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Testing Adaptive Friction for Generative AI Programming Through Reproducible Simulations

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ABSTRACT

Generative AI can accelerate programming while leaving independent competence insufficiently tested. This study develops a joint decision rule requiring adaptive reasoning prompts to demonstrate a meaningful transfer gain without excessive completion time. A reproducible simulation compared ordinary assistance, static friction, and adaptive friction using a continuous ability moderator and correlated learning and time outcomes. Six scenarios were evaluated at three sample sizes, with 5,000 replications per configuration. In one illustrative dataset of 165 synthetic users, adaptive friction increased the transfer index by 7.53 points and produced a geometric time ratio of 1.023 relative to ordinary assistance. These quantities reflect imposed assumptions, not observed human effects. Across repeated favourable simulations, the joint criterion passed in 54.2%, 87.0%, and 99.1% of runs at sample sizes of 165, 330, and 660. A separate detector model showed how false prompts can breach the time budget despite preserving expected learning gains. The contribution is an auditable evaluation framework connecting meaningful benefit, efficiency limits, and trigger uncertainty. Prospective human studies remain necessary to establish effectiveness.

Keywords*: Adaptive friction; Generative artificial intelligence; Unaided transfer; Non-inferiority; Simulation study*

1. Introduction

Generative artificial intelligence changes the cost of producing plausible solutions, and a completed task does reveal how much its author understands. Experiments on professional writing and workplace support document productivity gains under particular conditions [1-5]. Such evidence makes assisted programming attractive to educators and employers, while leaving a separate question unresolved: what remains when assistance disappears? Research in mathematics education demonstrates that interface safeguards can alter the relationship between assisted performance and subsequent unaided achievement [6-10]. Programming evaluation therefore needs to distinguish productive collaboration from the temporary substitution of machine output for human competence.

Cognitive offloading provides a more precise starting point than claims that AI inevitably damages thinking. External resources redistribute processing demands [11-13], and expectations of later access can change what people remember [13-16]. Neither mechanism establishes inevitable deterioration. The relevant design problem is whether delegation supports learning, displaces essential practice, or does both across different users and tasks. Work on automation distinguishes appropriate use from misuse and disuse [17-20]. Trust consequently matters when it tracks system capabilities and contextual demands [21-25]. A high acceptance rate, rapid insertion, or unchanged code block cannot independently diagnose careless reliance.

Reasoning prompts offer one plausible intervention. Self-explanation research links explanation generation to understanding [25-28], retrieval practice can improve later retention [29-32], and broader learning research cautions that instructional techniques differ in effectiveness and generalisability [33-36]. Nevertheless, additional interaction is not automatically educational. Cognitive load [37,38] and expertise reversal [39-42] make indiscriminate prompting questionable, particularly when experienced programmers already understand a recommendation. Cognitive forcing functions have also been investigated directly in AI-assisted decisions [43-46]. Adaptive friction therefore cannot credibly be introduced as the first attempt to interrupt unreflective AI use. Its value depends on selecting useful occasions without imposing disproportionate interruption costs.

Human–AI interaction guidelines emphasise intelligibility, correction, and user control [47-50]. Studies of explanations and appropriate reliance further show why confidence, acceptance, and decision quality require distinct measurements [51-54]. Related supplied work addresses trust calibration across organisational settings [55,56] and institutional AI governance [58]. A surgical-training preprint proposes evaluating competence after feedback withdrawal [59,60], while an education preprint considers pedagogical uncertainty and belonging [61-64]. These sources motivate attention to agency

and independent performance; they do not identify the causal effect of adaptive prompts in programming. Transfer across domains remains a hypothesis requiring direct investigation [65-69].

The methodological opportunity is to connect interface evaluation to an explicit decision standard. Demonstrating any positive mean effect is weaker than demonstrating a worthwhile benefit within an acceptable time budget. Equivalence testing separates negligible differences from statistically inconclusive comparisons [69-73]. Joint requirements are especially relevant when productivity gains could otherwise compensate numerically for unacceptable learning loss. Adaptive-intervention research supplies complementary concepts for defining decision occasions [74-79] and identifying proximal effects through repeated randomisation [80-82]. Here, those concepts inform a future experimental programme rather than implying that a working adaptive interface has already been validated.

The policy also requires a definition of independent competence. Debugging an unfamiliar implementation, explaining a boundary condition, and applying a concept to a new problem measure different capabilities. Prompt acceptance and self-reported confidence can supplement these assessments but cannot replace them. This separation prevents interface telemetry from simultaneously defining the intervention trigger and serving as circular evidence that the intervention improved reasoning.

This article develops a simulation-based evaluation of that decision problem, following principles for transparent simulation design [83-87]. Its contribution is threefold: a noncompensatory learning-and-time criterion, an operating-characteristic analysis under favourable and unfavourable assumptions, and an analytical account of trigger sensitivity and specificity. The research questions concern how often a candidate policy satisfies both requirements, whether the proposed sample size detects ability moderation, and how inaccurate triggers change expected feasibility. The supplied experimental blueprint establishes the starting assumptions, not empirical evidence. All supplied bibliographic records are retained in an applicability assessment, with unresolved metadata distinguished from checked sources. The resulting framework supports a defensible trial design while deliberately withholding claims about demonstrated human learning gains.

2. Methodology

We conducted a computational design study, not a participant experiment. Three equally sized synthetic arms represented ordinary assistance, fixed reasoning prompts, and adaptive prompts. Baseline ability followed a standard normal distribution and remained continuous [87-89]. The illustrative dataset used 55 units per arm. Table 1 specifies assumed coefficients; no parameter was estimated from human observations. Transfer was modelled as a continuous index in point units rather than a bounded test proportion. The bivariate model was:

$$\mathbf{z}_i = \mathbf{B}^{\top}\mathbf{x}_i + \boldsymbol{\varepsilon}_i, \quad \boldsymbol{\varepsilon}_i \sim \mathcal{N}_2(\mathbf{0}, \boldsymbol{\Sigma}), \quad \mathbf{z}_i = (Y_i, \log T_i)^{\top}$$

$$\mathbf{x}_i = (1, S_i, A_i, a_i, S_i a_i, A_i a_i)^{\top}, \quad \mathbf{B} = (\boldsymbol{\beta}, \boldsymbol{\alpha})$$

(1) In (1), the design vector contains an intercept, static and adaptive indicators, ability, and both treatment–ability interactions. The coefficient vectors govern immediate transfer and log completion time. Residual standard deviations were 6.5 points and 0.12 log minutes, with correlation 0.30. Separate ordinary least-squares fits produced conditional contrasts at mean ability and Student confidence intervals. Delayed outcomes were secondary illustrations generated by (2), with arm-specific decrements and independent Gaussian noise of standard deviation four.

$$D_i = Y_i + d_{C_i} + \eta_i, \quad \eta_i \sim \mathcal{N}(0, 4^2), \quad (d_O, d_S, d_A) = (-14, -6, -5)$$

(2) The primary policy decision required the one-sided 95% lower confidence bound for adaptive transfer benefit to exceed five points and the corresponding upper bound for its geometric time ratio to remain below 1.10. Equation (3) defines this intersection–union rule. Both thresholds are illustrative design choices requiring stakeholder justification. Time non-inferiority uses only the upper bound; descriptive equivalence additionally requires a 90% interval within [0.90, 1.10]. The exact log limits are log(0.90) and log(1.10), which are asymmetric.

$$G = \mathbf{1}\{\widehat{\beta}_A - t_{0.95,\,n-6}\,\mathrm{SE}(\widehat{\beta}_A) > 5\}$$

$$\times\, \mathbf{1}\{\widehat{\alpha}_A + t_{0.95,\,n-6}\,\mathrm{SE}(\widehat{\alpha}_A) < \log(1.10)\}$$

(3) We evaluated six mechanisms at total sample sizes 165, 330, and 660, with 5,000 independent replications per configuration. Scenarios varied adaptive benefit, moderation, and time cost while retaining the static comparator. Outputs included superiority rejection, time non-inferiority, joint passage, interaction rejection, bias, and interval coverage. Monte Carlo standard errors quantified simulation precision. The intersection–union decision concerns one prespecified policy; additional confirmatory comparisons would require a declared family and multiplicity control [26, 90-93].

Equation (4) separately examines expected detector feasibility using prevalence, sensitivity, specificity, prompt benefit, false-prompt harm, and aggregate interruption cost. It is an analytical sensitivity model, not a fitted explanation of the arm-level simulation. All simulations used complete outcomes. A

future randomised trial should retain all allocated participants, record reasons for missingness, and prespecify imputation and departures from missing-at-random assumptions [27, 28, 94-98]. No missing-data test can establish those assumptions. Code, generated data, coefficients, and random seeds accompany the manuscript.

$$r = p\,s + (1-p)(1-c), \quad \Delta_Y = b\,p\,s - h(1-p)(1-c)$$

$$R_T = 1 + kr/T_0, \quad \mathcal{F} = \{(s,c) : \Delta_Y \geq 5,\ R_T \leq 1.10\}$$

(4) In (4), p is need prevalence, s sensitivity, c specificity, b benefit when a needed prompt is delivered, h harm from an unnecessary prompt, k total added minutes if every eligible occasion is prompted, and T_0 baseline time. Parameters are fixed scenario assumptions.

Table 1. Assumed generating parameters and evaluation settings. These values are not empirical estimates.

Quantity	Ordinary / static / adaptive	Interpretation
Immediate intercepts	65 / 71 / 72.5	Assumed transfer index
Ability slopes	10 / 6.5 / 7.5	Points per baseline SD
Time intercepts	log(45) / log(45)+0.22 / log(45)+0.03	Log minutes at mean ability
Time ability slope	−0.15 in every arm	Log minutes per SD
Residual SD and correlation	6.5 points; 0.12 log minutes; ρ = 0.30	Bivariate Gaussian errors
Delayed change	−14 / −6 / −5	Additional error SD = 4
Joint decision thresholds	Gain > 5 points; time ratio < 1.10	One-sided 95% bounds
Replicated design	6 scenarios × 3 sizes × 5,000 runs	90,000 synthetic trials

3. Empirical results from simulation experiments

The illustrative dataset demonstrates the analysis pipeline rather than treatment effectiveness. At mean baseline ability, adaptive friction exceeded ordinary assistance by 7.53 transfer points, with a 95% confidence interval of 5.09–9.97; the static contrast was 6.20 points, with interval 3.76–8.65 (Table 2). Adaptive versus static friction differed by 1.33 points, with interval −1.14–3.79. Consequently, this realisation supports neither a precise learning advantage over static prompts nor a conclusion about real programmers. The favourable arm ordering largely recovers the ordering imposed by the generating coefficients [95-99].

The adaptive geometric time ratio was 1.023, with a 90% interval of 0.988–1.060. This interval satisfied both the illustrative upper non-inferiority limit and the stricter descriptive equivalence region. Static friction produced a ratio of 1.290, with interval 1.246–1.336. Adaptive assistance was therefore substantially faster than static prompting within this artificial dataset [100-103]. Combining the one-sided learning and time bounds yielded a positive joint decision (Figure 1). Such a decision is conditional on the chosen five-point minimum; changing that threshold changes the substantive question, even when the fitted coefficients remain identical.

Table 2. Conditional contrasts at mean baseline ability in one synthetic dataset (n = 165). Transfer intervals are descriptive and unadjusted.

Contrast	Immediate gain 95% CI	Time ratio 90% CI	Delayed gain 95% CI
Adaptive vs ordinary	7.53 (5.09, 9.97)	1.023 (0.988, 1.060)	16.39 (13.39, 19.39)
Static vs ordinary	6.20 (3.76, 8.65)	1.290 (1.246, 1.336)	14.67 (11.67, 17.67)
Adaptive vs static	1.33 (-1.14, 3.79)	0.793 (0.766, 0.821)	1.72 (-1.30, 4.75)

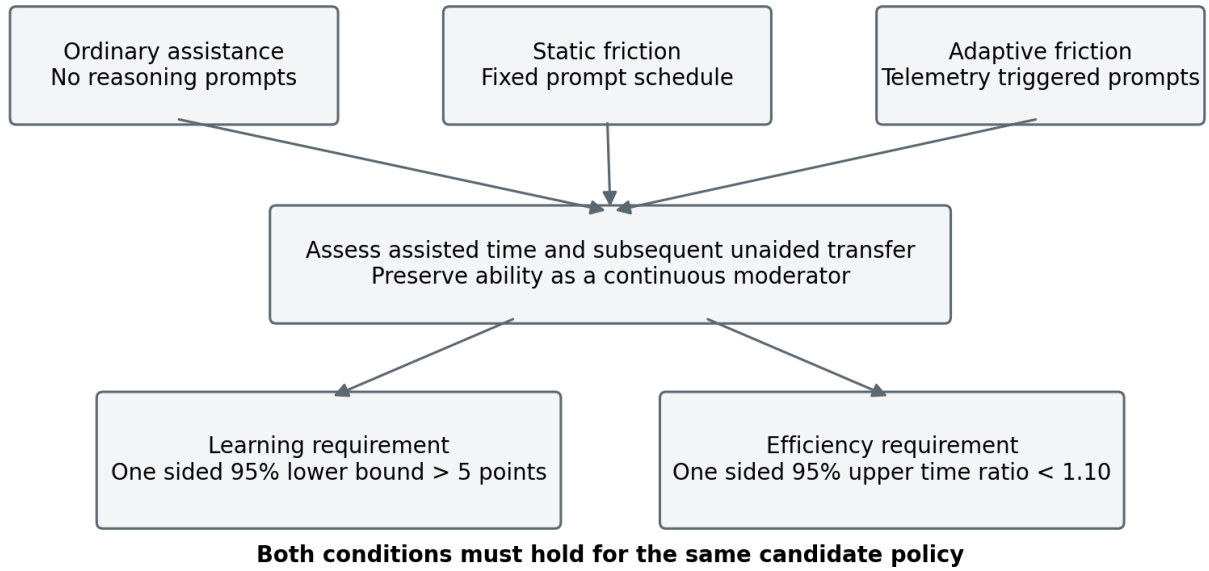


Figure 1. Proposed joint evaluation rule. The diagram specifies an evidence requirement; it does not depict a completed human experiment.

Ability-dependent estimates revealed why average effects cannot settle personalisation claims. The adaptive transfer contrast was 9.43 points at one standard deviation below mean ability and 5.63 points at one standard deviation above it (Figure 2). Their respective 95% intervals were 6.07–12.79 and 1.87–9.39. These overlapping intervals do not themselves test moderation; that requires the interaction contrast. Delayed adaptive and static advantages over ordinary assistance were 16.39 and 14.67 points. These larger differences follow the explicitly imposed arm-specific decay and cannot be interpreted as evidence that prompts protect long-term memory.

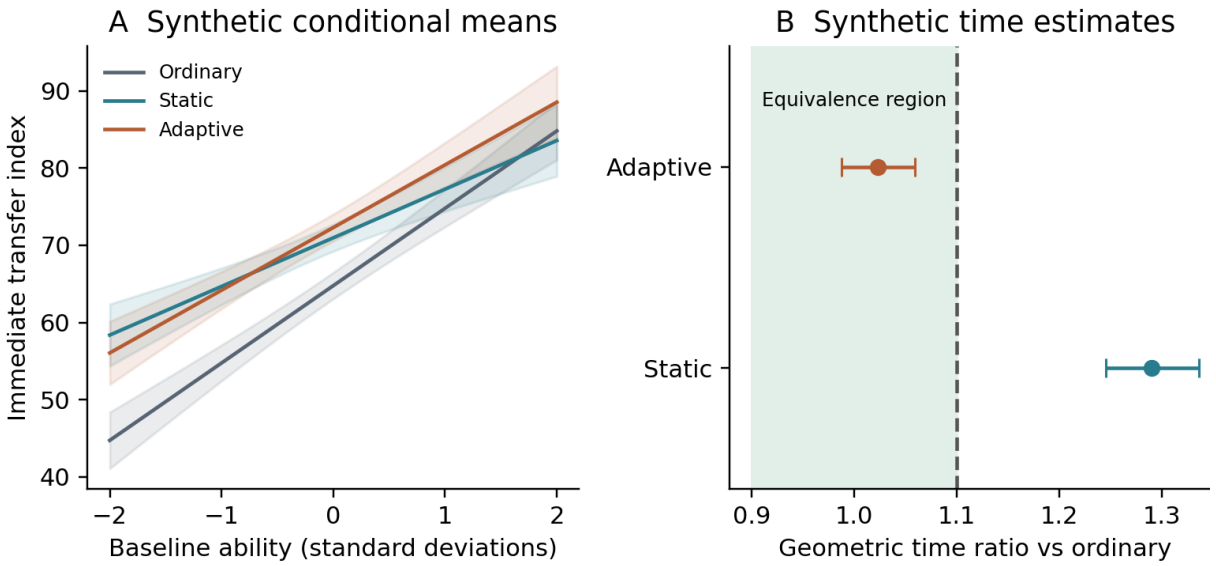


Figure 2. One illustrative simulation. Panel A shows fitted transfer means with 95% confidence bands. Panel B shows geometric time ratios with 90% intervals; shading indicates the descriptive equivalence region. All effects arise from assumed coefficients.

Repeated simulations exposed a substantial difference between detectable improvement and decision readiness. Under source-like assumptions, positive adaptive benefit was detected in every replication at each sample size. Yet the joint criterion passed in only 54.16% of runs with 165 units, compared with 87.04% at 330 and 99.08% at 660 (Table 3). At the smallest size, the Monte Carlo standard error for joint passage was 0.70 percentage points. The illustrative successful dataset therefore understates design uncertainty when considered alone. A convincing-looking single simulation is a poor substitute for operating characteristics [104-108].

Table 3. Operating characteristics under favourable source-like assumptions. NI = non-inferiority; MCSE = Monte Carlo standard error in percentage points; 5,000 replications per row.

Total n	Superiority %	Time NI %	Joint passage % (MCSE)	Moderation %
165	100.00	88.78	54.16 (0.70)	49.76
330	100.00	99.06	87.04 (0.47)	79.56
660	100.00	99.98	99.08 (0.14)	98.08

The original sample-size rationale also proved insufficient for the specified moderator. Interaction rejection under source-like assumptions reached 49.76%, 79.56%, and 98.08% across increasing sample sizes. Thus, 165 units did not deliver approximately eighty-percent moderation power for this generating mechanism. This finding does not establish a universal recruitment target: smaller effects, heavier tails, missing outcomes, model misspecification, or uneven allocation could alter requirements. Across all configurations, transfer-contrast coverage ranged from 94.50% to 95.58%, and absolute bias remained below 0.04 points. These checks support implementation accuracy under the model, not external validity [109-113].

Failure scenarios were equally informative (Figure 3). With a two-point adaptive gain, conventional superiority rejection increased from 48.36% to 94.06% as sample size grew, but the joint criterion never passed in 5,000 runs at any size. A large effect accompanied by excessive delay likewise never passed. At the five-point gain boundary, joint passage ranged from 3.70% to 4.94%; at the time boundary it ranged from 2.00% to 4.82%. These frequencies illustrate the conservative behaviour of requiring both conditions, while zero observed passages do not imply a zero underlying probability.

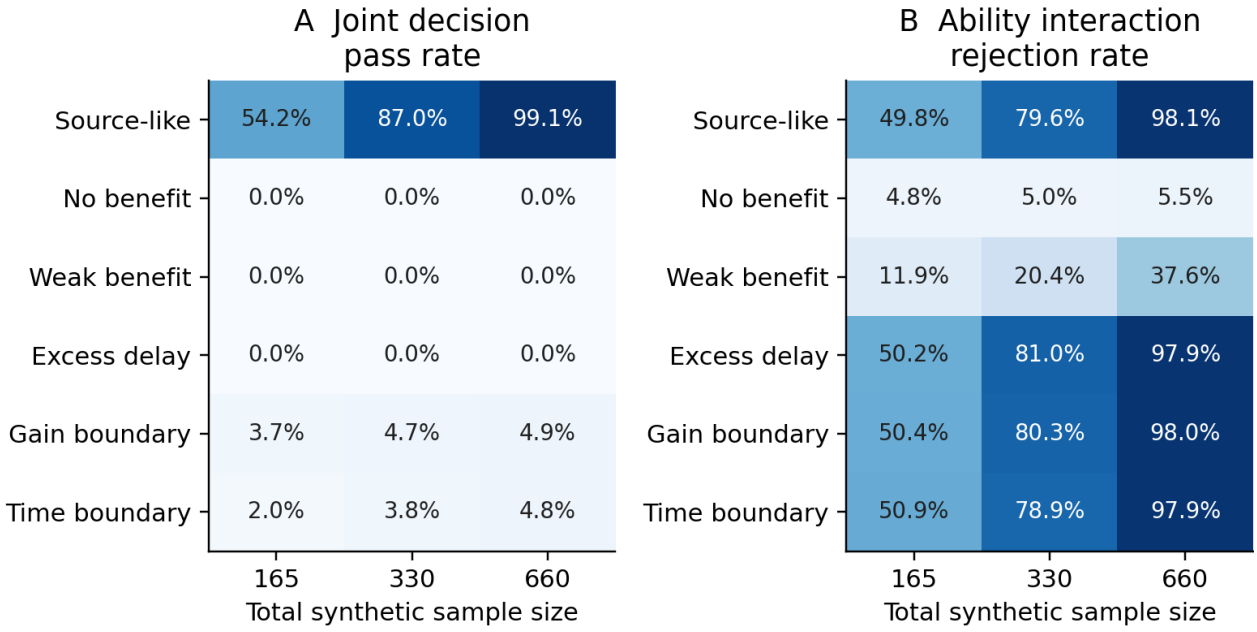


Figure 3. Monte Carlo operating characteristics. Boundary scenarios place one true parameter exactly at its decision limit. In the no-benefit scenario, interaction rejection is a type I error frequency rather than power. At 5,000 runs, MCSE cannot exceed 0.71 percentage points.

The confidence-bound criterion concerns one candidate policy at mean ability. It does not certify every ability subgroup, guarantee an individual benefit, or prove that the telemetry mechanism caused the simulated arm difference. Subgroup guarantees would require additional estimands and sufficient precision across the intended population. Likewise, the complete-data design omits attrition and task-level dependence, so its sample-size results are conditional benchmarks. For configurations with no observed joint passes, the approximate rule-of-three upper bound is 0.06% per configuration, rather than literal impossibility. Reporting these distinctions matters because simulation precision can be high even when scientific uncertainty about the generating assumptions remains substantial. The next evidential step is therefore an independently evaluated pilot that estimates plausible response magnitudes and interruption costs before any definitive trial recruitment calculation.

The detector analysis identified a complementary mechanism for failure (Figure 4). With need prevalence 0.40, sensitivity 0.80, and specificity 0.85, the assumed response model yielded a 7.50-point expected gain and an 8.2% time increase. Reducing specificity to 0.50 preserved a 7.08-point gain but raised time by 12.4%, violating the budget. False prompts therefore consumed efficiency without necessarily eliminating learning benefit. These are expected values, without inferential uncertainty, and the detector model is separate from the main simulation. Together, the analyses justify evaluating trigger accuracy, meaningful learning, and completion time as distinct quantities before prospective deployment.

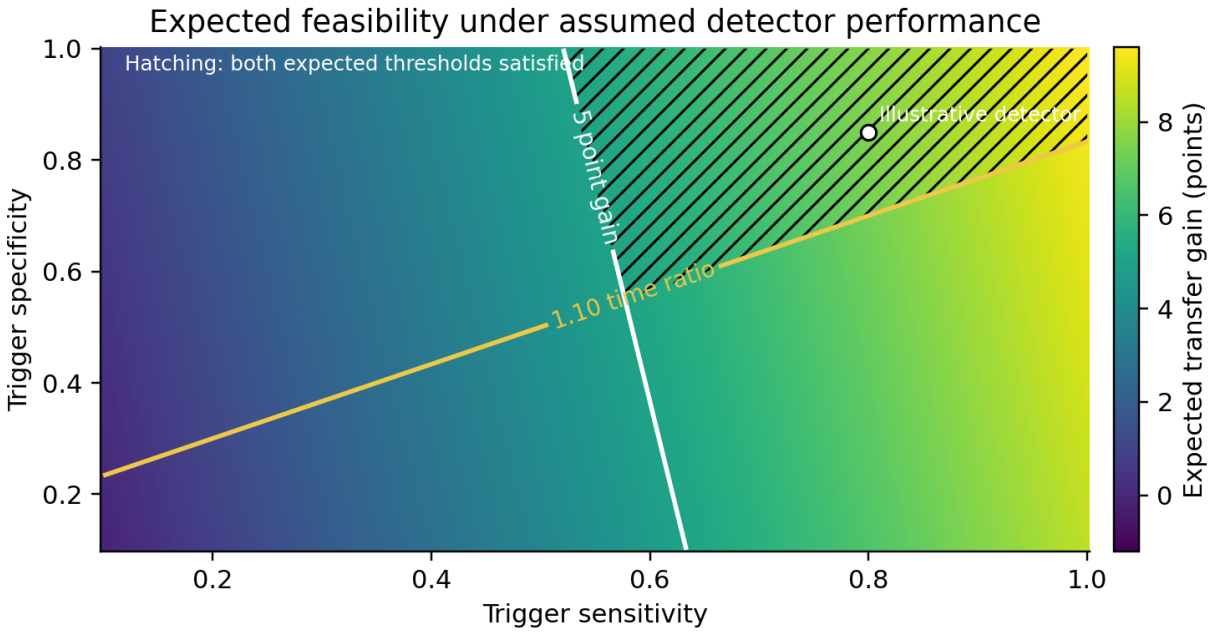


Figure 4. Analytical detector sensitivity, separate from the arm-level simulations. Assumptions: p = 0.40, b = 24 points, h = 2 points, k = 9 minutes, T_0 = 45 minutes. Hatching marks expected feasibility, not confidence-bound certification. The marked detector has sensitivity 0.80 and specificity 0.85.

4. Conclusion

Adaptive friction should be evaluated against explicit learning and efficiency requirements rather than a favourable average effect alone. This simulation study formalised that principle through a joint confidence-bound decision and demonstrated its consequences across six generating mechanisms. Under the favourable assumptions, a sample of 165 synthetic users almost always detected some learning benefit but satisfied the stronger joint requirement in only 54.2% of replications. Increasing sample size improved decision reliability and moderation detection, while insufficient benefit or excessive delay remained unacceptable. A separate analytical model showed how false prompts can exhaust the interruption budget even when expected learning gains persist.

These findings establish computational operating characteristics, not the effectiveness of an AI interface. The proposed thresholds, response coefficients, and detector properties require empirical justification. A prospective study should freeze the assistance model, validate telemetry against independent indicators, randomise interface policies, and assess unaided transfer after assistance withdrawal. Delayed testing, accessibility, and missingness should be prespecified. The practical contribution is an auditable standard for deciding what evidence would justify adaptive prompting, together with reproducible calculations that expose the limitations of the initial experimental design.

Data and research declarations

No human participants were recruited, and no observed participant records were analysed. All numerical datasets were generated computationally. The supplement contains the executable analysis, generated data, Monte Carlo summaries, figure files, and the reference audit. The prospective study design has not been described as preregistered or ethically approved.

Appendix A Reproducibility and prospective validation

The initial blueprint supplied hypothetical effects and example R code. The present Python implementation uses NumPy PCG64 with seed 20261103 for the illustrative dataset. Each replicated scenario uses seed 20261103 + 1000(j + 1) + k, where j = 0, 1, 2 indexes total sample size and k = 0, …, 5 indexes scenarios in Figure 3 order. Errors for immediate transfer and log time share a correlation of 0.30; delayed error is independently generated. Linear transfer indices are not clipped to an artificial 0–100 boundary. The illustrative scores range from 40.38 to 92.36.

Source-like, no-benefit, weak-benefit, excessive-delay, gain-boundary, and time-boundary scenarios respectively use adaptive transfer intercept increments (7.5, 0, 2, 7.5, 5, 7.5), adaptive ability interactions (−2.5, 0, −1, −2.5, −2.5, −2.5), and adaptive log-time increments (0.03, 0, 0.03, 0.15, 0.03, log(1.10)). Static coefficients remain fixed. Replications use independent synthetic datasets. Mean-ability contrasts target ability zero rather than the realised sample mean. Residual degrees of freedom equal n − 6.

For each fitted model, covariance is the inverse design cross-product multiplied by residual mean square. Superiority tests whether the one-sided 95% lower transfer bound exceeds zero; the joint rule replaces zero with five and also requires the upper time bound below log(1.10). Moderation rejection uses the two-sided 5% test of the adaptive ability interaction. Coverage refers to two-sided 95% intervals for the generating adaptive transfer increment.

Monte Carlo standard error is $\sqrt{[\hat{p}(1-\hat{p})/5000]}$. Detector parameters in Figure 4 are independent hypothetical inputs and do not calibrate the arm-level time coefficients.

A future evaluation should randomise users to the three policies with concealed allocation and freeze the AI model version, task bank, prompt rules, and scoring rubric before recruitment. Ability should be assessed before allocation and modelled continuously. An initial pilot should validate proposed triggers against independently coded reasoning behaviour; short acceptance times or unchanged code alone are insufficient. Assistance should then be withdrawn for independently scored debugging and transfer tasks, with a delayed test around two weeks later. Blinded scoring, accessibility adjustments, prompt overrides, exposure logs, consented telemetry, and data minimisation should be prespecified.

The current three-arm design cannot isolate adaptive timing from prompt dose. A subsequent dose-matched comparator or microrandomisation at eligible decision occasions would identify that mechanism more directly [24]. Calibration requires separately elicited predictions and observed task outcomes; absolute discrepancy alone should not be called overconfidence. Bounded confidence errors require suitable measurement models rather than an unrestricted Gamma model. Missingness and dropout mechanisms require sensitivity analyses. None of these prospective procedures has been carried out in the present simulation.

Appendix B Supplied corpus applicability assessment

All 89 supplied references are preserved as bibliographic records and assessed for scope. This title-level mapping is not a systematic review and does not establish the findings or quality of papers whose full texts were not examined. Four supplied records were independently checked against a primary abstract or metadata record: the trust-calibration article, public-sector governance article, surgical feedback-withdrawal preprint, and pedagogical-uncertainty preprint [17]–[20]. The other 85 are explicitly marked in the bibliography; their supplied metadata and links remain unverified here.

AI governance and institutional agency. Potential conceptual context for control, accountability, and institutional adoption; titles alone cannot support causal interface claims. [17], [18], [29]–[51]

Learning and independent competence. Thematic relevance to feedback withdrawal and educational context; evidence from other settings does not validate programming effects. [19], [20], [52]–[55]

Pricing and consumer trust. Possible analogies for trust and contestability, with no direct evidence on programming retention. [56]–[62]

Business capability and synthetic data. Context for adoption or synthetic analysis; taxation and investment studies are outside the primary research question. [63]–[73]

Production and supply chains. Potential deployment settings only; no basis for inferring a cognitive prompting effect. [74]–[79]

Health and care systems. Different populations, mechanisms, or outcomes; retained for corpus completeness rather than used as experimental support. [80]–[93]

Environmental and engineering systems. Outside the learning-effect evidence base. Domain examples cannot supply effect sizes or trigger validity for this study. [94]–[113]

The 24 additional references provide the directly applicable theoretical and statistical foundation. The combined bibliography contains 113 unique records, including the complete supplied corpus. Retaining an unresolved record for this audit is not an endorsement of its existence, publication status, or relevance. Unresolved entries require primary-source verification before they can support manuscript claims or remain in a journal submission bibliography.

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