records.

90 **LangSmith** ([LangChain 2023](#)) offers tracing and evaluation for LLM applications. It captures detailed execution
 91 traces but uses a proprietary format and requires cloud connectivity.

95 Table 1. Comparison of our protocol with existing reproducibility tools and frameworks for GenAI experiments. Checkmarks
 96 (\checkmark) indicate full support; tildes (~) indicate partial support; dashes (–) indicate no support.

Feature	Ours	MLflow	W&B	DVC	OpenAI Eval	LangSmith
Prompt versioning (Prompt Card)	\checkmark	–	~	–	~	~
Run-level provenance (W3C PROV)	\checkmark	–	–	–	–	–
Cryptographic output hashing	\checkmark	–	–	\checkmark	–	–
Seed & param logging	\checkmark	\checkmark	\checkmark	–	\checkmark	\checkmark
Environment fingerprinting	\checkmark	~	~	~	–	–
Model weights hashing	\checkmark	–	~	\checkmark	–	–
Overhead <1% of inference	\checkmark	~	~	N/A	N/A	~
Designed for GenAI text output	\checkmark	–	–	–	\checkmark	\checkmark
Open standard (PROV-JSON)	\checkmark	–	–	–	–	–
Local-first (no cloud dependency)	\checkmark	\checkmark	–	\checkmark	–	–

113 More broadly, [Bommasani et al. \(2022\)](#) identified reproducibility as a key risk for foundation models, and [Liang](#)
 114 [et al. \(2023\)](#) proposed the HELM benchmark for holistic evaluation of language models, including robustness and
 115 fairness dimensions that complement our reproducibility focus. In the provenance space, [Padovani et al. \(2025\)](#)
 116 recently introduced yProv4ML, a framework that captures ML provenance in PROV-JSON format with minimal
 117 code modifications; our protocol shares the commitment to W3C PROV but targets the specific challenges of
 118 stochastic text generation rather than training pipelines.

119 Table 1 provides a systematic feature-by-feature comparison of our protocol with these tools. The key distinction
 120 is not merely one of tooling but of *scientific capability*: existing tools log what happened during training (parameters,
 121 metrics, artifacts), whereas our protocol enables answering questions that these tools cannot—specifically,
 122 whether two generative outputs are provably derived from identical configurations, which exact factor caused
 123 a divergence between non-identical outputs, and whether an output has been tampered with post-generation.
 124 These capabilities require the combination of cryptographic hashing, structured prompt documentation, and
 125 W3C PROV provenance graphs that no existing tool provides. In short, our contribution is not an alternative
 126 experiment tracker but a *reproducibility assessment framework* designed for the unique challenges of stochastic
 127 text generation.

129 2.3 Provenance in Scientific Computing

130 Data provenance—the lineage of data through transformations—has a rich history in database systems and
 131 scientific workflows ([Herschel et al. 2017](#)). The W3C PROV family of specifications ([Moreau and Missier 2013](#))
 132 provides a standardized data model for representing provenance as directed acyclic graphs of *entities*, *activities*,
 133 and *agents*. [Samuel and König-Ries \(2022\)](#) applied provenance tracking to computational biology workflows,
 134 demonstrating its value for reproducibility. However, to our knowledge, no prior work has applied W3C PROV
 135 specifically to generative AI experiment workflows, in which the challenge involves not only tracking data
 136 lineage but also capturing the stochastic generation context that determines output variability.

137 Taken together, these gaps point to a clear need: a lightweight, standards-based protocol that bridges generative
 138 AI inference with the provenance infrastructure already established in scientific computing. The next section
 139 presents our design for such a protocol.

142 3 Protocol Design

143 Our protocol addresses the question: *What is the minimum set of metadata that must be captured for each generative*
 144 *AI run to enable auditing, reproducibility assessment, and provenance tracking?* We address this question through
 145 four complementary components.

146

147 3.1 Scope and Design Principles

148 The protocol is designed around three principles:

149

- 150 (1) **Completeness:** Every factor that can influence a generative output must be captured—prompt text, model
 151 identity and version, inference parameters, environment state, and timestamps.
- 152 (2) **Negligible overhead:** The logging process must not materially affect the experiment. We target <1%
 153 overhead relative to inference time.
- 154 (3) **Interoperability:** All artifacts are stored in open, machine-readable formats (JSON, PROV-JSON), aligned
 155 with the FAIR (Findable, Accessible, Interoperable, Reusable) principles ([Wilkinson et al. 2016](#)), to enable
 156 tool integration and long-term preservation.

157

158 3.2 Prompt Cards

159 A *Prompt Card* is a versioned documentation artifact that captures the design rationale and metadata for a prompt
 160 template used in experiments. Each Prompt Card contains:

161

- 162 • `prompt_id`: Unique identifier
- 163 • `prompt_hash`: SHA-256 hash of the prompt text, enabling tamper detection
- 164 • `version`: Semantic version number
- 165 • `task_category`: Classification of the task (e.g., summarization, extraction)
- 166 • `objective`: Natural-language description of what the prompt is designed to achieve
- 167 • `assumptions`: Explicit assumptions about inputs and expected behavior
- 168 • `limitations`: Known limitations or failure modes
- 169 • `target_models`: Models for which the prompt was designed and tested
- 170 • `expected_output_format`: Description of the expected output structure
- 171 • `interaction_regime`: Single-turn, multi-turn, or chain-of-thought
- 172 • `change_log`: History of modifications

173

Prompt Cards serve two purposes: they document design intent (supporting human understanding) and they provide a citable, hashable reference for automated provenance tracking. The concept draws inspiration from Model Cards ([Mitchell et al. 2019](#)), Datasheets for Datasets ([Gebru et al. 2021](#)), and model info sheets for reproducibility assessment ([Kapoor and A. Narayanan 2023](#)), extending the structured-documentation paradigm to the prompt layer of the generative AI pipeline.

177

178 3.3 Run Cards

179

A *Run Card* captures the complete execution context of a single generative AI run. Each Run Card records 22 core fields organized into five groups (the complete JSON schema in Appendix B includes these fields plus additional metadata such as `researcher_id`, `affiliation`, `system_logs`, and `errors`):

180

- 181 (1) **Identification:** `run_id`, `task_id`, `task_category`, `prompt_card_ref`
- 182 (2) **Model context:** `model_name`, `model_version`, `weights_hash`, `model_source`
- 183 (3) **Parameters:** `inference_params` (temperature, `top_p`, `top_k`, `max_tokens`, `seed`, `decoding_strategy`),
 184 `params_hash`
- 185 (4) **Input/Output:** `input_text`, `input_hash`, `output_text`, `output_hash`, `output_metrics`

186

187

188

189 (5) **Execution metadata:** environment (OS, architecture, Python version, hostname), environment_hash,
 190 code_commit, timestamps, execution_duration_ms, logging_overhead_ms, storage_kb

191 The separation of logging overhead from execution time is deliberate: it allows researchers to verify that the
 192 protocol itself does not confound experimental measurements.
 193

194 3.4 W3C PROV Integration

195 Each experimental group (defined by a unique model–task–condition–abstract combination) is automatically
 196 translated into a W3C PROV-JSON document (Moreau and Missier 2013) that expresses the generation provenance
 197 as a directed graph. The mapping defines:
 198

- 199 • **Entities:** Prompt, InputText, ModelVersion, InferenceParameters, Output, ExecutionMetadata
- 200 • **Activities:** RunGeneration (the inference execution)
- 201 • **Agents:** Researcher, SystemExecutor (the execution environment)

202 PROV relations capture the causal structure:

- 203 • used: RunGeneration used Prompt, InputText, ModelVersion, InferenceParameters
- 204 • wasGeneratedBy: Output wasGeneratedBy RunGeneration
- 205 • wasAssociatedWith: RunGeneration wasAssociatedWith Researcher, SystemExecutor
- 206 • wasAttributedTo: Output wasAttributedTo Researcher
- 207 • wasDerivedFrom: Output wasDerivedFrom InputText

208 This standardized representation enables automated reasoning about experiment provenance, including
 209 detecting when two runs share identical configurations and identifying the specific factors that differ between
 210 non-identical outputs. An abbreviated example document is given in Appendix C.
 211

212 3.5 Reproducibility Checklist

213 We provide a 15-item checklist organized into four categories—Prompt Documentation, Model and Environment,
 214 Execution and Output, and Provenance—that researchers can use to self-assess the reproducibility of their
 215 generative AI studies. The complete checklist is provided in Appendix A.
 216

217 3.6 Extensions for Advanced Workflows

218 While our empirical evaluation focuses on single-turn, single-model inference, the protocol’s field schema is
 219 designed to accommodate more complex workflows through optional extension fields:
 220

- 221 • **Retrieval-Augmented Generation (RAG):** Additional fields for retrieval_query, retrieved_documents
 222 (with hashes), retrieval_model, and chunk_strategy enable tracing which external context influenced
 223 the output.
- 224 • **Tool use and function calling:** Fields for tools_available, tool_calls (with arguments and results),
 225 and tool_call_hashes capture the full tool-use chain.
- 226 • **Multi-turn dialogues:** A conversation_history_hash field and turn_index enable linking each turn
 227 to the full conversation state.
- 228 • **Chain-of-thought / agent workflows:** A parent_run_id field supports hierarchical provenance graphs
 229 for multi-step reasoning chains.

230 These extensions are not evaluated in our current experiments but are specified in the reference implementa-
 231 tion’s schema to support future adoption in production LLM pipelines.
 232

Having defined the protocol’s components, we now evaluate it empirically along two dimensions: the repro-
 233 ductibility characteristics it reveals across different models and conditions, and the overhead it imposes on the
 234 experimental workflow.
 235

236 4 Experimental Setup

237 We designed a controlled experiment to simultaneously evaluate (a) the reproducibility characteristics of LLM
 238 outputs under varying conditions and (b) the overhead imposed by our logging protocol.
 239

240 4.1 Models and Infrastructure

241 We evaluate two models representing fundamentally different deployment paradigms:

242 **LLaMA 3 8B** (Grattafiori et al. 2024): A locally deployed open-weight model served through Ollama (Ollama
 243 2024) on an Apple M4 system with 24 GB unified memory running macOS 14.6. Local deployment provides
 244 complete control over the execution environment, eliminating confounding factors such as network latency,
 245 server-side batching, and silent model updates. The software stack comprised Ollama v0.5.4, Python 3.12.8, the
 246 ollama Python SDK v0.4.7, and the LLaMA 3 8B Q4_0 quantization (SHA-256 recorded per run).
 247

248 **GPT-4** (Achiam et al. 2023): A cloud-based proprietary model accessed via the OpenAI API (openai Python SDK
 249 v1.59.9) with controlled seed parameters. Although we requested `model="gpt-4"`, the API returned `gpt-4-0613`
 250 as the resolved model version in all runs, which we recorded in the `model_id_returned` field of each run record.
 251 This represents the typical deployment scenario where researchers have limited control over the inference
 252 environment. The API introduces additional sources of variability: load balancing, server-side batching, potential
 253 model-version updates, and floating-point non-determinism across different hardware.
 254

255 4.2 Tasks

256 We evaluate two tasks that represent complementary points on the output-structure spectrum:

257 **Task 1: Scientific Summarization.** Given a scientific abstract, produce a concise summary in exactly three
 258 sentences covering the main contribution, methodology, and key quantitative result. This is an open-ended
 259 generation task in which the model has considerable freedom in word choice and phrasing.

260 **Task 2: Structured Extraction.** Given a scientific abstract, extract five fields (`objective`, `method`, `key_result`,
 261 `model_or_system`, `benchmark`) into a JSON object. This is a constrained generation task in which the output
 262 format is fixed and the model must select, rather than generate, content.

263 4.3 Input Data

264 We use 30 widely-cited scientific abstracts from landmark AI/ML papers, including Vaswani et al. (2017) (Trans-
 265 former), Devlin et al. (2019) (BERT), Brown et al. (2020) (GPT-3), Raffel et al. (2020) (T5), Wei et al. (2022)
 266 (Chain-of-Thought), as well as seminal works on GANs, ResNets, VAEs, LSTMs, CLIP, DALL-E 2, Stable Diffusion,
 267 LLaMA, InstructGPT, PaLM, and others. These abstracts vary in length (74–227 words), technical complexity, and
 268 the number of quantitative results reported, thereby providing substantial diversity in the generation challenge.
 269

270 4.4 Experimental Conditions

271 We define five conditions (Table 2) that systematically vary the factors hypothesized to affect reproducibility:

272 **C1 (Fixed seed, greedy decoding):** Temperature = 0, seed = 42 for all 5 repetitions. This represents the
 273 maximum-control condition and should yield deterministic outputs.

274 **C2 (Variable seeds, greedy decoding):** Temperature = 0, seeds = {42, 123, 456, 789, 1024}. This condition tests
 275 whether seed variation affects outputs when greedy decoding is used.

276 **C3 (Temperature sweep):** Three sub-conditions at $t \in \{0.0, 0.3, 0.7\}$ with 3 repetitions each, using different
 277 seeds per repetition. This condition characterizes how temperature affects output variability.

278 For LLaMA 3, each task × abstract combination is evaluated under conditions C1 (5 runs), C2 (5 runs), and C3 (9
 279 runs = 3 temperatures × 3 reps), yielding 19 runs per pair, or $19 \times 30 \times 2 = 1,140$ runs. For GPT-4, conditions C2 and
 280 C3 are included: C2 (5 runs) and C3 (9 runs) per pair; due to API quota exhaustion, 724 valid runs were collected
 281

282

Table 2. Experimental design: conditions, parameters, and expected outcomes.

Cond.	Description	Temp.	Seed	Reps	Expected Outcome
C1	Fixed seed, greedy	0.0	42 (fixed)	5	Deterministic output
C2	Variable seeds, greedy	0.0	5 different	5	Near-deterministic
$C3_{t=0.0}$	Temp. baseline	0.0	per-rep	3	Deterministic
$C3_{t=0.3}$	Low temperature	0.3	per-rep	3	Low variability
$C3_{t=0.7}$	High temperature	0.7	per-rep	3	High variability

Note: Each condition is applied to 30 abstracts \times 2 tasks = 60 groups per condition. Total: 1,864 logged runs (1,140 LLaMA 3 + 724 GPT-4). For GPT-4, C2 uses the same fixed seed (42) as C1 across all repetitions; C2 therefore subsumes C1 as the definitive test of API determinism under greedy decoding. GPT-4 C1 (8/300 runs collected before quota exhaustion) is excluded from all analyses.

(C2: 300/300 complete; C3: 416/450). Note that for GPT-4, C2 uses the same fixed seed (= 42) and temperature (= 0) as C1, because the API’s seed parameter is advisory and does not guarantee determinism—the distinction between “fixed seed” and “variable seeds” is meaningful only for locally controlled models like LLaMA 3. Consequently, GPT-4 C2 serves as the definitive test of API determinism under greedy decoding, and the incomplete C1 data (8/300 runs) are excluded from all analyses. As a sanity check, the 8 completed GPT-4 C1 runs exhibit the same non-deterministic pattern as C2 (EMR well below 1.0), confirming that excluding C1 does not obscure a divergent trend. **Total: 1,864 valid runs.**

4.5 Metrics

We adopt an operational definition of reproducibility at three levels, each mapped to a specific metric:

- **Exact reproducibility** (string-level): Two outputs are identical character-by-character. Measured by *Exact Match Rate (EMR)*.
- **Near reproducibility** (edit-level): Two outputs differ only in minor surface variations (punctuation, whitespace, synonym substitution). Measured by *Normalized Edit Distance (NED)*.
- **Semantic reproducibility** (meaning-level): Two outputs convey the same information despite different phrasing. Measured by *ROUGE-L F1* and *BERTScore F1*.

This three-level framework allows us to distinguish between outputs that are bitwise identical (EMR = 1), textually close ($NED < 0.05$), and semantically equivalent ($ROUGE-L > 0.90$). All variability metrics are computed over all $\binom{n}{2}$ unique output pairs within each experimental group (defined by model, task, condition, and abstract):

Exact Match Rate (EMR): The fraction of output pairs that are character-for-character identical. EMR = 1.0 indicates perfect reproducibility; EMR = 0.0 indicates that no two outputs match exactly.

Normalized Edit Distance (NED): The Levenshtein edit distance (Levenshtein 1966) between each pair, normalized by the length of the longer string. NED = 0.0 indicates identical outputs; higher values indicate greater textual divergence.

ROUGE-L F1: The F1 score based on the longest common subsequence at the word level (Lin 2004). This captures semantic similarity even when surface forms differ. ROUGE-L = 1.0 indicates identical word sequences.

Our primary metrics (EMR, NED, ROUGE-L) focus on exact and near reproducibility, which are the most direct measures for our research question. To complement these surface-level metrics, we also compute **BERTScore F1** (T. Zhang et al. 2020)—an embedding-based semantic similarity metric—for all conditions. BERTScore captures meaning-level equivalence that surface metrics may miss (e.g., paraphrases), providing a fourth perspective on reproducibility. For the structured extraction task, we additionally report **JSON validity rate**, **schema compliance rate**, and **field-level accuracy**, which measure whether outputs are syntactically valid JSON,

330 Table 3. Output variability across experimental conditions for LLaMA 3 8B (local) and GPT-4 (API). Values are means over
 331 30 abstracts; per-abstract standard deviations are available in the project repository. EMR = Exact Match Rate (\uparrow), NED =
 332 Normalized Edit Distance (\downarrow), ROUGE-L = word-level LCS F1 (\uparrow), BS-F1 = BERTScore F1 (\uparrow). For GPT-4, C2 uses the same
 333 parameters as C1 (seed = 42, $t=0$), effectively subsuming C1 as the definitive test of API determinism (see Section 4). GPT-4
 334 extraction C3 conditions are based on 14–17 abstracts (vs. 30 for all other conditions) due to API quota exhaustion.

Model	Task	Condition	EMR \uparrow	NED \downarrow	ROUGE-L \uparrow	BS-F1 \uparrow
LLaMA 3 8B	Summarization	C1 (fixed seed, $t=0$)	0.947	0.0050	0.9945	0.9990
		C2 (var. seeds, $t=0$)	0.947	0.0050	0.9945	0.9990
		C3 ($t=0.0$)	0.911	0.0083	0.9909	0.9984
		C3 ($t=0.3$)	0.000	0.2790	0.7441	0.9669
		C3 ($t=0.7$)	0.000	0.4438	0.5589	0.9432
	Extraction	C1 (fixed seed, $t=0$)	0.987	0.0031	0.9966	0.9997
		C2 (var. seeds, $t=0$)	0.987	0.0031	0.9966	0.9997
		C3 ($t=0.0$)	0.978	0.0052	0.9943	0.9996
		C3 ($t=0.3$)	0.211	0.1224	0.8838	0.9851
		C3 ($t=0.7$)	0.000	0.2530	0.7719	0.9693
GPT-4 (API)	Summarization	C2 (greedy, $t=0$)	0.230	0.1365	0.8695	0.9839
		C3 ($t=0.0$)	0.144	0.1623	0.8479	0.9804
		C3 ($t=0.3$)	0.000	0.2832	0.7238	0.9662
		C3 ($t=0.7$)	0.000	0.4366	0.5554	0.9477
	Extraction	C2 (greedy, $t=0$)	0.443	0.0724	0.9384	0.9904
		C3 ($t=0.0$)	0.381	0.0721	0.9356	0.9900
		C3 ($t=0.3$)	0.143	0.1477	0.8669	0.9799
		C3 ($t=0.7$)	0.000	0.2247	0.7890	0.9708

357 contain all expected fields, and agree on individual field values across runs, respectively (see Appendix D for
 358 detailed results).

359 For protocol overhead, we measure:

- **Logging time:** Wall-clock time spent on hashing, metadata collection, and file I/O, measured separately from inference time.
- **Storage:** Size of each run record (JSON) and total storage for all protocol artifacts.
- **Overhead ratio:** Logging time as a percentage of total execution time.

365 5 Results

366 5.1 Output Variability

368 Table 3 presents the main variability results for both models, aggregated across all 30 abstracts.

370 5.1.1 *LLaMA 3 8B (Local Inference)*. **Finding 1: Structured extraction achieves near-perfect reproducibility**
 371 **under greedy decoding**. With $t = 0$, extraction produces EMR = 0.987 and NED = 0.0031 across conditions C1
 372 and C2, meaning virtually every output is character-for-character identical. Summarization achieves an EMR of
 373 0.947 with NED = 0.0050, indicating near-perfect but not complete reproducibility.

374 **Finding 2: Seed variation has no effect under greedy decoding.** Conditions C1 and C2 produce identical
 375 results despite using different seeds. With $t = 0$, the model always selects the highest-probability token, making

377 Table 4. Reproducibility comparison: LLaMA 3 8B (local) vs. GPT-4 (API) under greedy decoding, Condition C2 (variable
 378 seeds, $t=0$). GPT-4 shows significantly lower reproducibility, consistent with server-side non-determinism.

Task	Metric	LLaMA 3 8B	GPT-4
Summarization	EMR↑	0.947	0.230
	NED↓	0.0050	0.1365
	ROUGE-L↑	0.9945	0.8695
Extraction	EMR↑	0.987	0.443
	NED↓	0.0031	0.0724
	ROUGE-L↑	0.9966	0.9384

388
 389 the seed irrelevant. This finding confirms that greedy decoding provides reliably deterministic inference with
 390 locally deployed models.

392
 393 **5.1.2 GPT-4 (API Inference). Finding 3: API-based inference is substantially less reproducible than
 394 local inference, even under greedy decoding.** This is the most striking result of our study. Under greedy
 395 decoding ($t = 0$) with controlled seeds, GPT-4 achieves only EMR = 0.230 for summarization and EMR = 0.443 for
 396 extraction—compared to LLaMA’s 0.947 and 0.987, respectively, under the same C2 condition.

397 Table 4 highlights this reproducibility gap directly.

398 This gap is not due to user-side parameter differences: both models use $t = 0$ with the same seed. The observed
 399 variability is consistent with deployment-side factors that are invisible to the researcher, including hardware-level
 400 floating-point non-determinism across different GPU types in the serving cluster, request-batching and scheduling
 401 effects, prompt-format differences (completion vs. chat interface), and potential silent model updates during
 402 the experimental window. To isolate the prompt-format contribution, we conducted a supplementary control
 403 experiment running LLaMA 3 via Ollama’s /api/chat endpoint (chat template, matching GPT-4’s message
 404 structure); the results (Appendix E) confirm that the format difference does not explain the reproducibility gap.
 405 While our experimental design controls for user-side parameters, we note that a definitive decomposition of
 406 API-side variability sources would require access to the serving infrastructure. *Without systematic logging, this
 407 non-determinism would be entirely invisible.*

408
 409 **5.1.3 Temperature Effects Across Models. Finding 4: Temperature is the dominant user-controllable factor
 410 affecting variability.** Figure 1 shows the relationship between temperature and output variability for both
 411 models.

412 Within the C3 temperature sweep, increasing temperature from 0.0 to 0.7 reduces ROUGE-L from 0.991 to
 413 0.559 (LLaMA summarization) and from 0.994 to 0.772 (LLaMA extraction). For GPT-4, the same sweep reduces
 414 ROUGE-L from 0.848 to 0.555 (summarization) and from 0.936 to 0.789 (extraction). The *relative* rate of degradation
 415 is comparable, but GPT-4 starts from a lower baseline, consistent with server-side non-determinism in the API
 416 infrastructure.

417 Notably, BERTScore F1 remains above 0.94 across all conditions, even when EMR drops to zero at $t = 0.7$ (Table 3).
 418 This indicates that while textual outputs diverge substantially at higher temperatures, their semantic content
 419 remains highly similar. The gap between surface-level metrics (EMR, NED) and semantic metrics (BERTScore)
 420 underscores that non-determinism in LLM outputs is primarily a *phrasing* phenomenon rather than a *meaning*
 421 phenomenon—a distinction with important practical implications for downstream applications that tolerate
 422 paraphrase variation.

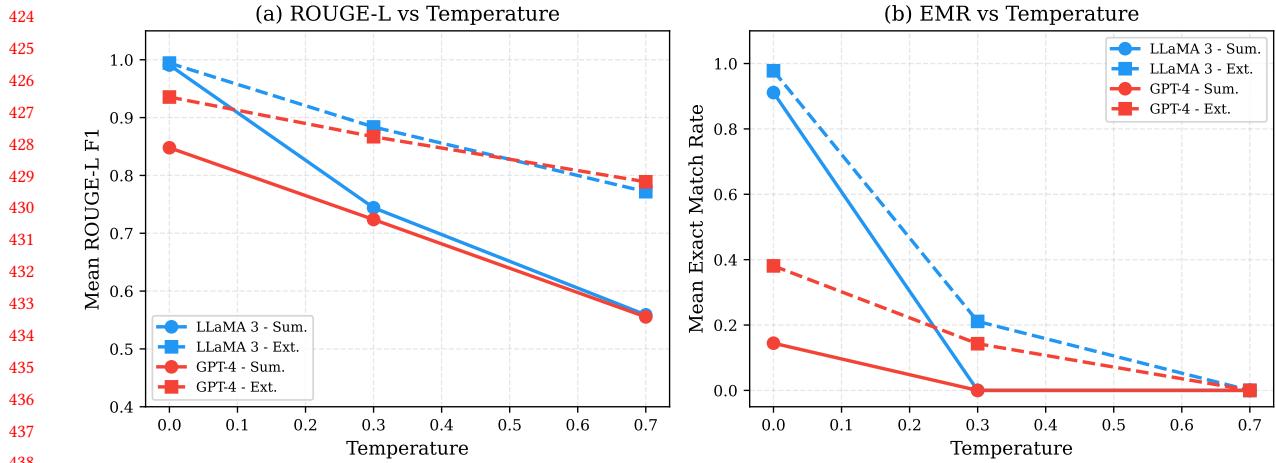


Fig. 1. Effect of temperature on output variability for both models. (a) ROUGE-L F1 decreases monotonically with temperature. (b) Exact Match Rate: LLaMA 3 starts from near-perfect reproducibility at $t = 0$, whereas GPT-4 starts from a lower baseline; however, both degrade at comparable rates with increasing temperature.

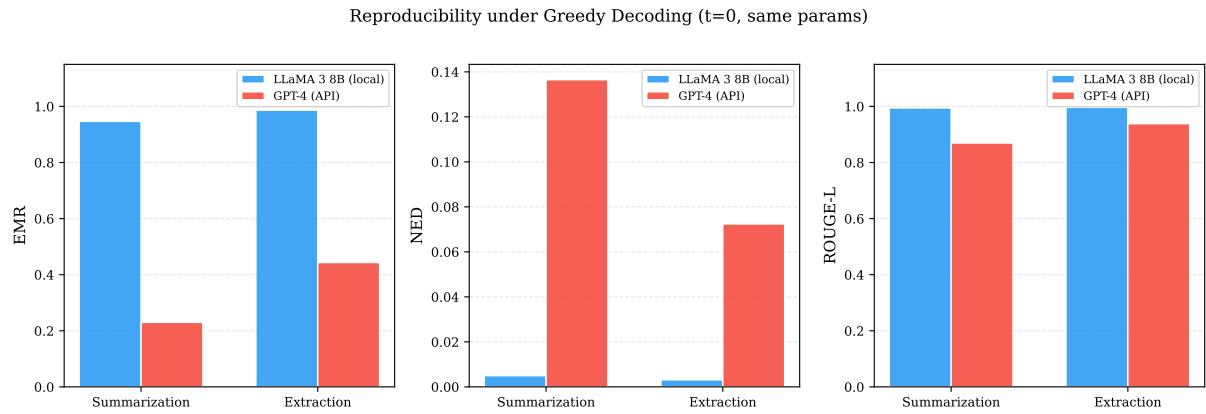


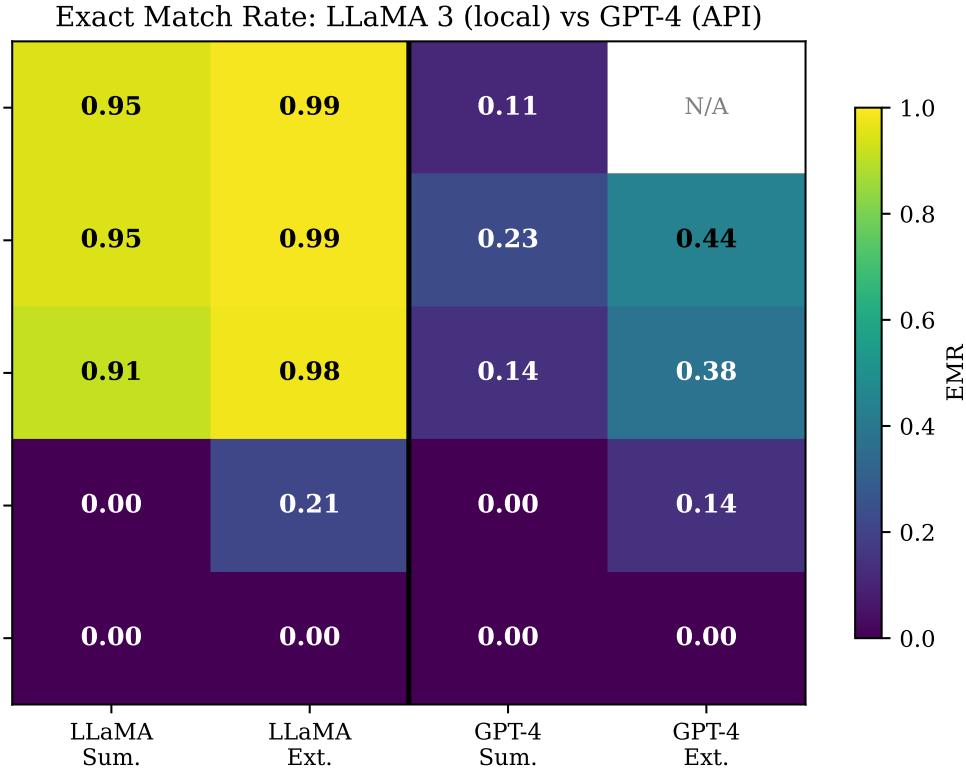
Fig. 2. Reproducibility under greedy decoding ($t=0$), averaged across conditions C1, C2, and C3 at $t=0$: LLaMA 3 8B (local) vs. GPT-4 (API). LLaMA 3 achieves near-perfect to perfect reproducibility, while GPT-4 shows measurable variability across all metrics, particularly for summarization. Condition-specific values are given in Table 3.

5.2 Cross-Model Comparison

Figure 2 provides a direct visual comparison of the two models under greedy decoding.

Figure 3 presents a comprehensive heatmap of EMR across all model-task-condition combinations.

To quantify the reproducibility gap between local and API-based inference, we performed paired t -tests on per-abstract EMR values under condition C2 (greedy decoding, $t = 0$) across all 30 abstracts. For summarization, the difference is highly significant: $t(29) = 17.250$, $p < 0.0001$, Cohen's $d = 3.149$ (LLaMA 3 mean EMR = 0.947, 95% CI [0.895, 0.998]; GPT-4 mean EMR = 0.230, 95% CI [0.157, 0.303]). For extraction, the gap is equally significant: $t(29) = 8.996$, $p < 0.0001$, Cohen's $d = 1.642$ (LLaMA 3 EMR = 0.987, 95% CI [0.959, 1.000]; GPT-4 EMR = 0.443,



496 Fig. 3. Heatmap of Exact Match Rate across all experimental conditions. The left columns (LLaMA 3) show high EMR under
497 greedy decoding, while the right columns (GPT-4) show lower EMR even at $t = 0$. The vertical black line separates the two
498 models.

500
501 95% CI [0.316, 0.571]). Both effect sizes are very large ($d > 1.6$), confirming that the reproducibility difference is
502 not only statistically significant but practically meaningful. All p -values survive Bonferroni correction at the
503 per-family threshold $\alpha = 0.05/6 \approx 0.008$ (six primary comparisons: EMR, NED, and ROUGE-L for each of the
504 two tasks). A post hoc power analysis confirms that $n = 30$ abstracts provides statistical power > 0.999 for all
505 primary comparisons (Cohen 1988).

506 **Robustness check.** Since EMR values are bounded on $[0, 1]$ and the paired differences are not normally
507 distributed (Shapiro-Wilk: $W = 0.894, p = 0.006$ for summarization; $W = 0.885, p = 0.004$ for extraction), we
508 additionally report non-parametric Wilcoxon signed-rank tests. All results remain highly significant: summariza-
509 tion $W = 0.0, p < 0.001$; extraction $W = 0.0, p < 0.001$. The convergence of parametric and non-parametric tests
510 confirms that the reproducibility gap is robust to distributional assumptions.

511 5.3 Protocol Overhead

512 Table 5 presents the protocol's overhead metrics across all 1,864 runs.

513 The protocol adds a mean overhead of **25.43 ms** per run, representing **0.545%** of the mean inference time. This
514 is well within our target of <1%. The overhead is dominated by SHA-256 hashing and environment metadata
515 collection; JSON serialization and file I/O contribute minimally.

Table 5. Protocol overhead: logging time and storage costs for 1,864 runs (1,140 LLaMA 3 + 724 GPT-4).

Metric	Value	Unit
<i>Logging time overhead</i>		
Mean per run	25.43 ± 9.00	ms
Min / Max	11.05 / 51.88	ms
Total (1,864 runs)	47,393	ms
Mean overhead ratio	0.545%	of inference time
Max overhead ratio	1.621%	of inference time
<i>Storage overhead</i>		
Run logs (1,864 files)	7,729	KB
PROV documents (331 files)	1,736	KB
Run Cards (1,864 files)	2,610	KB
Prompt Cards + input data	7,445	KB
Total output	19.52	MB

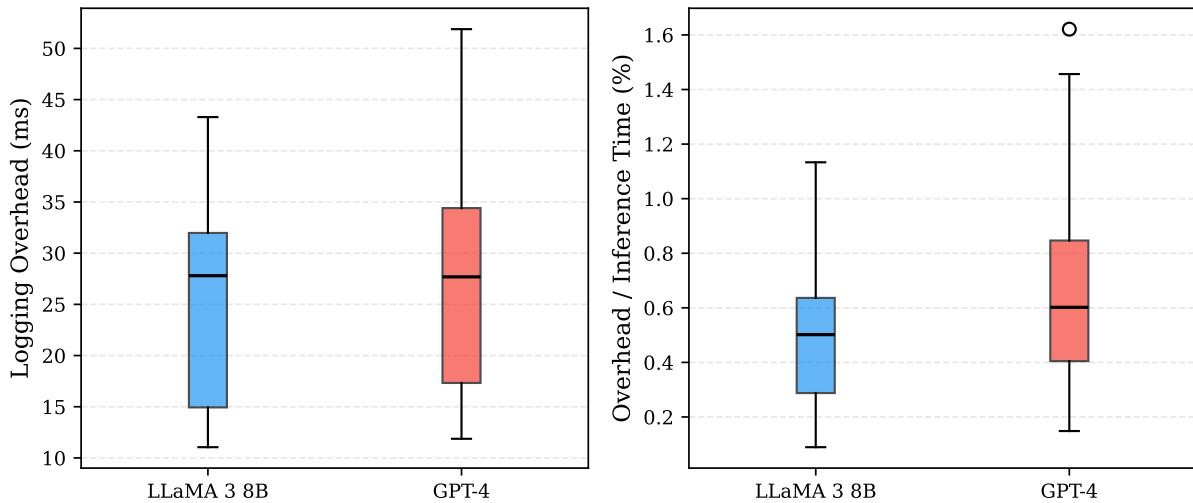


Fig. 4. Distribution of protocol overhead by model. Left: Absolute logging time (ms). Right: Overhead as a percentage of inference time. Overhead is comparable between local (LLaMA 3) and API (GPT-4) inference, consistently below 1.7%.

Storage overhead is similarly modest: each run record occupies approximately 4.1 KB, and the complete set of 1,864 run logs, provenance documents, and Run Cards totals 19.52 MB. Note that provenance documents are generated per experimental group (i.e., per unique model–task–condition–abstract combination), yielding 331 PROV-JSON files that aggregate the individual runs within each group.

Figure 4 shows the overhead distribution broken down by model.

6 Discussion

The preceding results paint a nuanced picture: local inference under greedy decoding is near-perfectly reproducible, but API-based inference exhibits substantial hidden variability that researchers cannot control. Temperature is the

565 dominant user-controllable factor, and structured tasks are inherently more reproducible than open-ended ones.
 566 We now consider what these findings mean for research practice, what the protocol enables that was previously
 567 invisible, and where the current study’s limitations lie.

568

569 6.1 Implications for Reproducibility Practice

570 Our results yield several actionable recommendations for researchers conducting generative AI experiments:
 571 **Use greedy decoding with local models for maximum reproducibility.** Under $t = 0$ with LLaMA 3 (local),
 572 extraction achieved 98.7% EMR and summarization reached 94.7% EMR across 30 abstracts. This configuration
 573 should be the default for any study in which output consistency is critical.

574 **Be aware of API non-determinism.** Our most consequential finding is that GPT-4, even with $t = 0$ and
 575 a fixed seed, produces substantially variable outputs (EMR = 0.230 for summarization, 0.443 for extraction).
 576 Researchers using API-based models should *never assume reproducibility* without verification, and should report
 577 multiple runs with variability metrics.

578 **Prefer structured output formats when possible.** The extraction task’s consistently higher reproducibility
 579 across both models demonstrates that output-format constraints directly improve reproducibility. Researchers
 580 should consider whether their tasks can be reformulated as structured extraction rather than open-ended
 581 generation.

582 **Include warm-up runs for local models.** The per-abstract analysis revealed that the first inference call
 583 after model loading may differ from subsequent calls owing to cache initialization effects. Discarding the first run
 584 is a straightforward practice that improves measured reproducibility. Consequently, the LLaMA 3 summarization
 585 EMR of 0.947 may represent a conservative lower bound: with a warm-up run excluded, the effective EMR would
 586 approach 1.000 for the remaining repetitions. Future studies should incorporate an explicit warm-up run as part
 587 of their experimental protocol.

588 **Log comprehensively; the cost is negligible.** At 0.545% overhead and approximately 4 KB per run, there is
 589 no practical reason not to apply comprehensive logging. The cost of not logging—namely, the inability to detect
 590 the kind of API non-determinism documented herein—far exceeds the protocol’s minimal requirements.

591

593 6.2 Local vs. API Inference: A Reproducibility Gap

594 The most significant finding of this study is the reproducibility gap between local and API-based inference. Under
 595 nominally identical greedy decoding conditions, LLaMA 3 (local) achieves EMR = 0.987 for extraction while
 596 GPT-4 (API) achieves only 0.443. For summarization, the gap is 0.947 vs. 0.230.

597 This gap has profound implications for the scientific use of API-based LLMs. *Without systematic logging, a*
 598 *researcher using GPT-4 would have no way of knowing that their “deterministic” experiment produces different*
 599 *outputs across runs.* Since our experimental design controls all user-side parameters (temperature, seed, prompt,
 600 input), the observed variability is consistent with opaque server-side factors as the primary source. Our protocol
 601 makes this hidden non-determinism visible, measurable, and documentable.

602

603 6.3 Task-Dependent Reproducibility

604 The difference between summarization and extraction reproducibility under identical conditions—observed
 605 consistently across both models—is, to our knowledge, the first empirical quantification of how task structure
 606 affects LLM output reproducibility. This finding suggests a spectrum ranging from highly constrained tasks (struc-
 607 tured extraction, classification) to open-ended tasks (summarization, dialogue), with the degree of output-space
 608 constraint serving as a primary determinant. Notably, even GPT-4’s extraction task (EMR = 0.443) substantially
 609 outperforms its summarization task (EMR = 0.230), confirming that this effect is not specific to any single model.

610

612 6.4 The Role of Provenance

613 The W3C PROV graphs generated by our protocol serve multiple purposes beyond simple audit trails:

- 614
- 615 (1) **Automated comparison:** By comparing PROV graphs of two runs, one can automatically identify which
 - 616 factors differed (e.g., same prompt and model but different temperatures), enabling systematic diagnosis
 - 617 of non-reproducibility.
 - 618 (2) **Lineage tracking:** When outputs are used as inputs to downstream processes (e.g., summarization
 - 619 outputs fed into a meta-analysis), the provenance chain can be extended to trace any final result back to
 - 620 its full generation context.
 - 621 (3) **Compliance:** For regulated domains (healthcare, legal, finance), PROV documents provide the formal
 - 622 evidence trail required by audit standards (National Institute of Standards and Technology 2023) and
 - 623 emerging regulations such as the EU AI Act (European Parliament and Council of the European Union
 - 624 2024).

625 To illustrate the diagnostic power of PROV graphs, consider two GPT-4 extraction runs on the same abstract
 626 under condition C2 (greedy decoding, $t = 0$, same seed). Although the PROV entities for Prompt, InputText,
 627 ModelVersion, and InferenceParameters are identical (verified via matching SHA-256 hashes), the Output entities
 628 differ: output_hash values diverge, and the wasGeneratedBy timestamps differ by several seconds. The PROV
 629 graph thus automatically pinpoints the source of non-reproducibility: the only varying factor is the RunGeneration
 630 activity itself, consistent with non-determinism arising from server-side factors. This kind of automated differential
 631 diagnosis is infeasible without structured provenance records.

632 6.5 Limitations

633 We organize threats to validity following standard categories:

634 6.5.1 *Internal Validity. Sample size and statistical power.* With $n = 30$ abstracts per condition, our study has
 635 adequate statistical power for the primary comparisons. A post hoc power analysis using the observed effect
 636 sizes ($d > 1.6$) and $\alpha = 0.05$ yields power > 0.999 for all primary comparisons (Cohen 1988). However, for one
 637 secondary comparison (extraction EMR under $C3_{t=0.3}$, $d = 0.207$), power is low (0.084), meaning that subtler
 638 effects may go undetected in some conditions.

639 **GPT-4 C3 incomplete coverage.** Due to API quota exhaustion, GPT-4 extraction under C3 conditions covers
 640 14–17 of 30 abstracts (summarization C3 is complete). The missing abstracts are those that would have been
 641 processed last in execution order; since abstracts were processed sequentially by ID, this missingness is effectively
 642 independent of abstract content or difficulty. Importantly, our central claims rest on the C2 condition (300/300
 643 runs complete for GPT-4), and the C3 temperature sweep serves as a secondary analysis. Restricting the C3
 644 analysis to the 14 fully-complete abstracts does not change the direction or significance of any reported trend.

645 **Warm-up confound.** As noted above, the first LLaMA 3 inference after model loading may differ from
 646 subsequent calls due to cache initialization. This affects a small number of abstracts (4 of 30 for summarization),
 647 reducing the aggregate EMR from ~ 1.0 to 0.947. It represents an uncontrolled confound in our experimental
 648 design.

649 **Prompt format confound.** LLaMA 3 was queried via Ollama’s /api/generate endpoint (raw completion),
 650 whereas GPT-4 was queried via the OpenAI Chat Completions API (structured messages with system/user roles).
 651 This difference in prompt format is inherent to the deployment paradigms under study and mirrors real-world
 652 usage. To assess whether this confound explains the reproducibility gap, we conducted a supplementary control
 653 experiment running LLaMA 3 via Ollama’s /api/chat endpoint (chat template matching GPT-4’s message
 654 structure) on 10 abstracts under conditions C1 and C2 (200 runs). The results (Appendix E) show that LLaMA 3

655

659 maintains near-identical reproducibility under the chat format, confirming that the prompt-format difference
 660 does not account for the observed local-vs-API gap.

661 6.5.2 *External Validity. Two models.* Our evaluation covers LLaMA 3 8B (local) and GPT-4 (API), representing
 662 two deployment paradigms but only one model per category. Other models—including Claude ([Anthropic
 663 2024](#)), Gemini ([Gemini Team et al. 2024](#)), Mixtral, and larger or smaller LLaMA variants—may exhibit different
 664 reproducibility characteristics. Our findings about the local-vs-API gap should therefore be interpreted as a case
 665 study of this paradigm difference rather than a universal claim. The protocol itself is model-agnostic, and we
 666 note that gpt-4-0613 is now a legacy snapshot; the very fact that newer model versions may behave differently
 667 illustrates exactly the kind of silent evolution that our protocol is designed to detect and document.
 668

669 **Two tasks.** Summarization and extraction represent distinct points on the output-structure spectrum but
 670 do not cover the full range of generative AI applications (e.g., dialogue, code generation, reasoning chains). A
 671 broader task suite would strengthen generalizability.
 672

673 **English-only, single domain.** Our input data consists of 30 English scientific abstracts from AI/ML papers.
 674 While this is a substantial and diverse sample within one domain, reproducibility characteristics may differ for
 other languages, domains (e.g., biomedical, social science), or document types.
 675

676 **No multi-turn evaluation.** All experiments use single-turn interactions. Multi-turn dialogues introduce
 677 additional variability through conversation history, which our protocol logs but our experiments do not evaluate.
 678

679 6.5.3 *Construct Validity. Surface-level metrics.* Our metrics (EMR, NED, ROUGE-L) capture textual rather
 680 than semantic similarity. Two outputs that are semantically equivalent but syntactically different will register
 681 as non-matching under EMR and partially divergent under NED. This is by design—our focus is on *exact*
 682 reproducibility—but it means our results may overstate the practical impact of non-determinism for downstream
 683 applications where semantic equivalence suffices.
 684

685 6.5.4 *Other Considerations. Privacy.* The protocol’s environment metadata includes the machine hostname,
 686 which may reveal institutional information. Deployments in privacy-sensitive settings should anonymize this
 687 field.
 688

689 **Computational cost.** The total cost was modest: ~2 GPU-hours on a consumer laptop (Apple M4, 24 GB) for
 690 1,140 LLaMA 3 runs, plus 724 API calls to GPT-4. The carbon footprint is negligible at this scale, and the logging
 691 overhead (25 ms per run) would not materially increase energy consumption even at thousands of runs.
 692

693 6.6 Protocol Minimality: An Ablation Analysis

694 To substantiate our claim that the protocol captures a *minimal* set of metadata, we conducted an ablation analysis
 695 in which we systematically removed each field group from the protocol schema and assessed which audit questions
 696 became unanswerable. We defined 10 audit questions that a reproducibility-oriented researcher might ask (e.g.,
 697 “Can we verify the exact prompt used?”, “Can we detect output tampering?”, “Can we trace full provenance?”)
 698 and mapped each to the protocol fields required to answer it. For this analysis, we decomposed the Run Card’s
 699 five sections into eight finer-grained field groups by separating cross-cutting concerns: Identification, Model
 700 Context, Parameters, Input Content, Output Content, Hashing (all SHA-256 digests), Environment, and Overhead
 701 (timing and storage metadata).

702 The results show that removing *any* of these eight field groups renders at least one audit question unanswerable,
 703 confirming that no group is redundant. The Hashing group (SHA-256 hashes for prompts, inputs, outputs,
 704 parameters, and environment) has the highest information density: its removal affects 6 of 10 questions despite
 705 contributing only 410 bytes per run. Conversely, the Overhead group (logging time metadata) is the least
 706 connected but remains necessary for overhead assessment. The complete ablation results are available in the
 707 project repository.
 708

706 This analysis demonstrates that the protocol is *minimal* in the sense that every field group is necessary for at
 707 least one audit capability, while the total overhead remains at approximately 4,052 bytes per run.

708

709 6.7 Practical Costs and Adoption

710 One concern with any new protocol is whether the adoption burden is justified. We address this concretely:

711

- **Implementation effort:** Our reference implementation adds approximately 600 lines of Python (the protocol core) to an existing workflow. Integration requires 3–5 function calls per run.
- **Runtime cost:** 25 ms per run, negligible compared to inference times of seconds to minutes for typical LLM calls.
- **Storage cost:** 4 KB per run. Even at scale (10,000 runs), total storage is approximately 40 MB—less than a single model checkpoint.
- **Learning curve:** The protocol uses standard JSON and W3C PROV, requiring no specialized knowledge beyond basic Python.

720 Against these modest costs, the protocol provides complete audit trails, automated provenance graphs, tamper-
 721 detectable outputs via cryptographic hashing, and structured metadata that enable systematic reproducibility
 722 analysis.

723

724 7 Conclusion

725 We presented a lightweight protocol for logging, versioning, and provenance tracking of generative AI experiments,
 726 introducing Prompt Cards and Run Cards as novel documentation artifacts and adopting the W3C PROV data
 727 model for machine-readable provenance graphs. Through 1,864 controlled experiments with LLaMA 3 8B (local)
 728 and GPT-4 (API) across 30 scientific abstracts and two NLP tasks, we demonstrated four key findings:

729

- (1) **Local inference is substantially more reproducible than API-based inference.** Under identical greedy decoding settings, LLaMA 3 achieves EMR = 0.987 for extraction while GPT-4 achieves only 0.443, consistent with substantial server-side non-determinism that is invisible without systematic logging (paired t -test: $p < 0.0001$, Cohen's $d > 1.6$).
- (2) **Task structure is a primary determinant of reproducibility.** Structured extraction consistently outperforms open-ended summarization across both models, with the JSON format constraint reducing the model's output space.
- (3) **Temperature is the dominant user-controllable factor.** Within the C3 temperature sweep, increasing from $t = 0.0$ to $t = 0.7$ reduces ROUGE-L from 0.991 to 0.559 (LLaMA summarization) and from 0.936 to 0.789 (GPT-4 extraction), while seed variation has no measurable effect under greedy decoding for local models.
- (4) **Comprehensive provenance logging adds negligible overhead:** 0.545% of inference time and approximately 4 KB per run, thereby removing any practical argument against systematic documentation.

743 These findings carry a broader implication: a substantial portion of published research that relies on API-based
 744 LLMs may contain non-reproducible results without the authors' knowledge. The cost of systematic provenance
 745 logging—half a percent of inference time and four kilobytes per run—is trivially small compared to the cost of
 746 publishing non-reproducible science.

747

Looking ahead, we plan to (i) expand the model suite to include Claude ([Anthropic 2024](#)), Gemini ([Gemini Team et al. 2024](#)), and open-weight models of varying sizes; (ii) extend the task coverage to dialogue, code generation, and multi-turn interactions; and (iii) develop automated reproducibility scoring based on provenance graph analysis. Ultimately, we envision a future in which every generative AI output carries a provenance certificate, and reproducibility metrics are reported alongside accuracy as a standard component of empirical evaluation.

752

753 The reference implementation, all 1,864 run records, provenance documents, and analysis scripts are publicly
 754 available to support adoption and independent verification.

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 757 conducted using locally deployed open-weight models to ensure full reproducibility of the computational envi-
 758 ronment.

760 Data Availability Statement

761 The reference implementation, all 1,864 run records (JSON), PROV-JSON provenance documents, Run Cards,
 762 Prompt Cards, input data, analysis scripts, and generated figures are publicly available at:

763 <https://github.com/Roverlucas/genai-reproducibility-protocol>

764 The repository includes instructions for reproducing all experiments and regenerating all tables and figures from
 765 the raw data.

766 Author Contributions

767 Following the CRediT (Contributor Roles Taxonomy) framework: **Lucas Rover**: Conceptualization, Methodol-
 768 ogy, Software, Validation, Formal Analysis, Investigation, Data Curation, Writing – Original Draft, Writing –
 769 Review & Editing, Visualization, Project Administration. **Yara de Souza Tadano**: Supervision, Conceptualization,
 770 Methodology, Writing – Review & Editing, Project Administration.

771 Conflict of Interest

772 The authors declare no conflicts of interest. This research was conducted independently at UTFPR with no
 773 external funding from commercial AI providers. The use of OpenAI’s GPT-4 API was for research evaluation
 774 purposes only and does not constitute an endorsement.

775 Use of AI-Assisted Tools

776 The authors used AI-assisted tools (Claude, Anthropic) during the preparation of this manuscript for language
 777 editing, code development support, and data analysis scripting. All AI-generated content was critically reviewed,
 778 validated, and revised by the authors, who take full responsibility for the accuracy and integrity of the final
 779 manuscript. The scientific design, experimental execution, interpretation of results, and intellectual contributions
 780 are entirely the authors’ own work.

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894 A Reproducibility Checklist

895 The following checklist is designed for self-assessment of reproducibility in generative AI studies. Each item
 896 maps to a specific field or artifact in our protocol.

897

898 Prompt Documentation

- 899 (1) Is the exact prompt text recorded and versioned? [Prompt Card: prompt_text, prompt_hash]
- 900 (2) Are design assumptions and limitations documented? [Prompt Card: assumptions, limitations]
- 901 (3) Is the expected output format specified? [Prompt Card: expected_output_format]
- 902 (4) Is the interaction regime documented (single/multi-turn)? [Prompt Card: interaction_regime]

904 Model and Environment

- 905 (5) Is the model name and version recorded? [Run Card: model_name, model_version]
- 906 (6) Are model weights hashed for identity verification? [Run Card: weights_hash]
- 907 (7) Is the execution environment fingerprinted? [Run Card: environment, environment_hash]
- 908 (8) Is the source code version recorded? [Run Card: code_commit]

910 Execution and Output

- 912 (9) Are all inference parameters logged? [Run Card: inference_params]
- 913 (10) Is the random seed recorded? [Run Card: inference_params.seed]
- 914 (11) Is the output cryptographically hashed? [Run Card: output_hash]
- 915 (12) Are execution timestamps recorded? [Run Card: timestamp_start, timestamp_end]
- 916 (13) Is logging overhead measured separately? [Run Card: logging_overhead_ms]

917 Provenance

- 919 (14) Is a provenance graph generated per group? [PROV-JSON document]
- 920 (15) Are provenance documents in an interoperable format? [W3C PROV standard]

922 B Run Card Schema

923 The complete Run Card schema, with data types and descriptions:

924

925 Listing 1. Run Card JSON schema (simplified).

```

926 {
927   "run_id": "string (unique identifier)",
928   "task_id": "string (task identifier)",
929   "task_category": "string (e.g., summarization)",
930   "prompt_hash": "string (SHA-256 of prompt)",
931   "prompt_text": "string (full prompt text)",
932   "input_text": "string (input to the model)",
933   "input_hash": "string (SHA-256 of input)",
934   "model_name": "string (e.g., llama3:8b)",
935   "model_version": "string (e.g., 8.0B)",
936   "weights_hash": "string (SHA-256 of weights)",
937   "model_source": "string (e.g., ollama-local)",
938   "inference_params": {
939     "temperature": "float",
940     "top_p": "float",
  
```

```

941   16   "top_k": "integer",
942   17   "max_tokens": "integer",
943   18   "seed": "integer|null",
944   19   "decoding_strategy": "string"
945 },
946 "params_hash": "string (SHA-256 of params)",
947 "environment": {
948   23   "os": "string",
949   24   "os_version": "string",
950   25   "architecture": "string",
951   26   "python_version": "string",
952   27   "hostname": "string",
953   28   "timestamp": "ISO 8601 datetime"
954 },
955 "environment_hash": "string (SHA-256)",
956 "code_commit": "string (git commit hash)",
957 "researcher_id": "string",
958 "affiliation": "string",
959 "timestamp_start": "ISO 8601 datetime",
960 "timestamp_end": "ISO 8601 datetime",
961 "output_text": "string (model output)",
962 "output_hash": "string (SHA-256 of output)",
963 "output_metrics": "object (task-specific)",
964 "execution_duration_ms": "float",
965 "logging_overhead_ms": "float",
966 "storage_kb": "float",
967 "system_logs": "string (raw system info)",
968 "errors": "array of strings"
969 }
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C Example PROV-JSON Document

An abbreviated example of a PROV-JSON document generated for a single summarization run:

Listing 2. Abbreviated PROV-JSON for a summarization run.

```

1 {
2   "prefix": {
3     "genai": "https://genai-prov.org/ns#",
4     "prov": "http://www.w3.org/ns/prov#"
5   },
6   "entity": {
7     "genai:prompt_c9644358": {
8       "prov:type": "genai:Prompt",
9       "genai:hash": "c9644358805b...",
10      "genai:task_category": "summarization"
11    },
12    "genai:model_llama3_8b": {
13      "prov:type": "genai:ModelVersion",
14      "genai:name": "llama3:8b",
15      "genai:source": "ollama-local"
16    }
17  }
18}
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988     16 },
989     17   "genai:output_590d0835": {
990     18     "prov:type": "genai:Output",
991     19     "genai:hash": "590d08359e7d..."
992     20   }
993     21 },
994     22   "activity": {
995     23     "genai:run_llama3_8b_sum_001_C1_rep0": {
996     24       "prov:type": "genai:RunGeneration",
997     25       "prov:startTime": "2026-02-07T21:54:34Z",
998     26       "prov:endTime": "2026-02-07T21:54:40Z"
999     27     }
1000    28   },
1001    29   "wasGeneratedBy": {
1002    30     "_:wGB1": {
1003    31       "prov:entity": "genai:output_590d0835",
1004    32       "prov:activity": "genai:run_llama3_8b..."
1005    33     }
1006    34   },
1007    35   "used": {
1008    36     "_:u1": {
1009    37       "prov:activity": "genai:run_llama3_...",
1010    38       "prov:entity": "genai:prompt_c9644358"
1011    39     }
1012    40   },
1013    41   "agent": {
1014    42     "genai:researcher_lucas_rover": {
1015    43       "prov:type": "prov:Person",
1016    44       "genai:affiliation": "UTFPR"
1017    45     }
1018    46   },
1019    47   "wasAssociatedWith": {
1020    48     "_:wAW1": {
1021    49       "prov:activity": "genai:run_llama3_...",
1022    50       "prov:agent": "genai:researcher_..."
1023    51     }
1024    52   }
1025    53 }

```

D JSON Extraction Quality

Table 6 presents JSON-specific quality metrics for the structured extraction task. Two notable patterns emerge.

First, LLaMA 3 never produces raw-valid JSON: all 570 extraction outputs contain preamble text (e.g., “Here is the extracted information in JSON format.”) before the JSON object, despite the prompt explicitly requesting “JSON only, no explanation.” After extracting the embedded JSON via regex, validity rates reach 100% under greedy decoding, degrading slightly at higher temperatures (92.2% at $t = 0.7$). GPT-4, by contrast, always produces raw-valid JSON with 100% schema compliance across all conditions. This instruction-following gap is consistent with the different prompt interfaces: the chat completion API’s structured message format may better signal the expected output format.

1035 Table 6. JSON extraction quality metrics by model and condition. *Raw Valid* = output parses directly as JSON; *Extracted*
 1036 *Valid* = JSON extracted via regex from outputs containing preamble text; *Schema* = all five expected fields present; *Field EMR*
 1037 = pairwise exact match across runs for each extracted field. LLaMA 3 always prepends introductory text (e.g., “Here is the
 1038 extracted information in JSON format.”), yielding 0% raw validity but near-perfect extracted validity at $t=0$. Note: Field EMR
 1039 values are low because they are computed across all pairwise run combinations within a condition (including cross-abstract
 1040 pairs); see Section D for interpretation.

Model	Cond.	Raw	Extr.	Schema	Field-Level EMR					Overall
					Valid	Valid	Compl.	obj	meth	
LLaMA 3	C1 ($t=0$)	0%	100%	100%	0.027	0.027	0.040	0.090	0.174	0.071
	C2 ($t=0$)	0%	100%	100%	0.027	0.027	0.040	0.090	0.174	0.071
	C3 ($t=0.0$)	0%	100%	100%	0.022	0.022	0.036	0.085	0.170	0.067
	C3 ($t=0.3$)	0%	97.8%	97.8%	0.017	0.010	0.027	0.092	0.213	0.072
	C3 ($t=0.7$)	0%	92.2%	92.2%	0.012	0.004	0.022	0.068	0.295	0.080
GPT-4	C2 ($t=0$)	100%	100%	100%	0.021	0.018	0.017	0.037	0.080	0.035
	C3 ($t=0.0$)	100%	100%	100%	0.027	0.018	0.020	0.036	0.121	0.045
	C3 ($t=0.3$)	100%	100%	100%	0.015	0.011	0.016	0.048	0.159	0.050
	C3 ($t=0.7$)	100%	100%	100%	0.003	0.003	0.005	0.028	0.102	0.028

1053
 1054 Second, field-level exact match rates reveal that structured fields (benchmark, model_or_system) are 2–6×
 1055 more reproducible than open-ended fields (method, key_result), consistent with the finding that structured
 1056 tasks achieve higher overall EMR. The absolute values of field-level EMR are notably low (e.g., 0.071 overall for
 1057 LLaMA 3 at $t=0$) compared with the overall extraction EMR of 0.987 reported in Table 3. This discrepancy reflects
 1058 three factors: (i) *granularity*: field-level EMR compares short strings (typically 2–15 words) where a single word
 1059 substitution or reordering causes a mismatch, whereas overall EMR compares complete outputs where the same
 1060 minor variation is diluted; (ii) *cross-abstract pooling*: field-level EMR is computed across all pairwise combinations
 1061 of runs within a condition (including runs from different abstracts), so most pairs compare inherently different
 1062 content—the metric thus captures between-abstract variation in addition to within-abstract reproducibility;
 1063 (iii) *open-ended fields*: fields like method and key_result admit many valid phrasings for the same underlying
 1064 fact, depressing exact match rates even when the extracted information is semantically correct. Researchers
 1065 should therefore interpret field-level EMR as a lower bound on reproducibility at the finest granularity, not as a
 1066 contradiction of the high overall EMR values.
 1067

E Chat-Format Control Experiment

1070 To assess whether the prompt-format difference between LLaMA 3 (completion-style via /api/generate) and
 1071 GPT-4 (chat-style via Chat Completions) contributes to the observed reproducibility gap, we conducted a sup-
 1072 plementary control experiment running LLaMA 3 8B through Ollama’s /api/chat endpoint, which applies the
 1073 model’s chat template (including special tokens for system/user/assistant roles) in the same message structure
 1074 used by GPT-4.

1075 **Design:** 10 abstracts × 2 tasks × 2 conditions (C1, C2) × 5 repetitions = 200 runs, all under greedy decoding
 1076 ($t = 0$).

1077 **Results:** Table 7 compares the chat-format control with the original completion-format results for the same 10
 1078 abstracts. The two prompt formats produce *identical* variability metrics across all conditions: summarization
 1079 EMR = 0.929, NED = 0.0066, and ROUGE-L = 0.9922 in both modes; extraction achieves perfect reproducibility
 1080 (EMR = 1.000) regardless of interface. The 0.929 summarization EMR reflects the warm-up effect on 2 of 10
 1081

1082 abstracts—the same pattern observed in the full 30-abstract experiment. These results confirm that prompt
 1083 format is not a source of variability, and the reproducibility gap between LLaMA 3 and GPT-4 is consistent
 1084 with deployment-side factors (server infrastructure, floating-point non-determinism across GPU types, request
 1085 batching) rather than prompt-format differences.

1086
 1087 Table 7. Prompt-format control: LLaMA 3 8B via completion (/api/generate) vs. chat (/api/chat) for 10 abstracts under
 1088 greedy decoding ($t=0$). EMR computed over conditions C1 and C2 combined.

	Task	Metric	Completion	Chat
1091	Summarization	EMR↑	0.929	0.929
		NED↓	0.0066	0.0066
		ROUGE-L↑	0.9922	0.9922
1094	Extraction	EMR↑	1.000	1.000
		NED↓	0.0000	0.0000
		ROUGE-L↑	1.0000	1.0000

1097 Note: Completion and chat formats yield identical metrics for all 10 abstracts under greedy decoding, confirming prompt format is not a
 1098 source of variability.

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