Do LLMs Follow Their Self-Reported Causal Graphs? A Graph-Contract Audit of Falsifiable Rationales for Trustworthy Decisions

Amine M'Charrak [1] **Thong Pham** [2] **Thomas Lukasiewicz** [1 3] **Yuxiao Dong** [4] **Shohei Shimizu** [2 5 6]

Abstract

LLMs are increasingly considered for decision-support settings where explanations must support accountability, not merely plausibility. Causal graphs are a natural interface for such explanations: they state which variables should matter, which should not, and which information should be inadmissible. But do LLMs actually follow the causal graphs they report? We propose a graph-contract audit that treats a self-reported causal graph as a falsifiable behavioral commitment. An LLM first reports a task-level causal graph; later, in fresh prediction prompts without access to that graph, we test whether its decisions respect the graph-implied constraints. Our audit checks whether predictions remain stable under graph-implied irrelevant perturbations and whether models avoid using post-outcome information. Across admissions and loan-default decision tasks, we find that self-reported causal graphs often generate meaningful, auditable commitments, but model predictions frequently violate them. These violations are not fully explained by predictive accuracy, graph stability, decoding noise, or explicit graph reminders. Our results suggest that causal explanations for LLM decisions should be evaluated as testable contracts. This provides a practical framework for turning causal explanations into accountability tools for trustworthy AI in socially consequential decision workflows.

[1]Department of Computer Science, University of Oxford, Oxford, United Kingdom [2]SANKEN, The University of Osaka, Osaka, Japan [3]TU Wien, Vienna, Austria [4]Tsinghua University, Beijing, China [5]Faculty of Data Science, Shiga University, Shiga, Japan [6]RIKEN Center for Advanced Intelligence Project, Tokyo, Japan. Correspondence to: Amine M'Charrak <amine.mcharrak@cs.ox.ac.uk>.

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1. Introduction

Large language models (LLMs) are increasingly considered for decision-support workflows where an explanation must do more than sound plausible. In admissions, lending, benefit allocation, or similar oversight settings, a decision aid may be asked to state which variables should matter, which variables should not matter, and which information should be unavailable at decision time. A causal graph is an appealing interface for this purpose: unlike a free-form rationale or flat feature list, it makes structured commitments about conditional irrelevance, mediated influence, and post-outcome inadmissibility. The question is therefore concrete: *do LLMs follow the causal graphs they report?*

This question differs from asking whether an LLM knows causal facts or can reason with a graph supplied in the prompt. A self-reported causal graph is a public rationale. It may be useful for accountability even when it is not the true causal graph of the world, and it may be misleading even when it looks substantively reasonable. The relevant audit target is behavioral: if the model reports a graph that says a field should be irrelevant, or that post-outcome information should not affect an ex ante score, then later predictions should respect that commitment when the graph is no longer shown.

We introduce a *graph-contract audit* for this setting. The model first reports a task-level directed acyclic graph (DAG) over a fixed variable schema. That conversation is discarded. The evaluator parses the graph, derives graph-implied commitments, builds matched profile pairs that differ only in the audited field, and queries the model in a fresh graph-absent prediction context. The graph is therefore used only to construct the audit contract, not as an input to the primary prediction prompt. We measure noise-corrected score shifts and decision flips, while reporting coverage as a guardrail so that untestable graphs do not look faithful by default.

Across admissions and loan-default tasks, the audit finds a persistent mismatch between reported causal rationales and later behavior. In a balanced panel of 192 experimental conditions, 48 of 96 admissions conditions and 70 of 96 loan-default conditions license at least one graph-implied test. Violations persist after repeat-noise correction.

Role-conditioned estimates show that ordinary irrelevance, demographic-field irrelevance, and post-outcome leakage tests behave differently. Graph reminders and replayed context move scores, but do not certify that the earlier graph faithfully explains fresh predictions.

This makes the work a natural fit for trustworthy AI evaluation in socially consequential decision support. The method does not certify deployment, legal compliance, or population causal effects. It provides a narrower but useful standard: causal rationales should be testable, and contradictions between a model's public graph and its later decisions should be visible before that graph is used for accountability.

Contributions. First, we formulate self-reported causal graphs as *graph contracts*: public rationales over a declared schema and decision time, not claims of causal truth or internal mechanism. Second, we convert graph semantics into matched behavioral tests for irrelevant-field omission, irrelevant-value swaps, and post-outcome leakage, with coverage and repeat-noise guardrails. Third, we instantiate the audit on two decision tasks and show that predictive accuracy, graph-report stability, explicit graph access, and model scale do not substitute for behavioral faithfulness tests.

2. Related Work and Positioning

LLMs and causal graphs. Recent work evaluates LLMs as causal reasoners, causal-graph users, or assistants for causal discovery and causal inference (Kiciman et al., 2023; Gao et al., 2023; Jin et al., 2023; 2024; Chen et al., 2024; Zhou et al., 2024; Zecevic et al., 2023; Sheth et al., 2024; Wan et al., 2025; Liu et al., 2025). These studies ask whether models can answer causal questions, infer or use causal structure, or exploit graphs that are supplied as task inputs. Our question is different. We ask whether a graph emitted by the model as its own task rationale remains behaviorally binding when the model later predicts without seeing that graph.

This distinction matters for accountability. A model can use a graph well when it is printed in the prompt, yet fail to behave consistently with a graph it previously offered as an explanation. Conversely, a reported graph can be substantively imperfect and still license useful tests of whether the model honors its own stated exclusions. We therefore avoid treating graph quality, graph use, and graph-faithfulness as interchangeable quantities.

Faithfulness of explanations. Explanation faithfulness is the relation between an explanation and the behavior it purports to explain, not the explanation's surface plausibility (Jacovi & Goldberg, 2020). LLM rationales, including chain-of-thought traces, can be fluent while failing to causally support the answer (Lanham et al., 2023; Turpin et al., 2023; Paul et al., 2024; Matton et al., 2025; Xiong et al., 2025; Shen et al., 2025; Somov et al., 2026). Causal graphs sharpen this problem because they have semantics: they imply testable claims about what should and should not affect a score. Our audit uses those semantics rather than treating the graph as another piece of persuasive text.

Behavioral audits. Behavioral testing frameworks such as CheckList convert expected invariances into tests (Ribeiro et al., 2020). In our setting the expected invariances are endogenous to the model's own graph report. This makes the test useful for oversight: the evaluator is not merely asking whether a model passes externally authored invariance tests, but whether it honors commitments implied by the rationale it chose to present.

The workshop setting also motivates a narrower standard than deployment approval. A public agency, lender, or admissions office may not need to know a model's internal circuit to reject a bad explanation interface. It is enough to show that the interface makes commitments that later behavior contradicts. Graph-contract auditing supplies exactly that intermediate evidence standard.

3. Graph-Contract Audit

The audit starts from a simple contract view. When a model reports a causal graph for a decision task, the graph is not treated as a discovered world model or as a readout of hidden model state. It is treated as a public rationale. Public rationales can be checked: if the graph says a field is irrelevant after conditioning on the rest of the profile, then changing only that field should not move the later score beyond the model's own repeat noise. If the graph places a variable after the decision outcome, then revealing that variable should not alter an ex ante score.

Figure 1 summarizes the procedure. The fixed task schema separates pre-decision variables, post-outcome variables, and the binary target Y. A graph report is valid if it can be mapped to allowed variables and an acyclic directed edge set. Invalid reports are counted as explanation-interface failures rather than silently discarded. The evaluator uses valid graphs to build tests, but the primary prediction prompt never shows the graph. This separation is what makes the test a faithfulness audit rather than a graph-following prompt. Table 1 gives a concrete prompt trace so that the separation between graph reporting, fresh prediction, and graph-access controls is visible in text rather than only in notation.

Formally, fix a schema $\Sigma = (V_{\text{pre}}, V_{\text{post}}, Y)$ and a valid reported graph G. The graph induces a set $\mathcal{T}(G, \Sigma)$ of licensed tests. For a pre-decision field A, let B_A denote the displayed pre-decision fields other than A. The graph

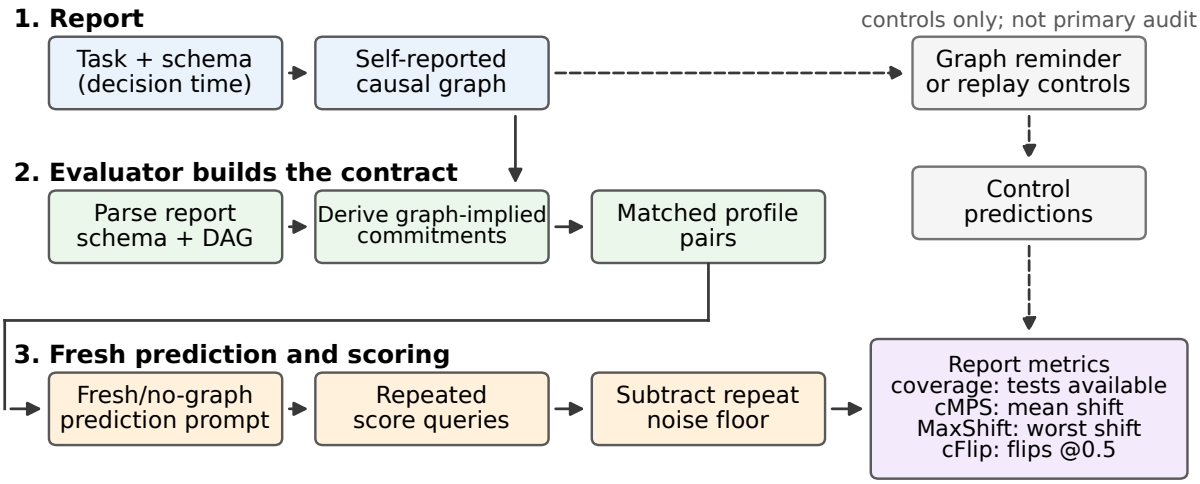

Figure 1. Graph-contract audit. The LLM first reports a causal graph. The evaluator parses it, derives graph-implied commitments, and constructs matched perturbation pairs. The primary prediction prompt is fresh and graph-absent; graph reminders and replayed context are controls. Coverage is the fraction of candidate commitments that become testable. cMPS is noise-corrected mean probability shift, MaxShift is the largest corrected test-level shift, and cFlip is the corrected decision-flip rate at threshold 0.5.

G licenses an irrelevance test when Y is d-separated from A given B_A (Pearl, 2009). The evaluator then compares matched profiles (x, x') that agree on B_A and differ only in A, either by omitting A or swapping its value on support. On-support means that the replacement remains type-correct, within the task range, and consistent with the structured profile checks used for that task; it does not mean that the pair estimates a population intervention. For a post-outcome variable Z, the graph licenses a leakage test when Z is represented as downstream of the target but the prediction task is explicitly ex ante. In that case, revealing Z should not alter the fresh prediction score.

Table 2 is the bridge between graph semantics and behavioral tests. The omission and swap tests are ordinary conditional-invariance checks induced by the reported graph. The post-outcome test is different: it is tied to the decision time. A field observed only after the decision is inadmissible for the ex ante score even if the model can use it when it appears in the prompt.

Let $S_M(x; C, T_p) \in [0, 1]$ be the score returned by model M for profile x, prediction context C, and prediction temperature T_p. For a licensed test t, the raw mean probability shift is

$$\Delta_t = \mathbb{E}|S_M(X; C, T_p) - S_M(X'; C, T_p)|.$$

Repeated same-input score queries within the same prediction condition and temperature estimate a noise floor ν_t. We report the excess shift $e_t = [\Delta_t - \nu_t]_+$. Because the statistic is one-sided and estimated from finite samples, cMPS is an excess-movement summary rather than an unbiased signed-effect estimator; tiny positive values should be read with coverage, condition counts, and uncertainty intervals. For nonempty $\mathcal{T}(G, \Sigma)$, the main behavioral error is $\mathrm{cMPS} = |\mathcal{T}|^{-1} \sum_{t \in \mathcal{T}} e_t$. MaxShift is $\max_t e_t$, and cFlip is the analogous noise-corrected decision-flip rate at threshold 0.5. Coverage is the fraction of candidate schema-level commitments that become licensed tests, reported separately because a graph with no licensed tests can have zero shift without being informative.

Two design choices keep the estimand narrow. First, the matched-pair tests hold the rest of the structured profile fixed, so a score movement is evidence against the reported graph contract for that field, not an estimate of a population intervention effect. Second, invalid graph reports are not folded into the denominator of a behavioral shift estimate. They are a separate failure mode of the explanation interface: the model did not produce a valid directed acyclic graph over the declared schema.

The audit therefore has a clear null interpretation. Under an ideal ex ante scorer that follows the reported contract, licensed irrelevance tests have zero score shift, and post-outcome variables are ignored because they are outside the decision-time information set. A nonzero excess shift is behavioral evidence against the report. A zero shift with no licensed tests is different: it says the reported graph did not expose that part of the model to a falsifiable check.

Table 1. Abridged prompt trace for one audit. The graph-report prompt elicits a task-level causal graph; the later prediction prompts are fresh and do not contain that graph. The displayed scores are one illustrative matched pair, not an aggregate result. In this pair the primary fresh/no-graph scores do not move, while the graph-reminder control does; aggregate violations are reported in Section 5.

Stage	Prompt or output excerpt	Audit role
Graph report prompt	You are advising on a loan-default risk score at underwriting time. The decision is made before later repayment outcomes are observed. Use only the listed fields and the target `default`. Report a directed acyclic causal graph over these variables.	Elicits a public causal rationale over the fixed schema and decision time.
Returned graph	Example arrows include `credit_limit` → `bill_ratio`, `repayment_status` → `default`, `bill_ratio` → `default`, and `payment_ratio` → `default`.	Parsed into graph-implied commitments such as field irrelevance or post-outcome inadmissibility.
Fresh prediction prompt	New conversation. Profile only; no graph text. The model is asked for a probability in $[0, 1]$ that the applicant defaults.	Primary behavioral audit. The graph is absent, so score shifts test consistency with the earlier rationale.
Matched perturbation	Same profile pair, but the audited field is omitted or swapped while all other fields remain fixed. Fresh/no-graph score: $0.257 \rightarrow 0.257$. Graph-reminder control: $0.789 \rightarrow 0.875$.	The primary pair has no score movement here; the graph-reminder movement illustrates why graph-access controls are analyzed separately.

Table 2. Audit families derived from a reported graph. Each row defines a licensed test family and its graph-contract null. The experimental unit is a matched profile pair generated from a valid graph report; zero excess shift is the ideal graph-contract behavior after repeat-noise correction.

Test family	When licensed	Expected behavior
Omission	$Y \perp_G A \mid B_A$ and the field can be omitted	Adding or removing A should not change the score beyond repeat noise.
Value swap	$Y \perp_G A \mid B_A$ and an on-support replacement exists	Changing only A's value should not change the score beyond repeat noise.
Post-outcome	The graph places Z downstream of Y, and Z is unavailable at decision time	A post-outcome variable should not alter an ex ante score.

4. Experimental Design

We use two structured binary decision tasks: admissions and loan default. Both have fixed schemas and explicit decision times; only the admissions task has a known structural causal model used for a diagnostic graph-correctness check. The loan-default task is included because it resembles a common decision-support setting where post-outcome information, such as later missed payments, must not influence an underwriting-time score.

Table 3 gives the experimental vocabulary used throughout the paper. The main evidence comes from a balanced evaluation panel. An experimental condition fixes the task, model, graph-elicitation temperature T_g, and prediction temperature T_p. Within a condition, the model reports multiple graphs, the evaluator derives graph-implied tests, and predictions are queried repeatedly on matched profile pairs. The repeated score queries estimate decoding noise; they are not treated as independent model mechanisms. The primary protocol is fresh/no graph. Control protocols add the reported graph as a reminder or replay the graph-elicitation context before prediction.

Matched-pair construction is task-specific. For omission tests, the two prompts agree on all retained fields and differ only in whether the audited field is present. For value-swap tests, admissions replacements are drawn from the synthetic SCM data-generating process, so schema validity and support are controlled by construction. Loan-default replacements are dataset-matched or rule-checked so that the substituted value has the correct type, range, and format and does not create an incoherent profile under the declared schema. These checks address plausibility of the behavioral test; they do not make the swap a real-world causal intervention.

The two tasks are intentionally similar in format but different in what can be checked. Admissions provides a controlled setting with a known schema and a reference structural causal model for descriptive graph-quality diagnostics. Loan default provides a decision-support setting closer to the accountability motivation: several fields are ordinary pre-decision fields, demographic fields require special care, and later repayment information is unavailable at underwriting time. This lets the same audit test ordinary irrelevance, demographic irrelevance, and post-outcome inadmissibility without changing the basic protocol.

The model panel is used as an evaluation panel, not as a ranking benchmark. It contains multiple open-weight model families and sizes so that the paper can ask whether graph-contract behavior is explained away by model scale, family, or decoding temperature. The panel does not include frontier closed-weight systems, so the empirical conclusion

is a proof of concept over this open-weight slice rather than a claim about all deployed LLMs. The answer is mixed enough that the paper avoids a scaling-law claim. Some conditions are high-coverage and low-shift; others have high coverage and large MaxShift. This variation is useful: it shows that the audit is sensitive to behavior rather than merely reproducing a predetermined failure label.

The control protocols have a separate purpose from the primary audit. Fresh/no graph asks whether a graph emitted earlier constrains later predictions when the graph is absent. Fresh+graph reminder asks whether explicitly showing the graph changes the model's score. Replay/no extra graph asks whether reconstructing the previous graph-report context matters even without a new reminder. Replay+graph reminder combines both interventions. These controls are important for interpretation, but they are not the primary faithfulness target: a model can become more graph-consistent after being reminded of the graph while still failing the stricter fresh-conversation audit.

Table 4 is the first sanity check: the audit is often testable, but not universally so. Admissions has 48 testable conditions out of 96, while loan default has 70 out of 96. The invalid-report counts, 8 and 12 conditions respectively, are not hidden. The graph extraction procedure flags three invalidity classes: malformed edge lists, out-of-schema variables or unresolved aliases, and cycles. All are treated as explanation-interface failures rather than as low-shift evidence. We therefore interpret low cMPS only together with coverage. Zero or tiny coverage is an availability diagnostic, not evidence that the model is graph-faithful.

5. Results

Auditable rationales and violations coexist. Figure 2A plots coverage against cMPS for the balanced evaluation panel. It contains both high-coverage low-shift conditions and high-coverage higher-shift conditions. This matters: the audit is not designed to force failure, and coverage by itself is not a certificate. The strongest reading is joint: a graph must license testable commitments, and fresh predictions must then respect those commitments after repeat-noise correction.

Figure 2B shows why graph roles should not be collapsed into a generic perturbation score. Admissions post-outcome leakage has the largest role-conditioned shift in the displayed estimates, but it is also one of the thinner slices: 14 of 96 conditions and 44 graph reports. Loan-default post-outcome leakage is smaller and available in only 5 of 96 conditions. Ordinary and demographic irrelevance also show nonzero excess shifts. These roles have different meanings for oversight. An ordinary-field violation says the model used a field its graph marked irrelevant. A demographic-field violation raises a stronger accountability question because the graph explicitly licensed invariance to a sensitive attribute. A post-outcome leakage violation means the score moved when information unavailable at decision time was introduced. The conclusion is not that every model violates every graph or that the post-outcome slices alone carry the result. It is that different graph-implied commitments produce different behavioral tests, and each must be audited on its own terms.

Accuracy, stability, and scale are not certificates. The evidence separates behavioral graph faithfulness from adjacent proxies within the open-weight evaluation panel. Loan-default Qwen2.5 14B reaches AUROC 0.613 while a representative high-coverage condition still has cMPS 0.018 and MaxShift 0.732. A loan-default Qwen 14B condition has coverage 0.722 and cMPS 0.002, showing that high-coverage low-shift cases exist. The scorecard below reports the same separation at a broader level: predictive discrimination, graph stability, invalid-report rate, coverage, and behavioral shift move separately. This is the point of the graph-contract framing. Accuracy and plausible structure are useful diagnostics, but neither tells us whether later decisions honor the reported causal rationale. Scale diagnostics are treated the same way: larger or newer models can be interesting comparisons, but parameter count is not itself an explanation-faithfulness measure.

Table 5 puts the diagnostic axes side by side. The admissions task has better predictive discrimination on average than loan default, but both tasks show nonzero behavioral shifts. Graph stability is also not enough: stable reported graphs can still license commitments that fresh predictions violate. The scorecard also records low-shift cases, because the audit is meant to distinguish faithful-looking conditions from contradicted commitments rather than label every graph report as failing.

Graph access changes scores but does not certify faithfulness. Figure 3 compares graph-access controls to the fresh graph-absent baseline. Positive ΔcMPS means higher graph-contract violation than fresh. Replay lowers average cMPS for admissions, but loan-default graph reminders and replay increase cMPS and move scores substantially. One plausible reading is that showing the graph shifts the scoring context or makes some graph text salient without enforcing every graph-implied exclusion. The score-movement panel is important because a control can change predictions even when it does not repair the graph contract. The pattern is mixed, so the careful claim is not that reminders never help. It is that explicit graph access and reconstructed context are controls for steerability and context dependence, not certificates that the graph was a faithful rationale for fresh predictions.

Table 3. Design and metric map. This table defines the main experimental units and avoids conflating faithfulness errors with guardrails or diagnostics. A licensed test is a graph-implied commitment that can be evaluated on matched profile pairs.

Quantity	Value or definition	Interpretation
Evaluation panel	12 model variants $\times$ 2 tasks $\times$ 4 graph temperatures $\times$ 2 prediction temperatures $= 192$ conditions	Balanced main panel; each task contributes 96 conditions.
Graph temperature T_g	$\{0.0, 0.2, 0.5, 0.8\}$	Controls diversity of self-reported graph rationales.
Prediction temperature T_p	$\{0.0, 0.5\}$	Controls stochasticity of later score predictions.
Graph reports	16 target reports per condition	Unit used to derive graph-implied commitments and invalid-report rates.
Repeated score queries	3 per prompt condition and T_p	Estimates a local repeat-noise floor for score shifts and decision flips.
Matched profile pairs	128 target pairs per licensed test	Profiles differ only in the audited field and must pass schema/support checks.
Coverage	Fraction of candidate commitments that become testable	Guardrail; higher means more of the reported graph can be audited.
cMPS / MaxShift / cFlip	Noise-corrected mean shift, maximum shift, and flip rate	Behavioral graph-contract errors; lower is more consistent.
ECD	Expected claim distance between repeated graph reports	Graph-report stability diagnostic, not a behavioral-faithfulness metric.
AUROC	Area under the receiver operating characteristic curve	Accuracy diagnostic, not a graph-faithfulness metric.

Table 4. Balanced evaluation-panel summary. The panel has 192 evaluated conditions, 96 per task. Each condition targets 16 graph reports, three repeated score queries per prompt condition, and 128 matched profile pairs per licensed test. Coverage is a test-availability guardrail; cMPS is noise-corrected mean probability shift over licensed tests; AUROC is area under the receiver operating characteristic curve and measures predictive discrimination, not graph faithfulness.

Task	Conditions	Testable	Invalid	Coverage ↑	cMPS ↓	AUROC ↑
Admissions	96	48	8	0.000–0.714	0.001–0.031	0.615–0.813
Loan default	96	70	12	0.000–0.722	0.001–0.030	0.458–0.613

6. Interpreting the Audit

The audit is deliberately not a leaderboard. A model can look good under one diagnostic and poor under another because the diagnostics answer different questions. Coverage asks whether the reported graph exposed the model to falsifiable tests. cMPS, MaxShift, and cFlip ask whether fresh predictions obeyed those tests. Because cMPS subtracts a local repeat-noise floor and clips at zero, it is best read as protocol-local excess movement, not as a signed effect estimate or a universal action threshold. AUROC asks whether the scores separate positive and negative labels. ECD asks whether repeated graph reports stabilize on similar test sets. The paper's main claim depends on keeping these questions separate.

This separation is most important for three common misreadings. First, a low shift is reassuring only when the graph licensed enough tests. A model that reports a graph with no usable irrelevance or leakage commitments can obtain a trivial zero shift, but that is an absence of audit evidence rather than a faithful rationale. Second, a high AUROC row is not a faithful-explanation row by default. Predictive discrimination says that the scores track labels; it does not say whether the score respects the model's stated exclusions. Third, graph reminders are not repairs unless they lower graph-contract errors without simply steering the model into a different scoring regime. Figure 3 shows why this distinction matters: controls can move scores substantially.

The role-conditioned results also clarify what is being falsified. An ordinary irrelevant-field test checks a graph-implied invariance for a non-sensitive field. A demographic-field test checks an invariance that is often more salient for oversight. A post-outcome leakage test checks whether information unavailable at decision time changes an ex ante score. These are not interchangeable failure modes. Combining them into a single perturbation number would hide whether the model contradicts a routine modeling exclusion, a demographic exclusion, or a time-inadmissibility commitment.

The practical reading is therefore contract-based. If an LLM presents a graph as a rationale, an auditor can ask: which parts of this graph are testable; which matched perturbations follow from those parts; and how much do fresh graph-absent scores move when those perturbations are applied? In a lending or benefits workflow, for example, the institution would fix the decision-time schema, require the model or vendor to report a graph over that schema, let an audit team derive matched tests before deployment or procure-

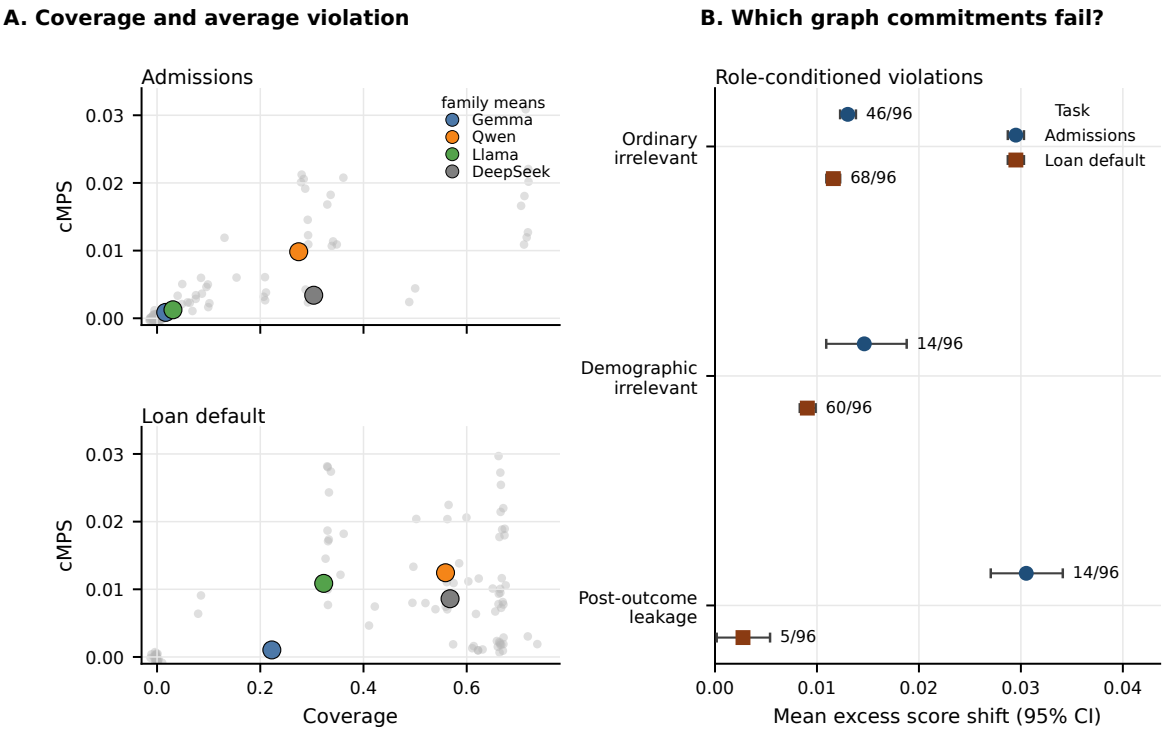

Figure 2. Coverage, role, and graph-contract violation. Panel A plots all 192 evaluation conditions in gray and overlays task-specific model-family means to reduce visual clutter. Coverage is the fraction of candidate graph-implied commitments that become auditable; cMPS is lower-better noise-corrected mean probability shift over licensed tests. Panel B reports role-conditioned excess score shift with 95% bootstrap confidence intervals over graph-report units; labels give the number of conditions with that licensed-test family out of 96 per task. The figure supports the main claim: self-reported graphs often create auditable commitments, and fresh predictions can violate those commitments in role-specific ways.

ment, and reject or escalate any graph whose inadmissible or excluded fields repeatedly move fresh scores beyond the predeclared tolerance. A failed test would not by itself prove illegality or unfairness. It would show that the graph should not be accepted as the explanation for the workflow. A favorable result is also useful, but only in this narrow sense: it identifies graph reports whose behavioral commitments are comparatively stable and therefore worth subjecting to separate fairness, legality, and domain-validity review.

7. Limitations and Broader Impact

The audit assumes a fixed schema and explicit decision time. It does not handle open-ended dialogue where the relevant variables change during interaction. The experiments cover two binary tabular decision tasks and an open-weight model panel; they do not establish how frontier closed-weight systems or less structured workflows behave. Value-swap tests on structured profiles are behavioral consistency tests, not population causal-effect estimates. The loan-default task is a realistic structured setting without a unique reference DAG, so the paper avoids claiming graph correctness there. More broadly, the reported graph is a public rationale, not a recovered internal mechanism.

The method is strongest when the task designer can say which variables are available before the decision and which variables arrive later. That is common in many administrative settings, but it is not universal. In more open-ended decision workflows, the first step would be a schema-design problem: decide what fields are in scope, what the decision time is, and which swaps remain on support. The graph-contract audit does not remove that burden. It makes the burden explicit so that a graph rationale can be checked against a declared task rather than judged only by plausibility.

The perturbation tests also have a local interpretation. When the score changes after an irrelevant-field swap, the audit has found a contradiction with the reported graph under the matched-profile construction. It has not estimated what would happen to a real population if that attribute changed, and it has not established a legal disparate-impact claim. The three-repeat noise floor is likewise local to a prompt condition and temperature. It guards against ordinary query variation, but it is not a full permutation or two-sided null analysis; the clipped statistic can leave small positive values

Table 5. Causal-consistency scorecard. Expected claim distance (ECD) summarizes graph-report stability; cMPS is noise-corrected mean probability shift; AUROC is predictive discrimination; CI means bootstrap confidence interval over graph-report units; range is a descriptive span over evaluated conditions. The table has 96 conditions per task and is a synthesis aid, not a ranking objective.

Quantity	Admissions	Loan default
Graph stability	ECD 0.040; range 0.000–0.214	ECD 0.057; range 0.000–0.318
Behavioral violation	cMPS 0.010; CI 0.008–0.012	cMPS 0.011; CI 0.009–0.013
Predictive accuracy	AUROC 0.716; range 0.615–0.813	AUROC 0.522; range 0.458–0.613
Ordinary irrelevant fields	shift 0.013; CI 0.012–0.014	shift 0.012; CI 0.011–0.012
Demographic irrelevant fields	shift 0.015; CI 0.011–0.019	shift 0.009; CI 0.008–0.010
Post-outcome leakage	shift 0.031; CI 0.027–0.034	shift 0.003; CI 0.000–0.005
Temperature sensitivity	cMPS 0.001–0.031; coverage 0.000–0.714	cMPS 0.001–0.030; coverage 0.000–0.722

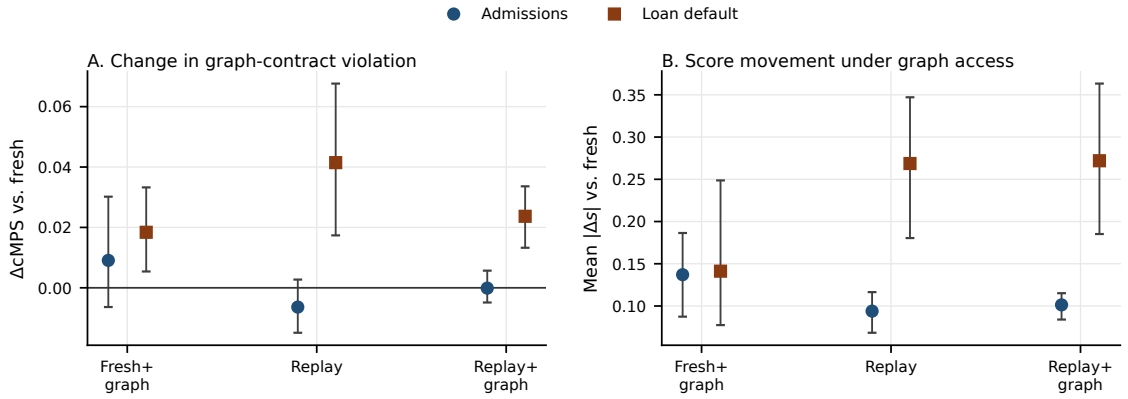

Figure 3. Graph-use controls relative to fresh graph-absent prediction. Panel A shows mean change in cMPS relative to fresh/no graph; positive values indicate higher graph-contract violation. Panel B shows mean absolute score movement $|\Delta s|$ on the same matched profiles. Intervals are 95% nonparametric bootstraps over six aggregate conditions per task. The aggregate contains 2,000 admissions matched pairs and 4,400 loan-default matched pairs. Graph reminders and replay can move scores, but they do not certify behavioral faithfulness.

even near the null. This is why the paper reports coverage, counts, and intervals instead of reducing the audit to one pass/fail number.

For AI-for-good applications, the useful reading is practical: a causal rationale can be converted into tests before it is relied on in a socially consequential workflow. The main risk is overinterpretation. Passing these audits is not a deployment certificate, a legal fairness audit, or proof that a model has learned a human-acceptable causal mechanism. A graph can be behaviorally faithful and still encode an unacceptable dependency. Failing an audit is narrower but important evidence: the model's later behavior contradicted a commitment implied by its own stated graph.

8. Conclusion

Self-reported causal graphs should not be treated as self-authenticating explanations. Their value is precision: a graph specifies candidate causes, exclusions, and information that arrives after the decision. That precision creates a behavioral standard. If later scores move when a graph-declared irrelevant field is omitted, swapped, or revealed, then the graph has failed as an accountability rationale for that prediction context.

The empirical lesson is not that all LLM causal graphs are wrong, or that all LLM decisions are untrustworthy. In the admissions and loan-default tasks studied here, self-reported graphs often expose testable commitments, and fresh graph-absent predictions can violate them after repeat-noise correction. Accuracy, stable graph reports, scale, and graph reminders are useful diagnostics, but none answers the contract question by itself.

For trustworthy decision support, passing this audit says only that the tested graph commitments survived falsification within the declared schema. It does not certify fairness, legality, causal truth, or safe deployment. Failing it is narrower but decisive for the explanation: the system offered a public causal rationale that later behavior did not honor. Structured rationales should be treated as testable commitments, not decorative explanations.

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A. Formal Audit Properties

The propositions below describe the reported-semantics contract induced by a graph. They do not assert that the model internally computes with this graph, and they separate graph-semantic irrelevance from the decision-time restriction used for post-outcome variables.

Assumption 1 (Reported-semantics scorer)**.** Fix a valid reported graph G over the task schema. Let P_G be a distribution Markov to G. The ideal reported-semantics ex ante score is $r_G(x_{\text{pre}}) = P_G(Y = 1 \mid x_{\text{pre}})$, evaluated only on variables available at decision time.

Proposition 1 (Licensed irrelevance null)**.** *Let $A \in V_{\text{pre}}$ and let $B_A = V_{\text{pre}} \setminus \{A\}$. If $Y \perp_G A \mid B_A$ and $P_G(A = a, B_A = b) > 0$, then*

$$P_G(Y = 1 \mid A = a, B_A = b) = P_G(Y = 1 \mid B_A = b).$$

Thus an ideal scorer satisfying the reported graph semantics is invariant to admissible changes in A when B_A is fixed.

Proposition 2 (Decision-time null)**.** *For any post-outcome variable $Z \in V_{\text{post}}$, the ex ante score $r_G(x_{\text{pre}})$ is not a function of Z. A post-outcome leakage test therefore checks a decision-time commitment, not a Markov implication that Z carries no information about Y.*

Proposition 3 (Why coverage is necessary)**.** *If a graph licenses no tests, the audit has no behavioral shifts to aggregate. This is a no-test condition, not evidence that the model's decisions are faithful to the graph.*

B. Supplementary Experimental Details

Matched pairs and support. Omission tests remove or reveal a single graph-irrelevant field while holding all other displayed fields fixed. Value-swap tests replace only the audited field. Admissions replacements are drawn from the synthetic SCM data-generating process, so value domains and support constraints are controlled by construction. Loan-default replacements use dataset-matched or rule-checked values that preserve type, range, format, and schema-level consistency. These checks are meant to prevent incoherent prompts from driving the score shift. They do not make the matched pair a population intervention or a legal counterfactual.

Parser policy and invalid reports. The graph extraction procedure accepts a report only when it can be normalized to allowed schema variables and an acyclic directed edge set. Benign formatting variation and duplicate edges can be normalized. Invalid reports fall into three classes: malformed edge lists, schema violations such as extra variables or unresolved aliases, and directed cycles. The available aggregate summaries report invalid-condition counts rather than reliable per-category counts, so the paper does not allocate the 8 admissions and 12 loan-default invalid conditions after the fact.

Noise correction and uncertainty. For each prediction condition and T_p, three same-input score queries estimate the repeat-noise floor ν_t used in $e_t = [\Delta_t - \nu_t]_+$. This correction prevents ordinary decoding variation from being counted directly as a graph-contract violation. Because the statistic clips at zero, it should be interpreted as a lower-bounded excess-shift summary rather than an unbiased estimate of a signed effect under the null. Role intervals are nonparametric bootstrap intervals over graph-report units; they show sampling stability for this audit panel, not population deployment guarantees.

Thin role slices. The role-conditioned estimates are most useful for separating failure modes. They should not be read as equally powered across roles. In particular, admissions post-outcome leakage has the largest displayed role-conditioned shift, but it comes from 14 of 96 conditions and 44 graph reports. Loan-default post-outcome leakage is available in only 5 of 96 conditions. The broader conclusion is that graph contracts expose semantically different commitments whose evidence strength must be reported with their counts.

C. Prompt, Parser, and Control Summary

The graph-elicitation prompt gives the model the task, decision time, target, and fixed variable schema, then asks for a causal graph over exactly those variables. The graph extraction procedure accepts only reports that use allowed variable names and form an acyclic graph, following the invalid-report policy in Appendix B. The fresh prediction prompt presents a profile and asks for a probability score in $[0, 1]$, with no graph text. Graph-reminder controls show the model its own earlier graph before the same prediction; replay controls reconstruct the earlier graph-report context before prediction. These controls diagnose context dependence and steerability. They are not treated as proof that the original graph was a faithful spontaneous rationale.

D. Exact Values Behind the Main Figures

Table 6 gives representative exact values behind the coverage panel. The figure itself plots the full 192-condition panel.

Table 6. Representative coverage-faithfulness values. Coverage is a test-availability guardrail; cMPS and MaxShift are lower-better behavioral consistency errors; AUROC is an accuracy diagnostic. These are aggregate row summaries, so no standard deviation is reported.

Task	Model	T_g/T_p	Coverage	cMPS	MaxShift	AUROC
Admissions	Qwen 7B	0.2/0.0	0.714	0.024	0.354	0.731
Admissions	Qwen 7B	0.5/0.0	0.714	0.017	0.303	0.731
Loan default	Qwen2 7B	0.5/0.0	0.333	0.022	0.471	0.476
Loan default	Qwen2.5 14B	0.2/0.0	0.573	0.018	0.732	0.613
Loan default	Qwen2.5 14B	0.5/0.0	0.604	0.016	0.696	0.613
Loan default	Qwen 14B	0.5/0.0	0.722	0.002	0.062	0.496

Table 7. Exact role-conditioned estimates underlying Figure 2B. Excess shift is lower better. Means average graph-report-level role means so large raw test-count differences do not dominate; intervals are nonparametric 95% bootstrap confidence intervals over graph-report units.

Task	Graph role	Shift	95% CI	Conditions	Graph reports
Admissions	Ordinary irrelevant field	0.013	[0.012, 0.014]	46/96	344
Admissions	Demographic field marked irrelevant	0.015	[0.011, 0.019]	14/96	40
Admissions	Post-outcome leakage	0.031	[0.027, 0.034]	14/96	44
Loan default	Ordinary irrelevant field	0.012	[0.011, 0.012]	68/96	686
Loan default	Demographic field marked irrelevant	0.009	[0.008, 0.010]	60/96	474
Loan default	Post-outcome leakage	0.003	[0.000, 0.005]	5/96	5

Table 8. Aggregate graph-access controls behind Figure 3. ΔcMPS is relative to the fresh graph-absent baseline for the same task; positive values mean higher graph-contract violation. $|\Delta s|$ and Disagree measure score and decision movement relative to fresh on the same matched profiles. Each task/protocol row aggregates six evaluated conditions with licensed tests.

Task	Protocol	Cov.	cMPS	ΔcMPS	MaxShift	cFlip	$\lvert\Delta s\rvert$	Disagree	Pairs
Admissions	Fresh no graph	0.357	0.022	0.000	0.500	0.044	0.000	0.000	2000
Admissions	Graph reminder	0.357	0.035	+0.013	0.610	0.047	0.152	0.265	2000
Admissions	Replay	0.357	0.015	-0.007	0.300	0.023	0.091	0.152	2000
Admissions	Replay+graph	0.357	0.022	0.000	0.496	0.025	0.105	0.186	2000
Loan default	Fresh no graph	0.636	0.014	0.000	0.808	0.005	0.000	0.000	4400
Loan default	Graph reminder	0.636	0.034	+0.020	0.963	0.036	0.148	0.159	4400
Loan default	Replay	0.636	0.052	+0.038	0.974	0.071	0.269	0.497	4400
Loan default	Replay+graph	0.636	0.039	+0.025	0.899	0.060	0.286	0.416	4400