

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

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A *Run Card* captures the complete execution context of a single generative AI run. Each Run Card records 24 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

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

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189 (5) **Execution metadata:** environment (OS, architecture, Python version, hostname), environment_hash,
 190 code_commit, timestamps (start/end), 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 • **RAG:** Fields for retrieval query, retrieved documents (with hashes), retrieval model, and chunk strategy
 trace which external context influenced the output.
- 222 • **Tool use and function calling:** Fields for available tools, tool calls (with arguments, results, and hashes)
 capture the full tool-use chain.
- 223 • **Multi-turn dialogues:** A conversation_history_hash field and turn_index enable linking each turn
 to the full conversation state.
- 224 • **Chain-of-thought / agent workflows:** A parent_run_id field supports hierarchical provenance graphs
 for multi-step reasoning chains.

225 These extensions are not evaluated in our current experiments but are specified in the reference implementa-
 226 tion’s schema to support future adoption in production LLM pipelines.
 227

228 Having defined the protocol’s components, we now evaluate it empirically along two dimensions: the repro-
 229 ducibility characteristics it reveals across different models and conditions, and the overhead it imposes on the
 230 experimental workflow.
 231

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 **4.2 Tasks**

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

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

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

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 **Design principle for API models.** For cloud-hosted APIs whose seed parameter is advisory rather than
 273 deterministic (as documented by OpenAI for GPT-4), the fixed-vs.-variable seed distinction has no guaranteed
 274 effect server-side. We therefore treat C2 as the primary test of determinism under greedy decoding for such
 275 models; the rationale is detailed below.

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

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

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.

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

For LLaMA 3, each task \times abstract combination is evaluated under conditions C1 (5 runs), C2 (5 runs), and C3 (9 runs = 3 temperatures \times 3 reps), yielding 19 runs per pair, or $19 \times 30 \times 2 = 1,140$ runs. For GPT-4, conditions C2 and C3 are included: C2 (5 runs) and C3 (9 runs) per pair; due to API quota exhaustion, 724 runs were collected (C2: 300/300 complete; C3: 416/450; C1: 8/300, excluded from analyses as detailed below), yielding 716 runs used in all reported analyses. 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 ($\text{NED} < 0.05$), and semantically equivalent ($\text{ROUGE-L} > 0.90$). 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.

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

365 For protocol overhead, we measure:

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

372 5 Results

373 5.1 Output Variability

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

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 \uparrow	0.947	0.230
	NED \downarrow	0.0050	0.1365
	ROUGE-L \uparrow	0.9945	0.8695
Extraction	EMR \uparrow	0.987	0.443
	NED \downarrow	0.0031	0.0724
	ROUGE-L \uparrow	0.9966	0.9384

388
 389 **5.1.1 LLaMA 3 8B (Local Inference). Finding 1: Structured extraction achieves near-perfect reproducibility**
 390 **under greedy decoding.** With $t=0$, extraction produces EMR = 0.987 and NED = 0.0031 across conditions C1
 391 and C2, meaning virtually every output is character-for-character identical. Summarization achieves an EMR of
 392 0.947 with NED = 0.0050, indicating near-perfect but not complete reproducibility.

393 **Finding 2: Seed variation has no effect under greedy decoding.** Conditions C1 and C2 produce identical
 394 results despite using different seeds. With $t=0$, the model always selects the highest-probability token, making
 395 the seed irrelevant. This finding confirms that greedy decoding provides reliably deterministic inference with
 396 locally deployed models.

397 **5.1.2 GPT-4 (API Inference). Finding 3: API-based inference is substantially less reproducible than local**
 398 **inference, even under greedy decoding.** This is the most striking result of our study. Under greedy decoding
 399 ($t=0$) with controlled seeds, GPT-4 achieves only EMR = 0.230 for summarization and EMR = 0.443 for extraction—
 400 compared to LLaMA’s 0.947 and 0.987, respectively, under the same C2 condition.

401 Table 4 highlights this reproducibility gap directly.

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

412 **5.1.3 Temperature Effects Across Models. Finding 4: Temperature is the dominant user-controllable factor**
 413 **affecting variability.** Figure 1 shows the relationship between temperature and output variability for both
 414 models.

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

420 Notably, BERTScore F1 remains above 0.94 across all conditions, even when EMR drops to zero at $t=0.7$ (Table 3).
 421 This indicates that while textual outputs diverge substantially at higher temperatures, their semantic content

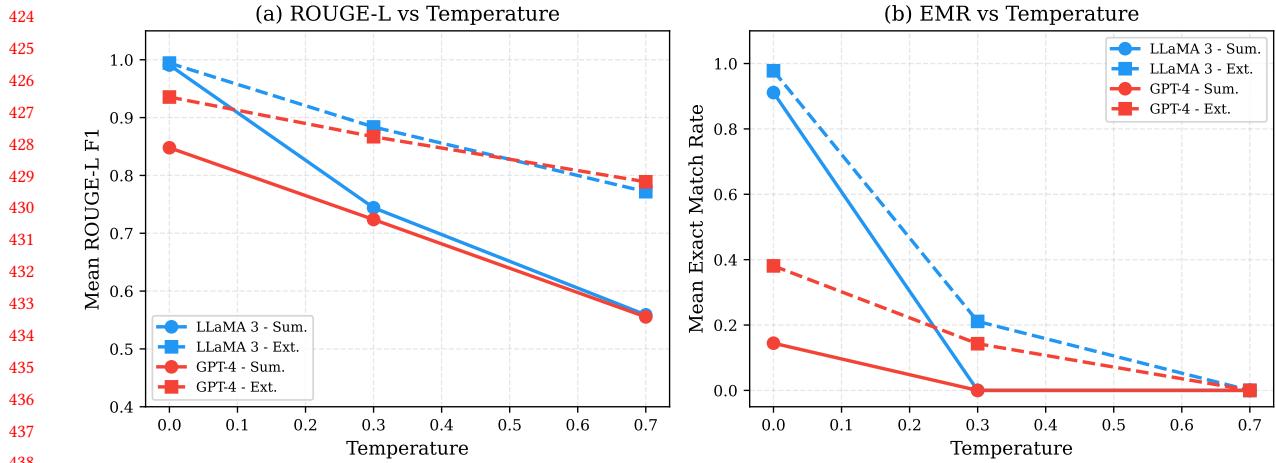


Fig. 1. Effect of temperature on output variability for both models. (a) ROUGE-L F1 decreases monotonically with temperature. (b) Exact Match Rate: LLaMA 3 starts from near-perfect reproducibility at $t=0$, whereas GPT-4 starts from a lower baseline; however, both degrade at comparable rates with increasing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

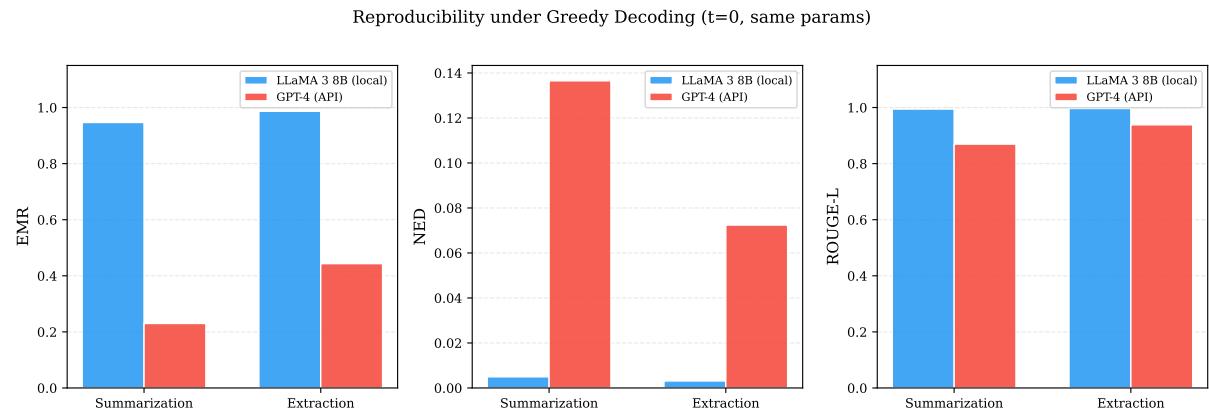


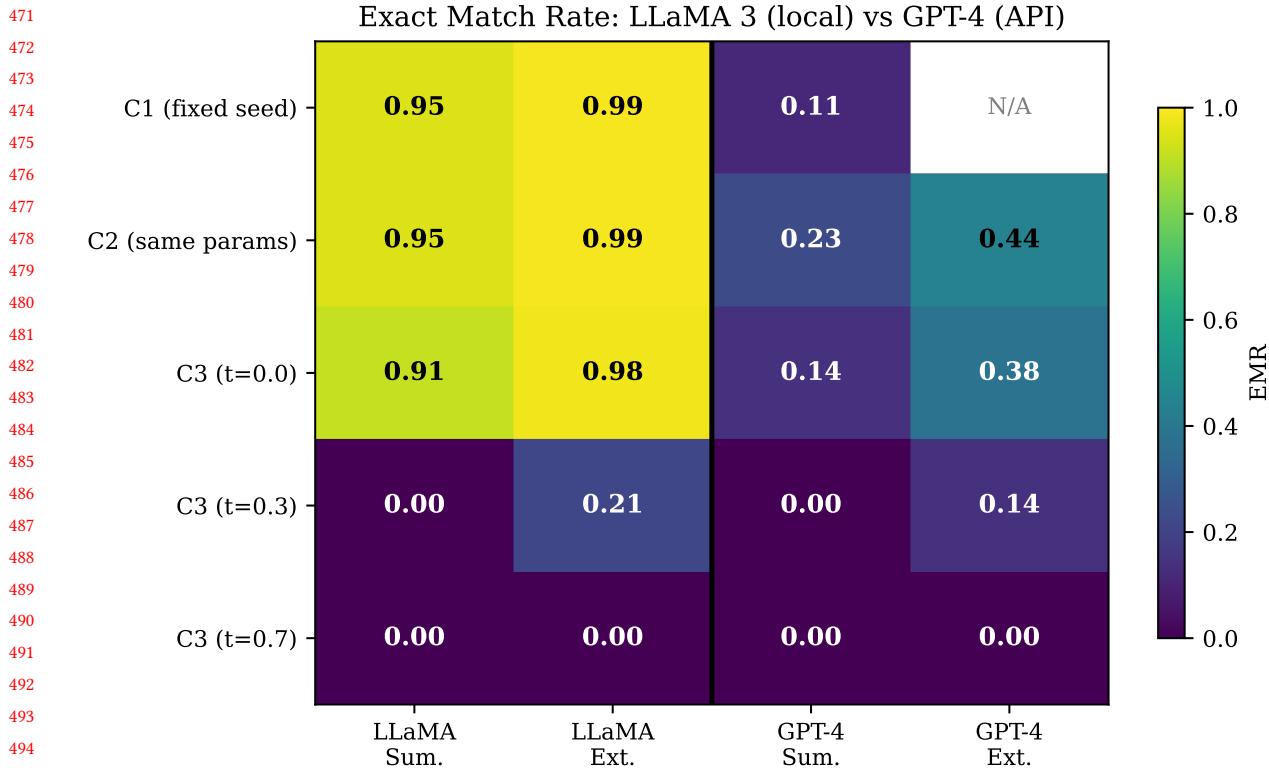
Fig. 2. Reproducibility under greedy decoding ($t=0$), averaged across conditions C1, C2, and C3 at $t=0$: LLaMA 3 8B (local) vs. GPT-4 (API). LLaMA 3 achieves near-perfect to perfect reproducibility, while GPT-4 shows measurable variability across all metrics, particularly for summarization. Condition-specific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

remains highly similar. The gap between surface-level metrics (EMR, NED) and semantic metrics (BERTScore) underscores that non-determinism in LLM outputs is primarily a *phrasing* phenomenon rather than a *meaning* phenomenon—a distinction with important practical implications for downstream applications that tolerate paraphrase variation.

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.



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 To quantify the reproducibility gap between local and API-based inference, we performed paired t -tests on
501 per-abstract EMR values under condition C2 (greedy decoding, $t=0$) across all 30 abstracts. For summarization, the
502 difference is highly significant: $t(29) = 17.250$, $p < 0.0001$, Cohen's $d = 3.149$ (LLaMA 3 mean EMR = 0.947, 95%
503 CI [0.895, 0.998]; GPT-4 mean EMR = 0.230, 95% CI [0.157, 0.303]). For extraction, the gap is equally significant:
504 $t(29) = 8.996$, $p < 0.0001$, Cohen's $d = 1.642$ (LLaMA 3 EMR = 0.987, 95% CI [0.959, 1.000]; GPT-4 EMR = 0.443,
505 95% CI [0.316, 0.571]). Both effect sizes are very large ($d > 1.6$), confirming that the reproducibility difference is
506 not only statistically significant but practically meaningful. All p -values survive Bonferroni correction at the
507 per-family threshold $\alpha = 0.05/6 \approx 0.008$ (six primary comparisons: EMR, NED, and ROUGE-L for each of the two
508 tasks; BERTScore and field-level EMR are reported as secondary exploratory analyses without correction). A post
509 hoc power analysis confirms that $n = 30$ abstracts provides statistical power > 0.999 for all primary comparisons
510 (Cohen 1988).

511 **Robustness check.** Since EMR values are bounded on $[0, 1]$ and the paired differences are not normally
512 distributed (Shapiro-Wilk: $W = 0.894$, $p = 0.006$ for summarization; $W = 0.885$, $p = 0.004$ for extraction), we
513 additionally report non-parametric Wilcoxon signed-rank tests. (We retain paired t -tests as the primary analysis
514 because they are robust to moderate departures from normality at $n = 30$; the non-parametric tests serve as
515 a confirmatory robustness check.) All results remain highly significant: summarization $W = 0.0$, $p < 0.001$;
516

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

extraction $W = 0.0$, $p < 0.001$. The convergence of parametric and non-parametric tests confirms that the reproducibility gap is robust to distributional assumptions.

5.3 Protocol Overhead

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

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

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

6.1 Implications for Reproducibility Practice

Our results yield several actionable recommendations for researchers conducting generative AI experiments:

Use greedy decoding with local models for maximum reproducibility. Under $t=0$ with LLaMA 3 (local), extraction achieved 98.7% EMR and summarization reached 94.7% EMR across 30 abstracts. This configuration should be the default for any study in which output consistency is critical.

Be aware of API non-determinism. Our most consequential finding is that GPT-4, even with $t=0$ and a fixed seed, produces substantially variable outputs (EMR = 0.230 for summarization, 0.443 for extraction). Researchers

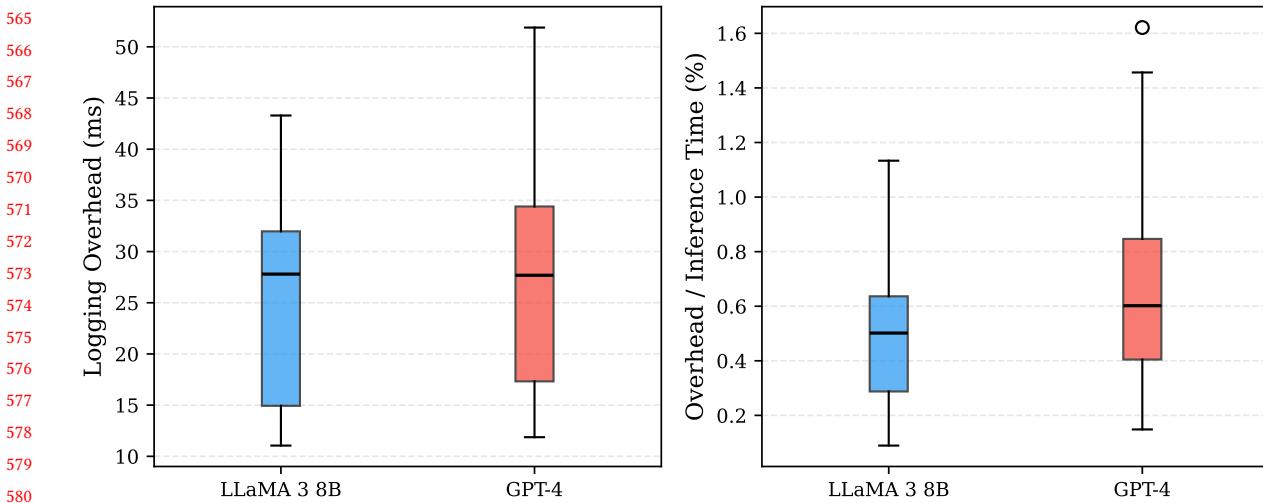


Fig. 4. Distribution of protocol overhead by model. Left: Absolute logging time (ms). Right: Overhead as a percentage of inference time. Overhead is comparable between local (LLaMA 3) and API (GPT-4) inference, consistently below 1.7%.

using API-based models should *never assume reproducibility* without verification, and should report multiple runs with variability metrics.

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

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

Log comprehensively; the cost is negligible. At 0.545% overhead and approximately 4 KB per run, there is no practical reason not to apply comprehensive logging. The cost of not logging—namely, the inability to detect the kind of API non-determinism documented herein—far exceeds the protocol's minimal requirements. For venues implementing reproducibility badges (Gundersen, Helmert, et al. 2024), Run Cards and PROV-JSON artifacts could serve as verifiable evidence of logging compliance for generative AI studies.

6.2 Local vs. API Inference: A Reproducibility Gap

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

This gap has profound implications for the scientific use of API-based LLMs. *Without systematic logging, a researcher using GPT-4 would have no way of knowing that their “deterministic” experiment produces different outputs across runs.* Since our experimental design controls all user-side parameters (temperature, seed, prompt, input),

612 the observed variability is consistent with opaque server-side factors as the primary source. A supplementary
 613 control experiment (Appendix E) further confirms that the prompt-format difference between the two deployment
 614 paradigms does not explain this gap. Our protocol makes this hidden non-determinism visible, measurable, and
 615 documentable.

616

617 6.3 Task-Dependent Reproducibility

618 The difference between summarization and extraction reproducibility under identical conditions—observed
 619 consistently across both models—is, to our knowledge, the first empirical quantification of how task structure
 620 affects LLM output reproducibility. This finding suggests a spectrum ranging from highly constrained tasks (struc-
 621 tured extraction, classification) to open-ended tasks (summarization, dialogue), with the degree of output-space
 622 constraint serving as a primary determinant. Notably, even GPT-4’s extraction task ($\text{EMR} = 0.443$) substantially
 623 outperforms its summarization task ($\text{EMR} = 0.230$), confirming that this effect is not specific to any single model.
 624

625

626 6.4 The Role of Provenance

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

- 628 (1) **Automated comparison:** By comparing PROV graphs of two runs, one can automatically identify which
 629 factors differed (e.g., same prompt and model but different temperatures), enabling systematic diagnosis of
 630 non-reproducibility.
- 631 (2) **Lineage tracking:** When outputs are used as inputs to downstream processes (e.g., summarization
 632 outputs fed into a meta-analysis), the provenance chain can be extended to trace any final result back to
 633 its full generation context.
- 634 (3) **Compliance:** For regulated domains (healthcare, legal, finance), PROV documents provide the formal
 635 evidence trail required by audit standards (National Institute of Standards and Technology 2023) and
 636 emerging regulations such as the EU AI Act (European Parliament and Council of the European Union
 637 2024).

638 To illustrate the diagnostic power of PROV graphs, consider two GPT-4 extraction runs on the same abstract
 639 under condition C2 (greedy decoding, $t=0$, same seed). Although the PROV entities for Prompt, InputText,
 640 ModelVersion, and InferenceParameters are identical (verified via matching SHA-256 hashes), the Output entities
 641 differ: `output_hash` values diverge, and the `wasGeneratedBy` timestamps differ by several seconds. The PROV
 642 graph thus automatically pinpoints the source of non-reproducibility: the only varying factor is the RunGeneration
 643 activity itself, consistent with non-determinism arising from server-side factors. This kind of automated differential
 644 diagnosis is infeasible without structured provenance records.
 645

646

647 6.5 Limitations

648 We organize threats to validity following standard categories:

649 *6.5.1 Internal Validity. Sample size and statistical power.* With $n = 30$ abstracts per condition, our study has
 650 adequate statistical power for the primary comparisons. A post hoc power analysis using the observed effect
 651 sizes ($d > 1.6$) and $\alpha = 0.05$ yields power > 0.999 for all primary comparisons (Cohen 1988). However, for one
 652 secondary comparison (extraction EMR under $C3_{t=0.3}$, $d = 0.207$), power is low (0.084), meaning that subtler
 653 effects may go undetected in some conditions.

654

GPT-4 C3 incomplete coverage. Due to API quota exhaustion, GPT-4 extraction under C3 conditions covers
 655 14–17 of 30 abstracts (summarization C3 is complete). The missing abstracts are those that would have been
 656 processed last in execution order; since abstracts were processed sequentially by ID, this missingness is effectively
 657 independent of abstract content or difficulty. Importantly, our central claims rest on the C2 condition (300/300
 658

659 runs complete for GPT-4), and the C3 temperature sweep serves as a secondary analysis. Restricting the C3
 660 analysis to the 14 fully-complete abstracts does not change the direction or significance of any reported trend. A
 661 post hoc comparison confirmed that missingness was content-independent: abstracts with full coverage ($n = 9$)
 662 did not differ from those with partial coverage ($n = 21$) in word count (Mann–Whitney $U = 105.5$, $p = 0.635$,
 663 $d = 0.23$) or character count ($U = 106.0$, $p = 0.619$, $d = 0.25$), and abstract processing order was uncorrelated
 664 with length (Spearman $\rho = -0.184$, $p = 0.331$).

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

669 **Prompt format confound.** LLaMA 3 was queried via Ollama’s /api/generate endpoint (raw completion),
 670 whereas GPT-4 was queried via the OpenAI Chat Completions API (structured messages with system/user roles).
 671 This difference in prompt format is inherent to the deployment paradigms under study and mirrors real-world
 672 usage. To assess whether this confound explains the reproducibility gap, we conducted a supplementary control
 673 experiment running LLaMA 3 via Ollama’s /api/chat endpoint (chat template matching GPT-4’s message
 674 structure) on 10 abstracts under conditions C1 and C2 (200 runs). The results (Appendix E) show that LLaMA 3
 675 maintains near-identical reproducibility under the chat format, confirming that the prompt-format difference
 676 does not account for the observed local-vs-API gap.

677 **6.5.2 External Validity. Two models.** Our evaluation covers LLaMA 3 8B (local) and GPT-4 (API), representing
 678 two deployment paradigms but only one model per category. Other models—including Claude (Anthropic
 679 2024), Gemini (Gemini Team et al. 2024), Mixtral, and larger or smaller LLaMA variants—may exhibit different
 680 reproducibility characteristics. Our findings about the local-vs-API gap should therefore be interpreted as a case
 681 study of this paradigm difference rather than a universal claim. The protocol itself is model-agnostic, and we
 682 note that gpt-4-0613 is now a legacy snapshot; the very fact that newer model versions may behave differently
 683 illustrates exactly the kind of silent evolution that our protocol is designed to detect and document.
 684

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

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

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

693 **6.5.3 Construct Validity. Surface-level metrics.** Our metrics (EMR, NED, ROUGE-L) capture textual rather
 694 than semantic similarity. Two outputs that are semantically equivalent but syntactically different will register
 695 as non-matching under EMR and partially divergent under NED. This is by design—our focus is on *exact*
 696 reproducibility—but it means our results may overstate the practical impact of non-determinism for downstream
 697 applications where semantic equivalence suffices.

698 **6.5.4 Other Considerations. Privacy.** The protocol’s environment metadata includes the machine hostname,
 699 which may reveal institutional information. Deployments in privacy-sensitive settings should anonymize this
 700 field.

701 **Computational cost.** The total cost was modest: ~ 2 GPU-hours on a consumer laptop (Apple M4, 24 GB) for
 702 1,140 LLaMA 3 runs, plus 724 API calls to GPT-4. The carbon footprint is negligible at this scale, and the logging
 703 overhead (25 ms per run) would not materially increase energy consumption even at thousands of runs.

706 6.6 Protocol Minimality: An Ablation Analysis

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

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

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

724 6.7 Practical Costs and Adoption

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

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

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

739 7 Conclusion

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

- 745 (1) **Local inference is substantially more reproducible than API-based inference.** Under identical
 746 greedy decoding settings, LLaMA 3 achieves EMR = 0.987 for extraction while GPT-4 achieves only 0.443,
 747 consistent with substantial server-side non-determinism that is invisible without systematic logging
 748 (paired t -test: $p < 0.0001$, Cohen’s $d > 1.6$).
- 749 (2) **Task structure is a primary determinant of reproducibility.** Structured extraction consistently
 750 outperforms open-ended summarization across both models, with the JSON format constraint reducing
 751 the model’s output space.

- 753 (3) **Temperature is the dominant user-controllable factor.** Within the C3 temperature sweep, increasing
 754 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
 755 (GPT-4 extraction), while seed variation has no measurable effect under greedy decoding for local models.
 756 (4) **Comprehensive provenance logging adds negligible overhead:** 0.545% of inference time and approx-
 757 imately 4 KB per run, thereby removing any practical argument against systematic documentation.

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

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

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

770 Acknowledgments

771 This work was supported by UTFPR – Universidade Tecnológica Federal do Paraná. The experiments were
 772 conducted using locally deployed open-weight models to ensure full reproducibility of the computational envi-
 773 ronment.

775 Data Availability Statement

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

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

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

783 Author Contributions

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

789 Conflict of Interest

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

794 Use of AI-Assisted Tools

795 The authors used AI-assisted tools (Claude, Anthropic) during the preparation of this manuscript for language
 796 editing, code development support, and data analysis scripting. All AI-generated content was critically reviewed,
 797 validated, and revised by the authors, who take full responsibility for the accuracy and integrity of the final
 798

800 manuscript. The scientific design, experimental execution, interpretation of results, and intellectual contributions
 801 are entirely the authors' own work.

802

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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;
 1037 *Field EMR* = within-abstract pairwise exact match across runs for each extracted field, averaged over abstracts (see Section D
 1038 for interpretation). LLaMA 3 always prepends introductory text (e.g., “Here is the extracted information in JSON format.”),
 1039 yielding 0% raw validity but near-perfect extracted validity at $t=0$.

Model	Cond.	Raw	Extr.	Schema	Within-Abstract Field EMR					Overall	
					Valid	Valid	Compl.	obj	meth	key_r	Field EMR
								mod/sys	bench		
LLaMA 3	C1 ($t=0$)	0%	100%	100%	0.987	0.987	0.987	1.000	0.987	0.989	
	C2 ($t=0$)	0%	100%	100%	0.987	0.987	0.987	1.000	0.987	0.989	
	C3 ($t=0.0$)	0%	100%	100%	0.978	0.978	0.978	1.000	0.978	0.982	
	C3 ($t=0.3$)	0%	97.8%	97.8%	0.747	0.460	0.552	0.862	0.805	0.685	
	C3 ($t=0.7$)	0%	92.2%	92.2%	0.522	0.167	0.267	0.611	0.711	0.456	
GPT-4	C2 ($t=0$)	100%	100%	100%	0.773	0.667	0.637	0.893	0.863	0.767	
	C3 ($t=0.0$)	100%	100%	100%	0.833	0.571	0.667	0.905	0.810	0.757	
	C3 ($t=0.3$)	100%	100%	100%	0.405	0.262	0.452	0.762	0.690	0.514	
	C3 ($t=0.7$)	100%	100%	100%	0.137	0.157	0.255	0.667	0.725	0.388	

1053 Second, within-abstract field-level exact match rates—computed by comparing only runs of the *same* abstract
 1054 under the same condition, then averaging across abstracts—confirm the overall reproducibility hierarchy. Under
 1055 greedy decoding, LLaMA 3 achieves near-perfect field EMR (0.982–0.989 overall), with all five fields at or above
 1056 0.978, consistent with the overall extraction EMR of 0.987 reported in Table 3. GPT-4 under greedy shows
 1057 lower field EMR (0.757–0.767 overall), with open-ended fields (method: 0.667, key_result: 0.637) lagging behind
 1058 structured fields (model_or_system: 0.893, benchmark: 0.863). As temperature increases, this gap widens: at
 1059 $t=0.7$, method drops to 0.167 (LLaMA) and 0.157 (GPT-4), while benchmark retains 0.711 and 0.725 respectively—a
 1060 4–5× difference. This within-abstract formulation isolates true reproducibility (same input, same conditions,
 1061 different runs) from between-abstract content variation, providing a methodologically clean measure of field-level
 1062 consistency.

1064 E Chat-Format Control Experiment

1065 To assess whether the prompt-format difference between LLaMA 3 (completion-style via /api/generate) and
 1066 GPT-4 (chat-style via Chat Completions) contributes to the observed reproducibility gap, we conducted a sup-
 1067 plementary control experiment running LLaMA 3 8B through Ollama’s /api/chat endpoint, which applies the
 1068 model’s chat template (including special tokens for system/user/assistant roles) in the same message structure
 1069 used by GPT-4.

1070 **Design:** 10 abstracts \times 2 tasks \times 2 conditions (C1, C2) \times 5 repetitions = 200 runs, all under greedy decoding
 1071 ($t=0$).

1072 **Results:** Table 7 compares the chat-format control with the original completion-format results for the same 10
 1073 abstracts. The two prompt formats produce *identical* variability metrics across all conditions: summarization
 1074 EMR = 0.929, NED = 0.0066, and ROUGE-L = 0.9922 in both modes; extraction achieves perfect reproducibility
 1075 (EMR = 1.000) regardless of interface. The 0.929 summarization EMR reflects the warm-up effect on 2 of 10
 1076 abstracts—the same pattern observed in the full 30-abstract experiment. These results confirm that prompt
 1077 format is not a source of variability, and the reproducibility gap between LLaMA 3 and GPT-4 is consistent
 1078 with deployment-side factors (server infrastructure, floating-point non-determinism across GPU types, request
 1079 batching) rather than prompt-format differences.

1082 Table 7. Prompt-format control: LLaMA 3 8B via completion (/api/generate) vs. chat (/api/chat) for 10 abstracts under
 1083 greedy decoding ($t=0$). EMR computed over conditions C1 and C2 combined.

1084	Task	Metric	Completion	Chat
1085	Summarization	EMR \uparrow	0.929	0.929
1086		NED \downarrow	0.0066	0.0066
1087		ROUGE-L \uparrow	0.9922	0.9922
1088	Extraction	EMR \uparrow	1.000	1.000
1089		NED \downarrow	0.0000	0.0000
1090		ROUGE-L \uparrow	1.0000	1.0000

1091 Note: Completion and chat formats yield identical metrics for all 10 abstracts under greedy decoding, confirming prompt format is not a
 1092 source of variability.

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