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PRISME: Intervention-Supervised Sparse Workspace Editing for Small Language Models

Design, controlled evaluation, and negative and positive evidence at the 100M-parameter scale

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Abstract

We design, implement, train and evaluate PRISME, a small-language-model architecture that augments a frozen causal decoder with a read-only source bank, an ephemeral sixteen-slot latent workspace that is re-created at every position, a shared sparse editor that rewrites one or two slots per action, a value controller trained on counterfactual intervention gains measured by executing paired edit/no-edit branches, and a differential readout with an exact zero-budget bypass. We reproduce the proposal's exact backbone parameter counts and its analytic operation table, prove its structural properties (causality, bitwise identity, exact locality, bounded displacement, one-step selection regret), and test seven pre-registered hypotheses with a complete implementation, a fifteen-invariant test suite, an incremental inference engine and controlled, seeded comparisons against a parameter-matched plain decoder, a dense-slot editor, a global-recurrence adapter trained with the identical recipe, five reference routers and exhaustive one- and two-step oracles. The zero-initialised readout prescribed by the proposal does not learn because of a scale mismatch; a scale-aware non-zero initialisation does. Sparse edits then lower a frozen backbone's held-out loss by 2.8 % and raise compositional exact match by about two points, but the global-recurrence adapter matches or exceeds this at every budget. Where to write does not matter for fixed schedules. An oracle that reads the target finds large opportunity (-15.4 % of loss), but 25 % of the variance of intervention gains is state-level rather than action-level, the opportunity is concentrated in high-uncertainty states where a wrong action costs more than a right one gains, and the learned controller ranks actions within a state near chance; it is worse than no routing on held-out data. On the locked test regimes the pre-registered primary effect is $+0.9$ points against a three-point threshold. At 103M parameters on 98M tokens of web text, neither the sparse editor nor the global adapter moves the frozen backbone by more than 0.1 % of loss and a parameter-matched plain decoder does better than both. Measured on Apple Silicon, per-action dispatch overhead erases most of the arithmetic savings of sparsity. We report the complete negative result on learned spatial routing, its mechanistic explanation, and the conditions under which the hypothesis could still be rescued.

Keywords: small language models; adaptive computation; latent workspace; counterfactual supervision; sparse routing; quality–compute frontier; reproducibility

JEL Classification: C45, C63, C88

1. Introduction

Small language models spend the same computation on every token. A growing body of work relaxes this by re-applying shared layers a variable number of times—recurrent depth, looped models, mixtures of recursions—and by learning when to stop (Dehghani et al., 2019; Geiping et al., 2025; Zhu et al., 2025; Bae et al., 2025; Wang et al., 2026). In all of these the extra computation is *global*: every refinement step rewrites the entire hidden state of the position. This paper asks a narrower question. If the extra computation is instead confined to a small latent workspace of M slots, and each step may rewrite only one or two of them, can a learned policy that chooses *where* to write—supervised by executed counterfactual branches that measure what each write would have done to the next-token loss—improve the quality–compute frontier beyond uniform or random slot selection, dense updates of every slot, and global latent recurrence at matched cost?

We call the resulting reference architecture PRISME (Predictive Routing with Intervention-Supervised Memory Editing). A causal decoder is left unchanged; a read-only *source bank* caches its past representations projected to a small width; at every position a fresh 16-slot workspace is initialised from the current representation; a shared two-block *sparse editor* rewrites one or two slots per action, chosen among 24 nonterminal actions and STOP by a *value controller*; a *differential readout* adds the change of an attention read over the workspace to the backbone's representation, so that with no edit the model returns the backbone output bitwise. The controller is trained on *intervention gains* $g(s,a;y) = \mathscr{L}(D(s),y) - \mathscr{L}(D(T_a(s)),y)$ obtained by actually executing the paired edit / no-edit branches at training time, with logged propensities.

What this paper is and is not. It is an executed, pre-registered research programme (Appendix E) with a complete implementation, invariant tests, controlled baselines and an honest accounting of parameters, operations, latency and memory. It is not a claim of architectural priority: an execution-time literature audit (Section 2) shows that each ingredient—sparse slot writes, loss-gain supervision of continue/stop decisions, zero-initialised identity insertion—has verified predecessors, and that only their combination lacks one. Nor is it a claim of superiority: the hypotheses of Section 1 are stated so that the experiments can refute them, and several are refuted.

Hypotheses. H1 (editor usefulness): with backbone and data fixed, sparse workspace edits improve held-out loss and compositional accuracy over the frozen backbone. H2 (spatial selection): learned selection of one or two slots beats uniform or random selection with the same editor at fixed cost. H3 (intervention supervision): predicting paired gains routes better than uncertainty-, entropy-, norm- or fixed-budget rules, controller overhead included. H4 (adaptivity): PRISME moves the quality-versus-cost or quality-versus-latency frontier relative to the best matched baseline. H5

(identity): at budget zero the output equals the backbone's. H6 (generalisation): gains survive unseen templates, deeper compositions and seeds. H7 (scaling, conditional). The pre-registered null possibilities are equally informative: an oracle may find beneficial edits that no learned controller can predict; token loss may improve while complete answers do not; a parameter-matched plain Transformer or a global recurrence may win at equal compute; fixed two-slot editing may match learned routing; dispatch overhead may dominate the theoretical sparse savings.

Summary of findings. On a frozen synthetic backbone with five adapter seeds, sparse one-or-two-slot edits lower the held-out answer-span loss by about 3 % and raise exact match by about two points at budget 2—but a parameter-matched global-recurrence adapter trained with the identical recipe does at least as well at every budget and dominates the quality-versus-cost frontier, and dense sixteen-slot writes do slightly better than sparse ones; on the locked test regimes the pre-registered primary effect is $+0.9$ points against a three-point threshold. Which slot is written does not matter for fixed schedules. An exhaustive one-step oracle that reads the target lowers the loss by 15 % on the synthetic suite and 3.6 % on web text, but the learned value controller—four feature sets, three loss variants, labels from an editor-unseen stream, calibration-based early stopping—never beats not routing on held-out data, because 25 % of the variance of intervention gains is action-level, opportunity concentrates in high-uncertainty states where a wrong action costs more than a right one gains, and the controller's within-state ranking is at chance. On 98M tokens of web text at 103M parameters, neither adapter moves the frozen backbone by more than a tenth of a percent of loss, the parameter-matched plain decoder does better than both, and the zero-initialised readout prescribed by the proposal does not learn without a scale-aware bootstrap. Measured on Apple Silicon, eight sparse edits cost three times the backbone's decode latency at 103M against an analytic 1.6 times, and dense writes are only 17 % slower than sparse ones. The exact zero-budget bypass holds bitwise; the parameter and operation accounting of the proposal is reproduced.

Contributions. (i) A complete, tested reference implementation of PRISME with exact parameter and operation accounting for 103M, 459M and 1.18B configurations, an incremental inference engine with separate backbone and bank caches, and a 15-invariant test suite (Sections 4–8). (ii) A counterfactual labelling and controller-training pipeline with logged propensities, exhaustive one-step and two-step oracle audits, and a variance decomposition of intervention gains into state and action components (Sections 6, 10.3). (iii) Controlled, seeded comparisons against a parameter-matched plain decoder, a dense-slot editor, a global-recurrence adapter trained with the identical recipe, and five reference routers, with paired uncertainty (Sections 9–10). (iv) Measured latency and memory profiles on Apple Silicon showing where sparse computation does and does not pay (Section 10.7). (v) A results-to-claim audit, pre-registration, deviations log and failure log that make

every number in this paper traceable to a run identifier (Section 14).

2. Related work and novelty positioning

We re-verified every reference at execution time from primary sources (arXiv metadata and full texts, official repositories and licences; Appendix D and `literature/novelty_audit.md`). Four strands are directly relevant.

Adaptive depth and latent recurrence. Universal Transformers (Dehghani et al., 2019) and ACT (Graves, 2016) introduced weight-shared recurrence over depth with learned per-position halting; PonderNet (Banino et al., 2021) made halting probabilistic. Mixture-of-Depths routes tokens around blocks with a fixed capacity (Raposo et al., 2024), LayerSkip trains early exits (Elhoushi et al., 2024), and CALM calibrates confidence-based exits (Schuster et al., 2022). The recurrent-depth line scaled latent recurrence to language model pretraining: Huginn (Geiping et al., 2025), Ouro (Zhu et al., 2025), Mixture-of-Recursions (Bae et al., 2025), looped Transformers (Giannou et al., 2023; Saunshi et al., 2025), and in 2026 RecurTrace (Wang et al., 2026), Elastic Latent Recurrence (Ladhak et al., 2026), Think-at-Hard (Fu et al., 2026; You et al., 2026) and several contemporaries listed in Appendix D. All of these refine the *entire* hidden state of a position (globally), and route depth per token or per sequence.

Slots, workspaces and sparse writes. Recurrent Independent Mechanisms (Goyal et al., 2019) and the Shared Global Workspace (Goyal et al., 2022) update a small number of competing modules per step; Slot Attention (Locatello et al., 2020) and Perceiver / Perceiver AR (Jaegle et al., 2021; Hawthorne et al., 2022) maintain a small bank of latents that cross-attend to a long input; Sparse Access Memory (Rae et al., 2016) writes sparsely into an external memory. Most recently ReFlux (Ye et al., 2026) writes router-selected sparse deep-to-shallow deltas into a frozen backbone, with the backbone unchanged when no route is active. PRISME's workspace differs in being *ephemeral* (reset per token), separate from a read-only cached bank, and edited by discrete 1–2-slot actions rather than by competition among modules.

Loss-gain supervision of routing. The supervision signal we use—the realised change in next-token loss of an executed extra branch—has become standard for *binary* continue/stop decisions in 2025–2026: Ouro's second stage fits its exit gate to observed per-token loss improvements (Zhu et al., 2025); TaH2 calls the same construction "lookahead depth supervision" (You et al., 2026); RecurTrace trains a halting head on an oracle "more depth still helps" label (Wang et al., 2026). Earlier, fitted value iteration and counterfactual risk minimisation established the use of logged

propensities for off-policy action evaluation (Ernst et al., 2005; Dudík et al., 2011; Swaminathan and Joachims, 2015). PRISME makes the label *action-conditional* over 24 spatial actions, logs propensities, and separates gain, cost, uncertainty and harm heads.

Zero-initialised identity insertion. Adding a zero-initialised branch so that a new module starts as the identity is a well-established device (Bachlechner et al., 2020; Alayrac et al., 2022; Zhang et al., 2023); exact bypass of unrouted tokens exists in Mixture-of-Depths and in TaH/TaH2, and ReFlux leaves the frozen backbone unchanged with no active route. The differential readout of Section 4.5 is a modest variant of this device and is *not* claimed as a contribution.

What is and is not new. After this audit, none of the three ingredients of PRISME is new on its own. What has no direct predecessor is their combination: a per-token slot workspace with selected local writes, trained with action-conditional counterfactual gains over a 25-action catalogue with logged propensities, behind an exact zero-budget bypass. Accordingly, the contribution of this paper is empirical and methodological—a controlled test of whether spatial selection supervised by counterfactual gains buys anything over uniform, dense or global alternatives at matched cost—rather than architectural priority. Table 14 in Appendix D gives the claim-by-claim comparison.

3. Formal problem

Autoregressive prediction under a computation budget. Let $\mathcal{V}$ be a vocabulary of size V and $x_{1:T} \in \mathcal{V}^T$ a sequence. A causal language model assigns $p_\theta(x_{t+1} \mid x_{1:t})$ and is trained to minimise the teacher-forced cross-entropy $\mathcal{L}(\theta) = \mathbb{E}\big[-\log p_\theta(x_{t+1} \mid x_{1:t})\big]$. We consider models whose per-position computation is *variable*: for each position t the model may execute a number $n_t \in \{0, \dots, R_{\max}\}$ of additional *actions* before emitting its distribution, each action having a known arithmetic cost. Writing $c(a)$ for the cost of action a (in units of the backbone's own operations per token), the realised cost of position t is $C_t = 1 + \sum_{r \le n_t} c(a_{t,r})$ plus the cost of deciding, and the quantities of interest are the pairs

$$\big(\,\text{quality}(\pi),\ \ \mathbb{E}_t[C_t \mid \pi]\,\big), \qquad \big(\,\text{quality}(\pi),\ \ \text{median}_t\ \text{latency}_t(\pi)\,\big), \tag{1}$$

traced out as the routing policy π and its penalties vary. Quality is either the held-out cross-entropy or, for tasks with programmatic ground truth, the exact-match accuracy of free greedy generation. The research question is whether a policy that chooses *where* to write (which of M latent slots) using supervision from executed counterfactual branches moves the frontier (1) relative to policies that do not choose (uniform or random slot selection, dense updates of all slots) and relative to global latent recurrence at equal cost.

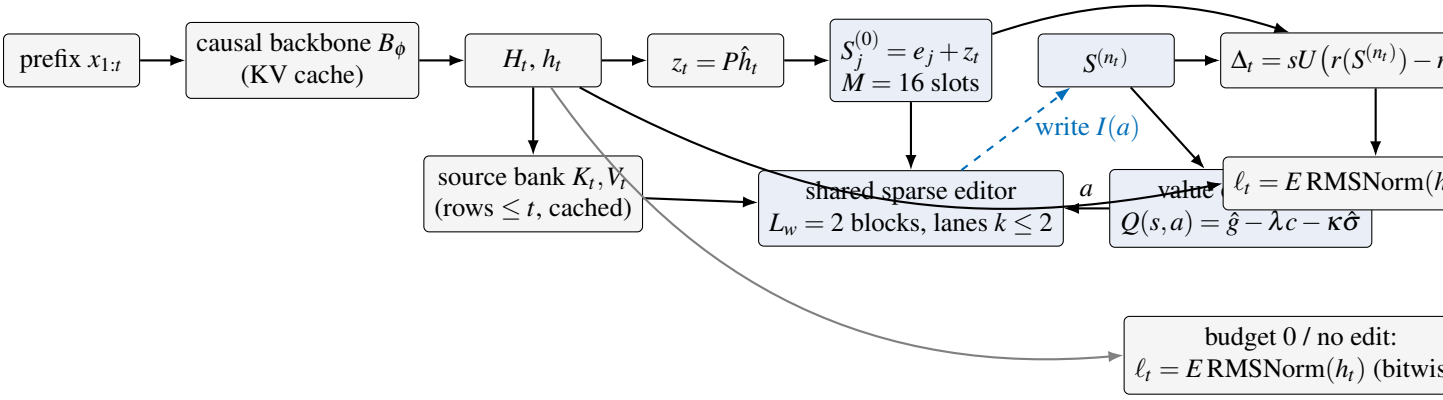


Figure 1. PRISME data flow for one position t. Solid arrows are read paths, the dashed arrow is the only write path (into 1–2 of the M slots per action). The backbone residual stream H, the source bank (K,V) and the summary z_t are inputs to the editor and are never modified by it; the workspace is discarded after the token. With zero executed edits the output is bitwise the backbone output (explicit bypass).

Causal prefix constraint. Every quantity that influences $p_\theta(x_{t+1} \mid x_{1:t})$—including the routing decision—must be a function of $x_{1:t}$ only. In particular the label x_{t+1} is available to the *training* procedure that constructs supervision, but never to the deployed router (Section 5).

What is *not* studied. The model has no persistent memory across positions (the workspace is reset at every t), no retrieval, no tools, no reinforcement learning, and no textual chain of thought. These exclusions are deliberate: they isolate the single mechanism under test.

4. The PRISME architecture

PRISME (Predictive Routing with Intervention-Supervised Memory Editing) adds to a standard decoder a read-only *source bank*, an ephemeral M-slot *workspace*, a shared *sparse editor*, a *value controller* and a *differential readout*. Figure 1 gives the data flow; Table 1 the three dimensioned configurations. All notation is collected in Appendix A.

4.1. Backbone and source bank

The backbone B_ϕ is a pre-norm causal decoder with RMSNorm (Zhang and Sennrich, 2019), rotary position embeddings (Su et al., 2021), grouped-query attention (Ainslie et al., 2023), SwiGLU feed-forward blocks (Shazeer, 2020), tied input/output embeddings and one final normalisation. It returns the final residual stream $H_t = B_\phi(x_{1:t}) \in \mathbb{R}^{t\times d}$, with $h_t = H_t[t]$, and its own head is $\ell_t^{\mathrm{bb}} = E\,\mathrm{RMSNorm}(h_t)$. The *source bank* is a projection of the history to the workspace width

$w \ll d$:

$$\hat{H}_t = \mathrm{RMSNorm}_{\mathrm{bank}}(H_t), \qquad K_t = \hat{H}_t W_K, \qquad V_t = \hat{H}_t W_V, \qquad z_t = P\hat{h}_t, \qquad W_K, W_V, P^\top \in \mathbb{R}^{d\times w}. \tag{2}$$

The bank is distinct from the Transformer KV cache: it stores projected *final* representations, it is append-only, and a query at position t may read rows $1,\dots,t$ only.

4.2. Ephemeral workspace

For every position a fresh workspace $S^{(0)} \in \mathbb{R}^{M\times w}$ is formed from M trainable address embeddings e_j and the current summary,

$$S_j^{(0)} = e_j + z_t, \qquad j = 0,\dots,M-1. \tag{3}$$

e_j carries no imposed semantic role; it only makes slots distinguishable. No state of S survives to position $t+1$; history is carried exclusively by the autoregressive prefix and the two caches.

4.3. Action catalogue

The nonterminal actions are $\textsc{single}_j$ ($j < M$, edit one slot) and $\textsc{pair}_{2i,2i+1}$ ($i < M/2$, edit two fixed slots), plus STOP. For $M = 16$ this gives $|\mathscr{A}| = 24$ nonterminal actions and 25 in total; the $\binom{16}{2} = 120$ arbitrary pairs are *not* available. We write $I(a) \subset \{0,\dots,M-1\}$ for the written index set of action a ($|I(a)| = k(a) \in \{1,2\}$, $I(\textsc{stop}) = \varnothing$). STOP is absorbing and a hard cap $R_{\max} = 8$ bounds the number of actions per position.

4.4. Shared sparse editor

Given the current workspace S, an action a with active set $I = I(a)$, the step index r and the remaining budget $\rho = R_{\max} - r - 1$, the editor forms the active queries $X = S_I + \mathrm{ctx}([r/R_{\max},\ \rho/R_{\max},\ k/2]) \in \mathbb{R}^{k\times w}$ and applies $L_w = 2$ *shared-weight* blocks. Each block computes, with pre-normalisation and residual connections,

$$X \leftarrow X + \mathrm{MHA}\big(q = X,\ k = K_t,\ v = V_t;\ \mathrm{mask}_{\le t}\big) \qquad \text{(read the causal source bank)} \tag{4}$$

$$X \leftarrow X + \mathrm{MHA}\big(q = X,\ k = \bar{S},\ v = \bar{S}\big) \qquad \text{(read all } M \text{ slots of the pre-write snapshot } \bar{S} = S) \tag{5}$$

$$X \leftarrow X + \mathrm{SwiGLU}(X). \tag{6}$$

The bank attention uses query and output projections only (the bank is already in width w and is cached), so the bank is never re-projected per action. The slot attention re-projects the M slot

keys/values at every block (reference implementation; an optimised kernel could cache the keys of unchanged slots). The two lanes of a pair read the *same* snapshot $\bar{S}$, so their writes are simultaneous by construction.

Clipped, gated write. Let $\tilde{S}_j$ be the lane output for $j \in I$. The write is

$$\delta_j = \tilde{S}_j - S_j, \qquad \delta_j^c = \delta_j \min\Big(1, \frac{c}{\max(\mathrm{rms}(\delta_j), \varepsilon)}\Big), \qquad \eta_j = \eta_{\max}\, \sigma\big(g([S_j; \tilde{S}_j])\big), \qquad S'_j = S_j + \eta_j \delta_j^c, \tag{7}$$

and $S'_m = S_m$ *exactly* for $m \notin I$ (the implementation materialises the new rows through a one-hot scatter from the snapshot, so the result is independent of lane order; Proposition 3). We use $\eta_{\max} = 0.25$, $c = 1$: these are untested research hyper-parameters, not tuned values. The gate g and the context map are zero-initialised, so $\eta_j = \eta_{\max}/2$ at initialisation.

4.5. Differential readout and exact bypass

A single attention read with a query that depends on z_t only, $r(S, z_t) = \sum_j \alpha_j S_j$, $\alpha = \mathrm{softmax}_j\big((W_q z_t) \cdot (W_k S_j)/\sqrt{w}\big)$, is evaluated at the final and at the initial workspace, and their difference is mapped back to the residual width:

$$\Delta_t = s\, U \big(r(S^{(n_t)}, z_t) - r(S^{(0)}, z_t)\big), \qquad h_t^{\mathrm{corr}} = h_t + \Delta_t, \qquad \ell_t = E\, \mathrm{RMSNorm}(h_t^{\mathrm{corr}}). \tag{8}$$

$U \in \mathbb{R}^{d \times w}$ is initialised at zero and $s > 0$ is a fixed scalar set once at the start of editor training to the measured root-mean-square of the backbone residual stream (Section 6); because the final RMSNorm is scale invariant, s only reparametrises the learning rate of U. When $n_t = 0$, the term (8) is algebraically zero; the implementation additionally branches out before allocating any slot when the budget is zero, so the fast path performs no workspace computation at all (Proposition 2).

4.6. Configurations and parameter accounting

5. Adaptive intervention policy

The deployed router is a *value controller* that scores every nonterminal action from target-free features and never executes a candidate edit. At state $s = (z_t, S, r, \rho)$ it computes one shared state representation $u = \mathrm{MLP}(W_z z_t + \mathrm{ctx}(r/R_{\max}, \rho/R_{\max}))$ and, for each action a,

$$\bar{s}_a = \frac{1}{k(a)} \sum_{j \in I(a)} S_j, \qquad \big(\hat{g}(s,a), \hat{c}(s,a), \log \hat{\sigma}(s,a), \hat{\ell}_{\mathrm{harm}}(s,a)\big) = W_{\mathrm{out}}\, \mathrm{SiLU}\big(u + W_s \bar{s}_a + \kappa_{k(a)}\big) + b, \tag{9}$$

Table 1. Dimensioned configurations and exact unique parameter counts (meta-device instantiation, `scripts/count_params.py`). Backbone counts equal the reference design exactly; workspace+controller counts differ from the proposal's figures (reconciled in Appendix C).

Property	PRISME-103M	PRISME-459M	PRISME-1.18B
Backbone layers L	12	16	24
Hidden d / FFN	768 / 2048	1536 / 4096	2048 / 5632
Query / KV heads	12 / 3	24 / 6	32 / 8
Workspace w / FFN_w	256 / 768	512 / 1536	768 / 2048
Slots M, blocks L_w, cap $R_{\max}$	16, 2, 8	16, 2, 8	16, 2, 8
Backbone parameters	99,502,848	446,744,064	1,149,339,648
Workspace + controller	2,631,173	10,505,221	21,655,813
Total	**102,134,021**	**457,249,285**	**1,170,995,461**
Proposal's workspace figure	3,088,260	12,336,900	25,779,844

so that scoring all $|\mathscr{A}|$ actions costs one MLP plus $|\mathscr{A}|$ projections of width w (Section 8). The four heads estimate the *intervention gain* $g(s,a)$ of Section 6, a cost proxy, a predictive scale, and the probability that the gain is negative. Actions are chosen greedily,

$$Q(s,a) = \hat{g}(s,a) - \lambda\, c(a) - \kappa\, \hat{\sigma}(s,a), \qquad Q(s,\text{STOP}) = 0, \qquad a^\star = \arg\max_{a \in \mathscr{A} \cup \{\text{STOP}\}} Q(s,a), \quad (10)$$

where $c(a)$ is the *known* analytic cost of the action (ops of one edit plus one controller evaluation, relative to the backbone's ops per token; Section 8); the learned $\hat{c}$ is kept only as a diagnostic regressor. The state is recomputed after every executed edit, and the loop stops when STOP wins or the cap $R_{\max}$ is reached. The penalties (λ, κ) are tuned on calibration rollouts only (Section 9); sweeping λ traces the controller's quality–cost curve.

Known limitations of the rule. The policy is one-step greedy: it cannot represent an action whose value materialises only after a later action. We quantify this myopia directly with a two-action lookahead oracle (Section 10.3) rather than assume it away. The rule is also only as good as the calibration of $\hat{g}$; Proposition 5 bounds the one-step loss of the greedy rule in terms of uniform estimation errors but says nothing about multi-step optimality.

6. Learning from interventions

6.1. Paired intervention-value target

For a training state s (a position t with workspace S after r executed actions), an action a and the known next token $y = x_{t+1}$, let $D(\cdot)$ denote the readout-plus-head map of (8) and T_a the editor's

transition. The intervention gain is

$$g(s,a;y) = \mathscr{L}\big(D(s),y\big) - \mathscr{L}\big(D(T_a(s)),y\big), \tag{11}$$

obtained by cloning s, executing a with the same checkpoint, and evaluating both branches on the same y. A positive gain means the edit reduced the teacher-forced loss on this example. This is an intervention on the model's own computation, not a causal effect in the world, and a token-level gain does not imply that a complete generated answer improves (Section 10.5). The label y enters (11) only; the runtime features of (9) never contain it (verified by a unit test that perturbs x_{t+1} and checks that all earlier predictions are unchanged).

6.2. Logging policy and propensities

On sampled training states we evaluate four candidates: the current controller's preferred action (or a random single if no controller exists yet), a competitive alternative from the other group (a pair if the preferred action is a single and vice versa), and two uniform draws (one single, one pair). The logging mixture's propensity of each candidate is recorded, enabling inverse-propensity or doubly-robust corrections of selection bias (Dudík et al., 2011; Swaminathan and Joachims, 2015). On an audit subset every one of the 24 actions is executed, giving exact one-step oracle values. Helpful and harmful gains are both kept; sibling branches of one state never cross a partition boundary; every label file records the SHA-256 of the editor checkpoint that produced it, and the controller trainer refuses labels from a different checkpoint.

6.3. Staged optimisation

Stage A (backbone). A common backbone checkpoint is trained (or, for the synthetic experiments, trained from scratch on the synthetic train split). Every comparison starts from the same weights, tokenizer and data.

Stage B (editor bootstrap). The backbone is frozen; the editor, bank projections, slot embeddings and readout are trained with the language loss under *forced* action sequences: each position draws a budget $b \in \{0,1,2,4,8\}$ and b uniformly random actions (singles and pairs balanced), later steps being STOP. Because $U = 0$ blocks all gradient into the editor at initialisation, the bootstrap requires a symmetry-breaking device; we study the zero initialisation, a non-zero initialisation and the scale s of (8) explicitly (Section 10.1).

Stage C (counterfactual collection). With the editor fixed, states are sampled by executing $r \sim U\{0,\dots,3\}$ random actions, and labels (11) are collected as above.

Stage D (controller fit). The controller minimises

$$\mathscr{L}_{\text{ctrl}} = \alpha\,\text{Huber}_{\delta}\big(\hat{g}, \text{sg}(g)\big) + \beta\,\text{BCE}\big(\hat{\ell}_{\text{harm}}, \mathbf{1}[g<0]\big) + \gamma\Big(\tfrac{(g-\text{sg}(\hat{g}))^2}{2\hat{\sigma}^2} + \log\hat{\sigma}\Big), \tag{12}$$

on the training partition; sign accuracy, Brier score, expected calibration error and oracle regret are measured on the calibration partition, where (λ, κ) are also selected by actual rollouts.

Stage E (lock). The final checkpoint (editor + controller + selected penalties) is frozen before any locked test split is read. Alternating editor/controller refinement is treated as a later ablation, not a dependency.

No teacher model and no distillation are used anywhere in this paper; the composite loss of the original proposal reduces to the language loss in Stage B and (12) in Stage D.

6.4. Delayed benefits

An action may have negative immediate gain yet enable a later beneficial action. We therefore report, on audit subsets, the exhaustive one-step oracle, the exhaustive two-action oracle ($|\mathscr{A}|(|\mathscr{A}|+1)$ branches per state) and the "greedy first, then best" value, and decompose the gap between the learned policy and the two-step oracle into an *opportunity* term (two-step minus one-step oracle) and a *prediction* term (one-step oracle minus learned policy).

7. Theoretical properties

The statements below are structural properties of the computation graph, not performance guarantees. Proofs are in Appendix B; each is accompanied by a numerical property test in `tests/unit/` that exercises the implemented code on random inputs (tests are evidence about the implementation, not proofs).

Assumption 1 (prefix causality of the components)**.** B_ϕ is causal; the bank visible to a query at position t is $\{(K_j, V_j)\}_{j\le t}$; the controller features at position t are functions of (z_t, S, r, ρ) only.

Proposition 1 (causality)**.** *Under Assumption 1, for every executed action sequence the corrected logits ℓ_t are a function of $x_{1:t}$ only, for all t and for any number of edits $n_t \le R_{\max}$.*

Proposition 2 (identity and exact bypass)**.** *If $n_t = 0$ then $\Delta_t = 0$ exactly and $\ell_t = \ell_t^{\text{bb}}$ in exact arithmetic. In floating point, the implementation returns the backbone head through an explicit branch that performs no workspace computation when the budget is zero, and through the same branch for positions with no executed edit, so the equality is bitwise for identical execution settings. After joint fine-tuning the identity holds relative to the* current *backbone, not to any earlier checkpoint.*

Proposition 3 (exact locality and order independence)**.** *For an action with active set I, $S'_m = S_m$ bitwise for all $m \notin I$, and the result of a pair action does not depend on the order in which its two lanes are processed.*

Proposition 4 (bounded displacement)**.** *With $\mathrm{rms}(v) = \|v\|_2/\sqrt{w}$, after n_j writes into slot j, $\mathrm{rms}\left(S_j^{(R)} - S_j^{(0)}\right) \le n_j\,\eta_{\max}\,c$. The bound is a consequence of clipping and of the triangle inequality; it does not imply convergence of the edits or any decrease of the loss.*

Proposition 5 (one-step selection regret)**.** *Fix a state s and let $J(a) = g(s,a) - \lambda c(a)$ for $a \in \mathscr{A}$ and $J(\mathrm{STOP}) = 0$, with estimates $\hat{J}(a) = \hat{g}(s,a) - \lambda \hat{c}(a)$. If $|\hat{g}(s,a) - g(s,a)| \le \varepsilon$ and $|\hat{c}(a) - c(a)| \le \delta$ for all feasible a, then the greedy choice $\hat{a} = \arg\max \hat{J}$ satisfies $\max_a J(a) - J(\hat{a}) \le 2(\varepsilon + \lambda\delta)$. With the known cost used in this paper, $\delta = 0$. Adding the risk term $-\kappa\hat{\sigma}$ changes the objective and the bound no longer applies as stated.*

Remark 1. None of the propositions bounds the multi-step value of a trajectory; the greedy rule can be arbitrarily sub-optimal on states where the best first action has negative immediate gain. Section 10.3 measures that opportunity directly.

8. Implementation and compute model

8.1. Reference implementation

The reference implementation is a self-contained PyTorch package (`prisme/`, about 1,100 lines) with the backbone, bank, workspace, editor, controller, readout, caches, counterfactual labelling, oracles and the analytic cost model, plus training, evaluation and baseline packages. In teacher-forced training all T positions of a batch are processed in parallel: the workspace is a tensor of shape $[B,T,M,w]$, the active queries a tensor $[B,T,k,w]$ with $k = 2$ lanes (the second lane is masked for single-slot actions), and the bank cross-attention of (4) is one scaled-dot-product call with a per-query causal mask over the T bank rows. Incremental decoding keeps two caches per request batch—the per-layer Transformer KV cache and the bank (K,V)—and is tested to agree with full-prefix recomputation within 10^{-4} under forced budgets, learned routing, early STOP and mixed edited/unedited positions; the bypass path is bitwise identical to the backbone.

8.2. Analytic operation counts

We count one multiply–add as two operations and ignore normalisations, activations and softmaxes. For a decode step at context length T the backbone costs $2N_{\mathrm{ne}} + 4LTd + 2dV$ operations (N_{ne} the non-embedding parameters; the vocabulary head is included). One editor block with k active lanes

Table 2. Analytic operations per generated token, PRISME-459M, context $T = 4096$ (`scripts/estimate_flops.py`). "Proposal" is the reference table of the original brief; "implemented" is the accounting of Section 8 with the controller evaluated once per executed action plus once to stop. These are arithmetic counts, not measured timings.

Policy	Proposal (10^9)	Implemented (10^9)	editor	controller	readout
Backbone only	1.296	1.296	0.000	0.000	0.000
4 actions, 1 slot each	1.645	1.666	0.256	0.090	0.019
4 actions, 2 slots each	1.767	1.787	0.378	0.090	0.019
8 actions, 2 slots each	2.195	2.219	0.756	0.143	0.019
8 actions, dense 16-slot writes	5.608	5.633	4.169	0.143	0.019

costs

$$F_{\text{block}}(k,T) = \underbrace{4kw^2 + 4kTw}_{\text{bank read}} + \underbrace{4kw^2 + 4Mw^2 + 4kMw}_{\text{slot read}} + \underbrace{6kwf_w}_{\text{FFN}}, \tag{13}$$

which is exactly the arithmetic model of the original proposal; the slot-key refresh $4Mw^2$ dominates for small k and is the obvious target for a cached kernel. One controller evaluation costs $2w^2 + 4w + 6wf_w + |\mathscr{A}|(2w^2 + 8w)$ and the readout $2w^2 + 2(2Mw^2 + 4Mw) + 2wd$. Table 2 reproduces the reference table of the proposal for the 459M configuration at $T = 4096$ and reports the implemented model's accounting; the two agree within 1.3 % (the proposal's controller and readout terms were slightly smaller). The per-action cost proxy used by the controller is $c(a) = \big(L_w F_{\text{block}}(k(a),T) + 8k(a)w + \text{one controller evaluation}\big)/\text{backbone ops}$; at the 103M configuration and $T = 512$ it is 0.080 for a single and 0.100 for a pair, so eight pair edits cost about +83 % of the backbone's operations and dense sixteen-slot writes about +310 %.

8.3. Memory

Parameters at two bytes each: 0.204 GiB (103M), 0.852 GiB (459M), 2.18 GiB (1.18B). Per request at context T: Transformer KV cache $2 \cdot L \cdot T \cdot d_{kv} \cdot 2$ bytes (96 MiB at 459M, $T = 4096$), bank $2 \cdot T \cdot w \cdot 2$ bytes (8 MiB), one workspace $M \cdot w \cdot 2$ bytes (16 KiB). The bank grows linearly with the context: PRISME v1 is not a constant-memory model, and the bank read in (13) is linear in T per active lane. Training stores one workspace per position ($B \cdot T \cdot M \cdot w$ floats) per recurrence step; measured peak memory is reported with every run.

8.4. Latency protocol

Latencies are measured for batch-1 and batch-8 greedy decoding with warm-up, device synchronisation after every token, fp32, a stated prompt length and number of generated tokens, and the cache state recorded (Section 10.7). Dynamic routing introduces host-side control flow at every

Table 3. Synthetic suite v1: splits fixed before sampling. n = items after cross-split exact-duplicate removal; "dropped" = duplicates removed. Test splits are locked until pre-registration is frozen.

Split	Families	Templates	Depths	n	dropped
train	arith, bind, fsm, excl, sched	0,1,2	1,2,3,4	23,005	995
calib	arith, bind, fsm, excl, sched	0,1,2	1,2,3,4	1,690	110
dev_iid	arith, bind, fsm, excl, sched	0,1,2	1,2,3,4	3,344	256
dev_template	arith, bind, fsm, excl, sched	3,4	1,2,3,4	2,373	27
dev_depth	arith, bind, fsm, excl, sched	0,1,2	5,6	1,800	0
dev_compose	compose	0,1,2	3,4,5	540	0
test_iid	arith, bind, fsm, excl, sched	0,1,2	1,2,3,4	3,316	284
test_template	arith, bind, fsm, excl, sched	3,4	1,2,3,4	2,328	72
test_depth	arith, bind, fsm, excl, sched	0,1,2	5,6	1,800	0
test_compose	compose	0,1,2	3,4,5	540	0
ctrl_train	arith, bind, fsm, excl, sched	0,1,2	1,2,3,4	11,030	970

action; this overhead is part of the measurement and is not removed.

9. Experimental design

9.1. Data

Synthetic compositional suite. Six generator families with programmatic ground truth (Table 3): multi-step arithmetic (arith), variable binding with dependent substitution and a distractor assignment (bind), register updates (fsm), conjunctive constraints with distractor items (excl), rule-based selection under a budget and an exclusion (sched), and a held-out composition of a binding chain feeding an arithmetic expression (compose). Each family has five phrasing templates; training uses templates 0–2 at depths 1–4 and a dedicated seed stream. Evaluation regimes: *iid* (training templates and depths, new seed), *template* (held-out paraphrases 3–4), *depth* (depths 5–6), and *compose* (held-out family). Splits are fixed *before* sampling by (family, template, depth, seed); items whose full text duplicates an item of an earlier split are dropped and counted (995 train, 256 dev-iid, 284 test-iid duplicates; `data/splits/synthetic_v1/manifest.json`). A separate *calibration* stream (seed 3) is used only for controller calibration and penalty selection. Test streams (seed 9) are locked: the evaluation driver refuses to read them until `protocol/locked_test_manifest.json` records the unsealing.

General language corpus. For the 103M experiments we stream the first documents of FINEWEB-EDU (sample-10BT, ODC-By 1.0; Penedo et al., 2024) in canonical shard order, deduplicate exactly, assign documents to train/val/test by content hash (98/1/1 %), and train a byte-level BPE tokenizer (32,768 symbols, digits split individually) on the train split only. The corpus holds 290.0M train,

2.91M validation and 2.88M locked test tokens (`data/processed/fineweb_edu_300m/manifest.json`).

9.2. Systems and controls

All systems in a comparison share the backbone checkpoint, tokenizer, data stream, step count and batch size. Table **??** lists every system with its unique parameter count, training exposure and inference regime. The parameter-matched plain decoder (feed-forward width 2144 instead of 2048, +2.65M parameters) is trained from scratch with the backbone recipe. The dense-slot editor writes all M slots per action with the same weights. The global-recurrence adapter (Section 2) refines the whole top representation with one shared block at width w, parameter-matched to the PRISME workspace, trained with the identical forced-budget recipe and the identical symmetry-breaking device. Reference routers (random, uniform round-robin, fixed pair, entropy threshold, norm-change threshold) reuse the trained PRISME editor and are therefore matched in everything except the choice of action. The exhaustive one-step and two-step oracles read the target and are reported as ceilings only.

9.3. Pre-registration, seeds and uncertainty

The primary outcome, metrics, decision thresholds and seed plan were written to `protocol/preregistration.md` before any test split was read (Appendix E). Differences between systems on the same items use a paired bootstrap over items (10,000 resamples); differences across training seeds use the per-seed statistic with a t-interval. Tokens of one training run are never treated as independent replications of training. Latency is measured on the stated device with warm-up and synchronisation.

10. Results

All numbers in this section are generated from result files by `scripts/make_tables.py` and are indexed in `results/claim_registry.csv` with their run identifiers. Unless stated otherwise, "loss" is the teacher-forced cross-entropy on the answer span of the synthetic suite (nats per token), "b" a forced budget applied to every position with uniformly random actions, and relative changes are with respect to the frozen backbone (budget 0) of the same run.

10.1. Learnability and the bootstrap problem (Phase 4)

With the readout initialised at zero, as prescribed, the editor did not learn at all in 700 steps on the frozen 2.1M-parameter backbone: validation loss was identical at every budget (0.4144, 0.4143, 0.4143, 0.4146, 0.4164) and the trainable gradient norm stayed at 0.01–0.02. The cause is a scale mismatch: the backbone residual stream has root-mean-square 5.6 while the difference of

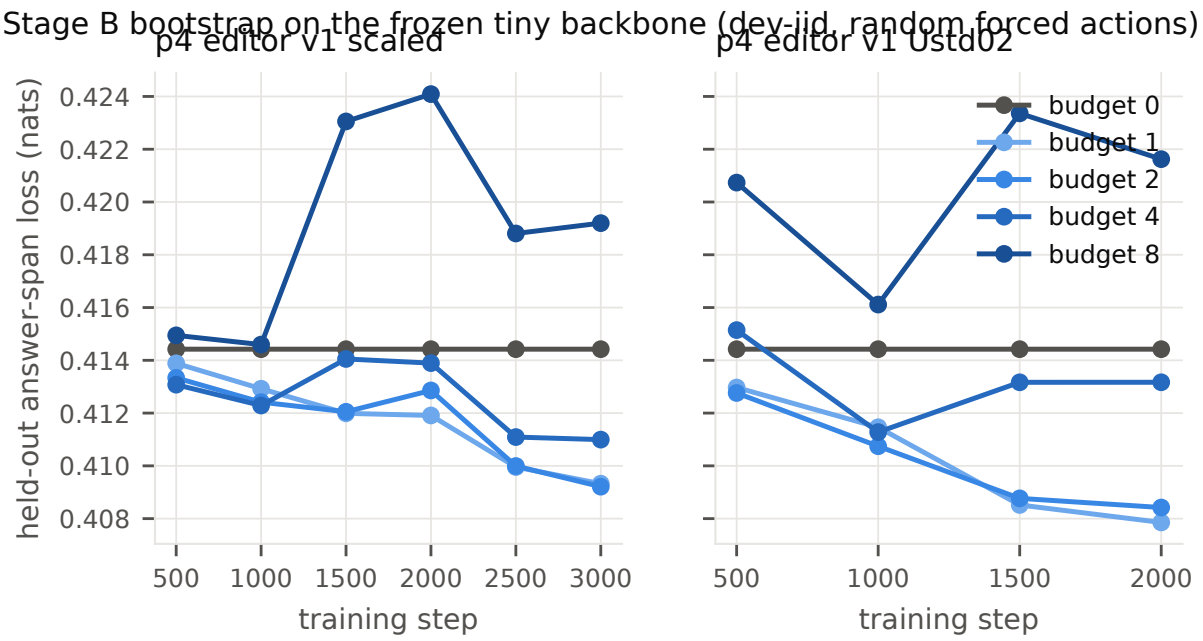


Figure 2. Stage B bootstrap on the frozen tiny backbone (dev-iid answer-span loss under random forced actions, budgets 0–8). Left: zero-initialised U with the scale s; middle and right: $U \sim \mathcal{N}(0, \sigma^2)$ with $\sigma = 0.2$ and 0.5. Budget 0 is the frozen backbone (identical in all panels).

the two attention reads has root-mean-square 0.06, so U must grow by two orders of magnitude before any edit can move a logit, and until it does the editor receives no gradient. Three remedies were compared under the identical recipe (Figure 2): the fixed scale s alone (zero U), s with $U \sim \mathcal{N}(0, 0.2^2)$, and s with $U \sim \mathcal{N}(0, 0.5^2)$. Only the non-zero initialisations learned within 2,000 steps (dev-iid loss $-1.4\,\%$ at budgets 1–2 for $\sigma = 0.2$); with $\sigma = 0.5$ the budget-8 loss was worse than the backbone throughout, and a learning rate of 3×10^{-3} with $w = 128$ collapsed to inert edits (every budget exactly equal to budget 0, gradient norm 0.00). The deliberate-overfitting test passed: on 64 items at budget 8 the frozen-backbone model reached a training loss of 0.0024 in 200 steps through the workspace path alone while the held-out loss rose, confirming that the gradient path and the capacity exist. Failure mode 13.1 of the proposal is therefore confirmed, and $U \sim \mathcal{N}(0, 0.2^2)$ with $s = \mathrm{rms}(H)$ is used for every adapter below, including the baselines.

10.2. Sparse editor versus global recurrence and dense writes (H1, H2)

Table 4 reports the pre-registered decisive comparison on the second tiny backbone (5.6M parameters, char-level): 5 independent adapter seeds for the sparse editor, 5 for the global-recurrence adapter and 5 for the dense sixteen-slot editor, all trained with the same forced-budget recipe for 3,000 steps. H1 is supported in its weak form: random sparse edits reduce the held-out answer-span loss by 2.1 % (budget 1), 2.8 % (budget 2) and 3.0 % (budget 4), with seed standard deviations

Table 4. Phase 4 decisive comparison on the frozen 5.6M-parameter synthetic backbone (char-level): held-out answer-span loss on dev-iid under random forced actions at fixed budgets, and free-generation exact match (dev-iid, 1,000 stratified items). Mean ± SD over independent training seeds of the adapter; the backbone, data, recipe and symmetry-breaking device are identical. Budget 0 is the frozen backbone for every system.

System	seeds	loss b=0	loss b=1	loss b=2	loss b=4	
PRISME sparse editor	5	0.4621 ± 0.0000	0.4525 ± 0.0006	0.4491 ± 0.0008	0.4483 ± 0.0015	0.45
Global recurrence (inspired)	5	0.4621 ± 0.0000	0.4454 ± 0.0013	0.4433 ± 0.0023	0.4449 ± 0.0033	0.44
Dense 16-slot writes	5	0.4621 ± 0.0000	0.4509 ± 0.0011	0.4476 ± 0.0020	0.4480 ± 0.0034	0.45
Exact match (%), dev-iid: b=0 / b=1 / b=2 / b=8						
PRISME sparse editor	5	37.7 ± 0.0 / 39.3 ± 0.3 / 39.6 ± 0.3 / 39.2 ± 0.5				
Global recurrence (inspired)	5	37.7 ± 0.0 / 39.8 ± 0.3 / 39.6 ± 0.4 / 40.0 ± 1.1				
Dense 16-slot writes	5	37.7 ± 0.0 / 39.8 ± 0.3 / 39.7 ± 0.3 / 39.0 ± 0.5				
Exact match (%), held-out template / depth / compose at b=2						
PRISME sparse editor	5	27.7 ± 0.6 / 29.4 ± 0.2 / 3.1 ± 0.3				
Global recurrence (inspired)	5	27.6 ± 0.3 / 29.9 ± 0.3 / 2.8 ± 0.2				
Dense 16-slot writes	5	28.0 ± 0.5 / 29.5 ± 0.2 / 3.1 ± 0.1				

below 0.002 nats, and raise dev-iid exact match from 37.7 % to 39.6 %. The effect is not specific to sparse local writes, however: the global-recurrence adapter—one shared block rewriting the whole latent, parameter-matched, same bootstrap—reduces the loss by 3.6 %, 4.1 % and 3.7 % at the same budgets and reaches 39.6 % exact match, i.e. it is at least as good at every budget and better at budget 8, where the sparse editor's gain shrinks to 1.0 %. Dense writes answer the second half of H2 directly: rewriting all sixteen slots at every action with the same editor weights (5 seeds) lowers the loss by 2.4, 3.1 and 3.1 % at budgets 1, 2 and 4—slightly *more* than the sparse one-or-two-slot writes at every budget, at roughly eight times the editor arithmetic—and gives the same exact match within noise (Table 8). Sparse local writing therefore buys nothing in quality at this scale; its only potential advantage is cost, and Section 10.7 shows that this advantage does not materialise in measured latency either. None of the adapters improves the held-out template, depth or compose regimes by more than one point (H6). Figure 3 places the same runs on the quality-versus-analytic-cost plane: the global adapter is both cheaper per step (one block at width w over the whole latent costs less than two shared blocks with slot re-keying for two lanes) and better at every budget, so it dominates the sparse editor along the entire frontier; H4 is refuted at this scale for the comparison the proposal considered most important.

10.3. Oracle opportunity and the structure of intervention gains (Phase 5)

Figure 4 and Table 6 summarise the counterfactual labels collected with the fixed editor on a stream the editor never trained on (12,272 and 1,645 exhaustive-audit states in the training and calibration partitions). Three facts shape everything downstream. First, the exhaustive one-step oracle finds

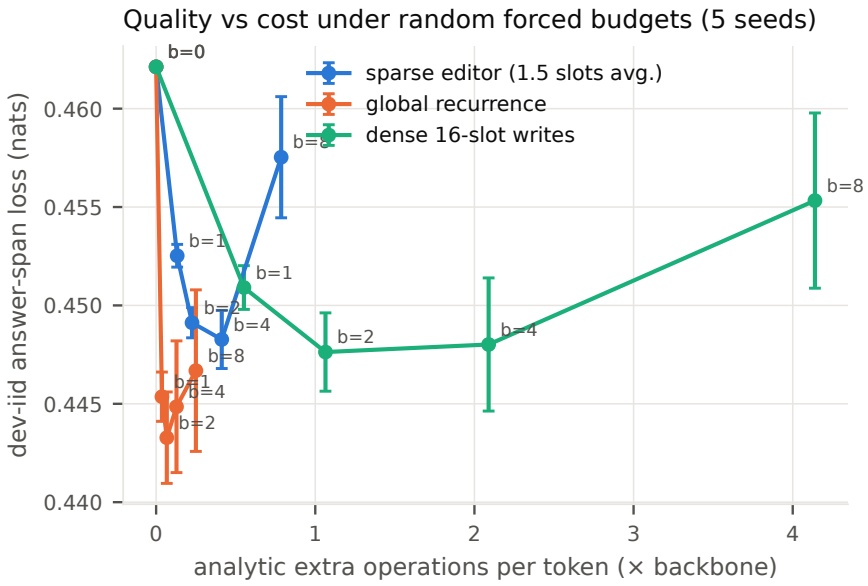


Figure 3. Quality versus analytic extra operations per token at the tiny scale (five adapter seeds, mean ± SD), random forced budgets $b \in \{0,1,2,4,8\}$, context 160. Costs from `prisme/flops.py`; the sparse editor's cost is the average of single and pair actions.

real value: choosing the best of the 24 actions, or stopping when none helps, gains 0.0136 nats per state on calibration against -0.0014 for a random action; rolled out greedily on dev-iid it lowers the loss by -7.1 % with 0.52 edits per token at budget 1 and by -15.4 % with 2.27 edits per token at budget 8 (Table 5). The two-action oracle gains 0.0340 nats against 0.0233 for one step, and "greedy first action then best second" recovers almost all of it, so myopia of the one-step rule is not the binding constraint at this scale. Second, most of the variance of the gains is between states, not between actions: 25 % of the variance of the audited gains is within-state (action identity), the rest is the state's mean gain; 34 % of states have all 24 actions positive and 31 % have all 24 negative. Decomposing the oracle, a perfect continue/stop decision with a random action captures 0.0069 of the 0.0136 nats; the choice of *which* slot captures the rest. Third, opportunity is concentrated where the backbone is uncertain and is dangerous there: in states above the median base loss the oracle gains 0.0272 nats while a random action loses -0.0028; in the top decile of base loss the oracle gains 0.027 and a random action loses -0.041 nats, with only 58 % of actions helpful (22 % in the lower half, where all gains are negligible).

10.4. Learned routing versus reference routers (H2, H3)

Table 5 compares, on dev-iid with the same editor, the frozen backbone, random, round-robin and fixed-pair schedules at budgets 1–8, entropy and norm-change routers over a threshold grid, the learned value controller at its locked penalty and along a λ grid, and the diagnostic oracles.

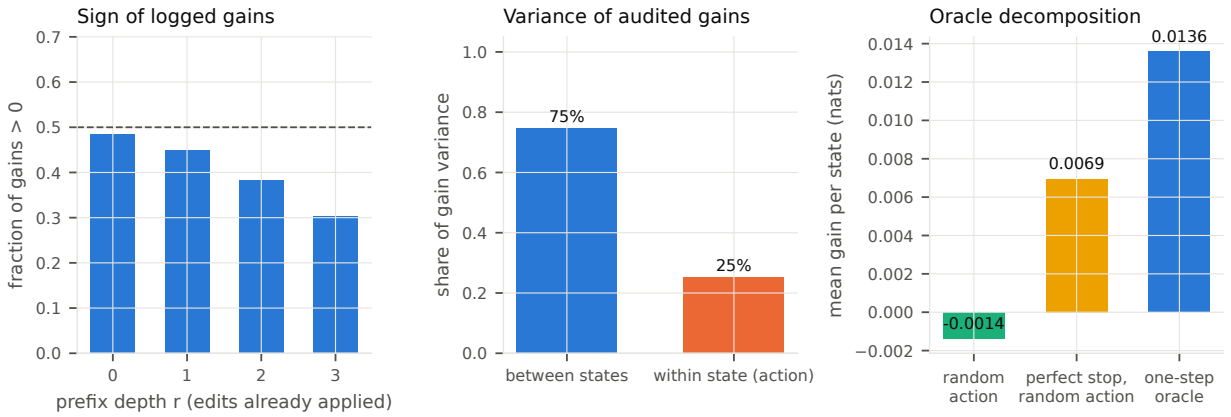


Figure 4. Counterfactual labels (calibration partition, editor-unseen stream). Left: fraction of positive gains by number of edits already applied. Middle: variance decomposition of the exhaustive 24-action audit. Right: mean gain per state of a random action, of a perfect continue/stop decision with a random action, and of the one-step oracle.

Three results follow. (i) Spatial selection does not matter for fixed schedules: at budget 1 random, round-robin and fixed-pair give -0.7, -0.8 and -0.7 % respectively, within the paired bootstrap noise of one another; more edits hurt (+2.7 % at budget 8). H2 is not supported. (ii) The learned controller is worse than not routing: at its calibration-selected penalty ($\lambda = 0.3$) it edits 0.22 times per token and *raises* the dev-iid loss by +1.0 %; every smaller λ is worse still, although the same controller had lowered the calibration loss by 1.1 % (Table 6). The reference routers do no better: the best entropy threshold gives +0.3 % and the best norm-change threshold -0.6 %. H3 is refuted at this scale. (iii) The mechanism of the failure is visible in the labels. With backbone-residual and entropy features the controller predicts the *sign* of a gain with 60.4 % accuracy (chance 50 %) and finds the states with opportunity, which are the high-loss states; but in exactly those states a wrong action costs more than the right action gains (-0.041 versus 0.027 nats in the top decile), and the controller's ranking of actions within a state is close to chance (top-1 agreement with the oracle 7–8 % against 4.2 % for a uniform guess; captured fraction of the oracle opportunity ≤ 5 %). The pre-registered null "an oracle can find beneficial edits, but the learned controller cannot predict them" is the outcome at this scale. (iv) The result replicates across adapter seeds on the second backbone (Table 7): over 5 seeds the locked controller changes the dev-iid loss by +0.3 % (SD 1.0) while editing a small fraction of tokens, the best fixed schedule lowers it by -4.2 % and the greedy oracle by -15.1 %; the controller beats the frozen backbone on 3 of 5 seeds and never approaches the fixed schedules at any cost. (v) Adding within-state ranking losses (pairwise on the logged candidates, listwise on the exhaustive audits; Appendix H) did not change the picture: the locked policy edits 0.07 tokens and matches budget 0 (0.4333 vs 0.4334), and every smaller penalty is worse.

10.5. Token loss versus task success

Exact match of free greedy generation moves with the loss on dev-iid but by less than the pre-registered minimum effect of three points (Table 8): the sparse editor gains $+1.9$ points at budget 2 (95 % t-interval over five seeds $[+1.6, +2.2]$; every per-seed paired item bootstrap excludes zero), global recurrence gains $+1.9$ at budget 2 and $+2.3$ at budget 8, and the paired difference between the two systems at budget 2 is $+0.1$ points $[-0.5, +0.7]$—a null—while at budget 1 global recurrence is ahead by 0.6 points $[0.3, 0.9]$. Transitions are two-sided: at budget 2 the sparse editor turns 39 items per thousand from wrong to right and 20 from right to wrong (Table 15); gains concentrate in fsm and bind, excl does not move. No adapter changes the held-out regimes by more than a point, and the compose family stays at 2–3 %.

10.6. Pre-registered primary outcome on the locked test regimes

The test streams (seed 9) were unsealed on 2026-10-30 after the pre-registration was frozen (Appendix E); every system was evaluated once, at budget 0 and at forced random budget 2, on 1,000 stratified items per regime (Table 9). The pre-registered primary outcome—pooled exact match over the iid, template and depth regimes at budget 2 versus the frozen backbone—is $+0.9$ points for the sparse editor (95 % t-interval over 5 seeds $[+0.9, +1.0]$), far below the pre-registered minimum effect of three points; against the global-recurrence adapter the paired difference is -0.3 points $[-0.6, +0.1]$ and against the dense editor $+0.4$ points $[+0.1, +0.7]$. The compose regime stays at 4–5 % for every system. By the decision rule fixed in advance, the architecture does not advance: it neither exceeds the strongest matched baseline by three points at comparable cost nor reduces cost at non-inferior quality.

10.7. Latency and memory

Table 10 gives measured batch-1 and batch-8 decode latencies for the tiny model on the development laptop (Apple M5 Max, fp32, PyTorch 2.15, MPS). Budget 0 costs 2.80 ms per token; each executed single-slot edit adds about 2.3 ms, so eight edits cost seven times the backbone although their analytic operation count is only 4.2 times; the dense sixteen-slot write at budget 8 is *not* slower than eight sparse writes (17.3 vs 19.3 ms), and one controller evaluation adds 2.9 ms even when it stops immediately. At this scale the per-action host dispatch and kernel-launch overhead dominates the arithmetic, exactly as the proposal's failure mode 13.6 anticipated; Section 10.8 repeats the measurement at 103M, where the arithmetic is 60 times larger per token.

10.8. The 103M pilot on web text (Phase 8)

Stage A. The 103M backbone (Table 1) was pretrained on 98.3M tokens of FineWeb-Edu (6,000 steps of 32×512, cosine schedule, fp32) on one Apple M3 Ultra in 2.3 hours (13.8k tokens/s, peak 28 GB, $6NP \approx 5.9 \times 10^{16}$ FLOPs), reaching a validation perplexity of 48.2. This is one token per parameter—far from compute-optimal and one-half of the budget announced in the proposal's progression (a stale configuration on the node, Appendix H); the parameter-matched plain decoder (feed-forward width 2144, 102.16M) was trained with the identical recipe (validation perplexity 47.9, 2.4 h). Only one backbone seed was affordable.

Stage B. On the frozen backbone, the sparse editor (1,907,713 trainable parameters with the residual-plus-entropy controller features, $s = \mathrm{rms}(H) = 8.85$) and the global-recurrence adapter (2,623,488) were trained for 3,000 steps of 16×512 tokens with the forced-budget recipe. At the learning rate that worked at the tiny scale (10^{-3}) the sparse editor collapsed to inert edits within 500 steps (every budget identical to budget 0, gradient norm zero); at 3×10^{-4} it trained (90 min, peak 25.9 GB) but the effect on held-out web text is negligible: -0.04, -0.06, -0.08 and $-0.06\,\%$ of loss at budgets 1, 2, 4 and 8. The global adapter, which did not collapse at 10^{-3}, gives -0.04, -0.06, -0.08 and $-0.07\,\%$ in 21 min. The dense sixteen-slot probe collapsed at both learning rates and yields no 103M result. On general web text, therefore, neither adapter moves a frozen 100M backbone by more than a tenth of a percent of loss with 24.6M adapter tokens—less than the run-to-run noise of pretraining itself—so H1 is not supported outside the synthetic suite, and the parameter-matched plain decoder is the relevant comparison (Table 11).

Stages C–D and routing. Counterfactual labels were collected on a validation-disjoint stream of web text and a controller fitted with the tiny-scale recipe (Table 12). The locked controller changes the validation loss by -0.01 % (0.61 edits per token); the one-step oracle reaches -3.6 % with 4.28 edits per token, and the best fixed schedule (round-robin, budget 1) -0.02 %. The picture of Section 10.4 is unchanged at 103M: there is oracle opportunity (now on natural text), and the controller does not capture it.

Latency at 103M. Table 13 repeats the latency protocol with the 103M model on the M3 Ultra (prompt 256, 32 generated tokens, fp32). Budget 0 costs 7.6 ms per token at batch 1; eight single-slot edits 22.9 ms; eight dense writes 26.9 ms; the controller with its locked penalty 11.0 ms. The sparse-versus-dense gap is now visible but remains far below the analytic ratio ($3.1\times$ more editor operations for dense writes), confirming that at this size the per-action dispatch, the slot re-keying and the bank read—not the lane arithmetic—set the cost.

10.9. Larger configurations

The 459M and 1.18B configurations were instantiated on the meta device for exact parameter accounting (Table 1) and the 459M operation table was reproduced (Table 2); neither was trained. No scaling claim is made (H7 untested).

11. Mechanistic studies and ablations

Where the gains are. The variance decomposition of Section 10.3 is the central mechanistic fact of this study: for the trained editor, whether an edit helps is mostly a property of the *state* (34 % of audited states have every action helpful, 31 % every action harmful), and only 25 % of the variance of gains is attributable to which slot is written. Opportunity is concentrated in high-loss states, where the spread between the best and a random action is largest (0.027 versus -0.041 nats in the top decile). A router that can only detect opportunity but not rank actions therefore edits exactly where editing is riskiest—which is what the learned controller did.

Harm and overthinking. Across fixed schedules the loss is U-shaped in the budget (Table 4): one or two random edits help, eight hurt, for both the sparse editor and (less so) the global adapter; the fraction of positive logged gains falls from 48 % at the first edit to 37–40 % after three (Figure 4, left). Edits compound noise faster than they compound signal in the forced-training regime, and nothing in the forced recipe teaches the editor to leave a good state alone.

Slot usage and fixed pairs. In the exhaustive audits no slot or pair is preferred: the best action's identity agrees between consecutive states 11–12 % of the time (uniform guess 4.2 %), singles and fixed pairs have indistinguishable mean gains on the editor-unseen stream, and the round-robin schedule, which visits every slot, matches random selection. The address embeddings e_j therefore do not induce functional specialisation in 3,000 steps of forced training; we make no interpretability claim about slots.

Myopia. The two-action oracle exceeds the one-step oracle by 0.0340 versus 0.0233 nats, but "greedy first action, then best second" recovers nearly all of it; the one-step greedy rule is not what limits the learned controller.

Cost decomposition. Analytically (Section 8), at the 103M configuration and $T = 512$ a single-slot edit costs 0.080 and a pair 0.100 of the backbone's operations per token, of which the slot-key refresh of all M slots is the largest single term; measured at the tiny scale, host dispatch dominates (Section 10.7).

Ablations executed. Readout initialisation (zero vs $\sigma \in \{0.2, 0.5\}$), readout scale, editor width ($w \in \{64, 128\}$), learning rate (10^{-3} vs 3×10^{-3}), budget schedule (mixed vs budget-8 only), write locality (1–2 slots vs all 16), global vs local recurrence, controller features (z vs $\hat{h}$ + entropy), label stream (editor-seen vs editor-unseen), penalties (λ, κ). Not executed: $M \in \{4, 8, 32\}$, editor depth $\in \{1, 4\}$, rematched pairs, joint fine-tuning (one attempt degraded the backbone and was stopped), distillation, quantisation. All executed runs, including failed ones, are listed in Appendix F.

12. Threats to validity and limitations

Scale. The decisive seeded comparisons were run on 2–6M-parameter character-level backbones trained on a synthetic suite; the 103M experiments (Section 10.8) use a single backbone seed and a 196M-token budget (two tokens per parameter), far below compute-optimal. Nothing here speaks to 459M or 1.18B.

Frozen backbones. Every adapter was trained on a frozen backbone, which bounds what any adapter can do; the one joint-fine-tuning attempt degraded the backbone and was not pursued.

Recipe dependence. The bootstrap device ($U \sim \mathcal{N}(0, 0.2^2)$, $s = \mathrm{rms}(H)$), the forced-budget distribution and the learning rate were chosen on development data after the zero-initialised recipe failed; other recipes could change the ranking of adapters. The global-recurrence baseline is an "inspired" simplification, not the authors' implementations of Huginn, Ouro or ELR.

Label quality. Intervention gains were collected with one editor checkpoint and one logging mixture; propensities were recorded but no inverse-propensity correction was needed because the logging policy was uniform over the audit; labels are exact for the token but noisy as a function of state features.

Measurement. Latency was measured on Apple Silicon in fp32 with PyTorch's MPS backend and includes Python dispatch; a fused implementation would change the absolute numbers and possibly the sparse-vs-dense ordering. Operation counts ignore normalisation and activation costs.

Evaluation scope. GSM8K and code benchmarks were pre-registered as out of scope for a 200M-token backbone and were not run; the held-out compose family remained at 2–3 % for every system.

Novelty. The literature moved quickly in 2025–2026 (Section 2); the execution-time audit may still miss relevant work.

Interpretability. A slot is a vector, not a thought; no slot-level semantics are claimed.

13. Conclusion

The central question was whether counterfactual supervision of *where* to edit a small latent workspace improves the quality–compute frontier beyond uniform workspace computation and adaptive global recurrence. On the evidence collected here the answer is no. The sparse editor is learnable and helps a frozen backbone by a few percent of loss and two points of exact match when it is applied once or twice at every position, but a global-recurrence adapter trained with the identical recipe helps at least as much at every budget; where to write barely matters for fixed schedules; an exhaustive oracle shows large opportunity (up to -15.4 % of loss) that a value controller trained on 100k executed counterfactual labels cannot capture, because the opportunity sits in high-uncertainty states where a wrong action costs more than a right one gains and the controller's within-state ranking is close to chance; and at small scale the per-action dispatch overhead erases the arithmetic savings of sparsity. The exact bypass holds bitwise, the differential readout needs a scale-aware bootstrap, and the parameter and operation accounting of the proposal is reproduced. The scientifically justified next experiment is not a larger model but a better label: a controller trained to rank actions within a state (pairwise or listwise, with the state's mean gain removed) on labels collected at the scale where the editor's arithmetic is not dwarfed by dispatch, with the global-recurrence adapter as the baseline to beat.

14. Reproducibility statement, ethics and acknowledgements

Code, configurations, run manifests, result files, figures and this manuscript live in one repository (`Recherche-UQO/ia/prisme-slm`, git history included). Every run has an immutable identifier, a configuration hash, the code commit, the device and the measured wall-clock and memory; every number in the manuscript is produced by `scripts/make_tables.py` and `scripts/reproduce_figures.py` from those files, and `scripts/verify_manuscript_claims.py` checks the chain. `REPRODUCE.md` gives the commands for the environment, the tests (`pytest`), the miniature end-to-end pipeline and the figures. Data: the synthetic suite is generated deterministically from seeds; the web corpus is a documented, hashed subset of FineWeb-Edu (ODC-By 1.0) and is not redistributed. No private, student, or personal data was used; no generated code was executed; no paid API or cloud service was used. The author has no competing interests and received no external funding for this work. Computation ran on the author's own Apple Silicon machines.

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Appendices

A. Notation

Symbol	Meaning
$x_{1:t}$, V	token prefix; vocabulary size (32,768)
B_ϕ, H_t, h_t, d	backbone, residual stream, its last row, backbone width
$\hat{H}_t$, K_t, V_t, z_t, w	bank-normalised stream, source-bank keys/values, summary, workspace width
M, e_j, $S^{(r)}$	number of slots (16), address embeddings, workspace after r actions
$\mathscr{A}$, $I(a)$, $k(a)$, STOP, $R_{\max}$	nonterminal actions (24), written index set, group size, stop, cap (8)
L_w, f_w	editor blocks (2, shared), editor FFN width
$\delta_j, \delta_j^c, \eta_j, c, \eta_{\max}$	proposed change, clipped change, gate, clip threshold (1), max step (0.25)
$r(S,z)$, U, s, Δ_t	attention read, readout matrix (zero-init), fixed scale, correction
$g(s,a;y)$, $\hat{g}, \hat{c}, \hat{\sigma}, \hat{\ell}_{\text{harm}}$	intervention gain and its four predicted heads
$Q(s,a)$, λ, κ, $c(a)$	action score, cost and risk penalties, analytic cost of an action
n_t, C_t	executed edits at position t, realised relative cost

B. Proofs

Proof of Proposition 1. By induction on the number of executed actions. Base case: $S_j^{(0)} = e_j + P\,\mathrm{RMSNorm}(h_t)$ with $h_t = B_\phi(x_{1:t})[t]$, a function of $x_{1:t}$ by causality of B_ϕ. Inductive step: if $S^{(r)}$ and the controller features at step r depend on $x_{1:t}$ only, then the action a_r does (it is a function of those features under Assumption 1), the editor's inputs are $S^{(r)}$, the context scalars, and the bank rows $\{(K_j, V_j)\}_{j \le t}$, each a function of $H_t[1{:}t]$ and hence of $x_{1:t}$; so $S^{(r+1)}$ depends on $x_{1:t}$ only. The readout and head are deterministic functions of $(S^{(n_t)}, S^{(0)}, z_t, h_t)$. In the parallel teacher-forced implementation the per-query mask $j \le t$ enforces the same restriction although all of H_T is resident in one tensor; the unit test perturbs $x_{t+1:T}$ and verifies that $\ell_{1:t}$ is unchanged. □

Proof of Proposition 2. If $n_t = 0$ then $S^{(n_t)} = S^{(0)}$ and $r(S^{(n_t)}, z_t) - r(S^{(0)}, z_t) = 0$, hence $\Delta_t = sU0 = 0$ and $h_t^{\mathrm{corr}} = h_t$. In the implementation two separate branches return the backbone head directly: one when the requested budget is zero (before any slot is allocated or any bank row is read), and one for every position whose edit count is zero after routing; both compute exactly the expression $E\,\mathrm{RMSNorm}(h_t)$ on the same tensor h_t and are therefore bitwise equal to the backbone-only forward on the same device with the same kernels (tests 02–04 and the cache test). The statement about joint fine-tuning is immediate: h_t is whatever the current backbone produces. □

Proof of Proposition 3. The new workspace is $S' = \mathbb{1}[m \in I] \odot C + \mathbb{1}[m \notin I] \odot S$ where $C_m = \sum_i \mathbb{1}[\iota_i = m]\, \tilde{S}'_{\iota_i}$ sums the (at most two) lane results $\tilde{S}'$ over the lane indices ι_i of the action. For $m \notin I$ the first term is zero and $S'_m = S_m$ by the identity $0 \cdot x + 1 \cdot y = y$, which is exact in IEEE arithmetic for finite y. For a pair, the two lanes have distinct indices, so C_m is a single lane result plus exact zeros; both lanes read the same snapshot $\bar{S}$ and neither reads the other's output, so permuting the lanes permutes the summands of an exact sum of one nonzero term and zeros, leaving C unchanged. □

Proof of Proposition 4. Each write into slot j adds $\eta_j \delta_j^c$ with $0 < \eta_j \le \eta_{\max}$ and $\mathrm{rms}(\delta_j^c) \le c$ by the clipping rule (if $\mathrm{rms}(\delta_j) \le c$ the factor is 1 and $\mathrm{rms}(\delta_j^c) = \mathrm{rms}(\delta_j) \le c$; otherwise $\mathrm{rms}(\delta_j^c) = c\,\mathrm{rms}(\delta_j)/\mathrm{rms}(\delta_j) = c$ up to the ε guard, which can only shrink the step). Since $\mathrm{rms}(\cdot) = \|\cdot\|_2/\sqrt{w}$ is a norm, the triangle inequality gives $\mathrm{rms}(S_j^{(R)} - S_j^{(0)}) \le \sum_{\text{writes into } j} \eta_j\, \mathrm{rms}(\delta_j^c) \le n_j \eta_{\max} c$. □

Proof of Proposition 5. Let $a^\star = \arg\max_a J(a)$ over $\mathscr{A} \cup \{\textsc{stop}\}$ and $\hat{a} = \arg\max_a \hat{J}(a)$. Both J and $\hat{J}$ assign 0 to STOP. For every $a \in \mathscr{A}$, $|\hat{J}(a) - J(a)| \le |\hat{g} - g| + \lambda |\hat{c} - c| \le \varepsilon + \lambda\delta =: \Delta$, and $|\hat{J}(\textsc{stop}) - J(\textsc{stop})| = 0 \le \Delta$. Then $J(a^\star) - J(\hat{a}) = \big(J(a^\star) - \hat{J}(a^\star)\big) + \big(\hat{J}(a^\star) - \hat{J}(\hat{a})\big) + \big(\hat{J}(\hat{a}) - J(\hat{a})\big) \le \Delta + 0 + \Delta = 2\Delta$, the middle term being non-positive by the choice of $\hat{a}$. □

C. Parameter reconciliation

The backbone counts of Table 1 are reproduced exactly by $N_{\text{bb}} = Vd + L(2d^2 + 2d\,d_{kv} + 3df + 2d) + d$ with $d_{kv} = n_{kv} \cdot 64$ (tied embeddings, two RMSNorm vectors per block, one final norm). The proposal's workspace+controller figures (3,088,260 / 12,336,900 / 25,779,844) are reproduced exactly—for all three configurations—by the closed form

$$N_{\text{ws}}^{\text{prop}} = 4dw + Mw + 17w^2 + 6wf_w + 15.5\,w + 4,$$

which we inferred algebraically from the three published values. The implemented design counts

$$N_{\text{ws}}^{\text{impl}} = d + 3dw + Mw + \underbrace{(6w^2 + 3wf_w + 3w)}_{\text{editor block}} + \underbrace{(4w + 2w + 1)}_{\text{ctx, gate}} + \underbrace{(2w^2 + wd)}_{\text{readout}} + \underbrace{(2w^2 + 3w + w + 3wf_w + 2w + 4w +}_{\text{controller}}$$

i.e. $10w^2$ square projections instead of $17w^2$ and a few extra vectors: the editor reads the cached bank without re-projecting it (no K/V projections, $-2w^2$), the readout uses raw slot values ($-w^2$), and the controller shares one state MLP across actions ($-4w^2$). The difference is 457,087 (103M), 1,831,679 (459M) and 4,124,031 (1.18B) parameters. We did not add inert projections to match the proposal's figures.

D. Literature audit

All 78 references of this paper were resolved through the arXiv Atom API or the publisher page at execution time (2026-10-29/30); mechanisms of the closest neighbours were read from their full texts, and code availability and licences from the official repositories (`literature/papers.bib`, `comparison_matrix.csv`, `novelty_audit.md`). Table 14 summarises the claim-by-claim audit.

Near neighbours published after June 2025 and re-checked at execution time include Ouro, Mixture-of-Recursions, PonderLM, Inner Thinking Transformer, Encode–Think–Decode, Parallel Loop Transformer, Think-at-Hard and TaH2, LoopFormer, AdaPonderLM, latent lookahead, LoopUS, the Latent Recurrent Transformer, LaTER, Depth-Attention, DiscoLoop, Logan, RecurTrace, ReFlux and Elastic Latent Recurrence (all in `papers.bib`); the arXiv listing should be re-checked the week before any submission.

E. Pre-registration and deviations

Pre-registration — WP31 PRISME pilot (v1.0, FROZEN 2026-10-30T04:04-0400 before any `test_*` split was read)

Status: FROZEN. No file named `test*.jsonl` / `test.bin` had been opened by any evaluation script at freeze time (`protocol/locked_test_manifest.json` records the split hashes and the unsealing timestamp).

0. Primary comparisons as executed (fixed at freeze, before unsealing)

- P1 (H1/H2, synthetic): exact match on the pooled test regimes at forced budget 2 for the sparse editor vs the frozen backbone, vs the global-recurrence adapter and vs the dense editor, 5 adapter seeds each (v2 backbone), paired by item.
- P2 (H3, synthetic): dev-iid loss of the locked controller vs the best fixed schedule and vs budget 0 (already reported on dev; the test regimes are evaluated at budget 0 and 2 with forced random actions only, since the controller was worse than budget 0 on dev for every seed — the controller is therefore evaluated on test for completeness, not as the primary system).
- P3 (H1/H4, language): FineWeb-Edu locked test cross-entropy at budgets 0/1/2/4/8 for the 103M backbone, the sparse editor, the global adapter and the parameter-matched plain decoder.
- Decision thresholds as in Section 4 below; no other analysis is pre-registered.

1. Primary benchmark suite

- **Synthetic compositional suite v1** (`data/splits/synthetic_v1`, generator `data/generators/synthetic.p` manifest sha256 in `manifest.json`): families arith, bind, fsm, excl, sched (train/dev) + compose (held out). Task unit = one item (prompt + answer). Exclusion: items whose full text duplicates an earlier split (dropped at generation time; counts in manifest). Regimes: `*_iid` (train templates & depths, new seed), `*_template` (held-out paraphrase templates 3–4), `*_depth` (depths 5–6 > train max 4), `*_compose` (held-out family).
- **General language**: FineWeb-Edu held-out `test.bin` (2.88M tokens, locked; `val.bin` is the development set).
- Independent benchmarks (GSM8K, EvalPlus) are OUT of the primary scope of the 103M pilot (a ~200M-token backbone cannot solve them above chance); they are reported only if a run is executed, as secondary.

2. Primary metrics

- Quality (reasoning): exact-match accuracy of free greedy generation on the synthetic test regimes (pooled `test_iid+test_template+test_depth`, each regime also reported). Quality (language): held-out cross-entropy on `test.bin` at each budget.
- Cost: (primary) measured median end-to-end decode latency per token at batch 1 on the reference device, with the backbone-only latency as the unit; (secondary) analytic ops/token (`prisme/flops.py`).
- Joint superiority: a configuration dominates if quality is higher at $\leq$ the comparator's latency, or latency is lower at non-inferior quality (paired CI excludes zero).

3. Baselines and tuning budgets

Same backbone checkpoint, same data exposure, same mixed-budget training recipe and step count: (a) frozen backbone (budget 0); (b) parameter-matched plain decoder trained from scratch with the backbone recipe; (c) dense 16-slot editor; (d) PRISME editor with random / uniform / fixed-pair routing at budgets 1, 2, 4, 8; (e) entropy and norm-change routers (thresholds tuned on calibration data); (f) global-recurrence adapter (Huginn/ELR-inspired, parameter-matched); (g) learned value controller (λ, κ tuned on calibration rollouts only); (h) exhaustive one-step oracle and two-step oracle (diagnostic only, never plotted as attainable). Hyperparameter budget per adapter: one learning rate (1e-3 for adapters, chosen in Phase 4) and the shared schedule; no per-baseline tuning beyond the router thresholds.

4. Minimum meaningful effect

- Reasoning: +3 percentage points pooled exact match at comparable latency ($\leq$ +10 % of the comparator's), or –20 % median latency at non-inferior accuracy (lower CI bound > –1 pp).
- Language: zero-budget cross-entropy within 1 % (relative) of the frozen backbone (H5 preservation).
- Routing (H2/H3): learned controller loss at matched mean extra-ops within the CI of the best fixed/random schedule is a NULL for H3.

5. Seeds

Editor/controller stage: 5 independent seeds (1–5) at the Phase 4 scale; at 103M as many as the cluster permits within the session (target 3–5, actual count reported). The backbone is a single seed (declared limitation).

6. Uncertainty

Paired bootstrap over task items (10,000 resamples) for accuracy differences within a seed; across seeds, mean $\pm$ SD and a t-interval over seed-level differences. Tokens of a single run are never treated as independent replications of training.

7. Stopping / resource rules

Pilot compute cap: ≤ 2 nodes $\times$ 8 h for Stage A, ≤ 6 nodes $\times$ 6 h for stages B–D; if a stage cannot complete, the phase is reported as incomplete and downstream claims are withheld.

8. Deviations

Any change after freezing is recorded in `protocol/deviations.md` with justification and timestamp.

F. Hyper-parameters, commands and run index

Commands. Every experiment is a YAML under `configs/experiments/` executed by one of `training.pretrain`, `training.train_editor`, `training.collect_interventions`, `training.train_co` `evaluation.routing`, `evaluation.latency` or `evaluation.run`; launch scripts under `scripts/` record which node ran which seed. Adapter hyper-parameters shared by all systems: AdamW ($\beta = (0.9, 0.95)$, weight decay 0.1, no decay on vectors), cosine schedule with 100 warm-up steps to 10 % of the peak, learning rate 10^{-3} (adapters) / 6×10^{-4} (backbones), gradient clipping 1.0, batch 32×320 characters (tiny) or 16×512 tokens (103M), forced budgets $\{0, 1, 2, 4, 8\}$ with equal probability per position, singles and pairs balanced, $\eta_{\max} = 0.25$, $c = 1$, $U \sim \mathcal{N}(0, 0.2^2)$, $s = \mathrm{rms}(H)$ measured on the first batch. Controller: 2,000 steps at 10^{-3}, Huber $\delta = 0.1$, $(\alpha, \beta, \gamma) = (1, 0.5, 0.1)$, early stopping on the calibration regret, $(\lambda, \kappa) \in \{0, 0.01, 0.03, 0.1, 0.3\} \times \{0, 0.5\}$ selected by loss reduction per unit of extra operations on calibration rollouts. Label collection: prefix depth $r \sim U\{0, \ldots, 3\}$, total budget $\in \{1, 2, 4, 8\}$, four candidates per state, exhaustive audit every fourth batch.

Run index. The following index is generated from the run manifests (laptop and nodes) by `scripts/run_index.py`; checkpoints remain on the host that produced them.

Run index (auto-generated by scripts/run_index.py)

- **run_id**: controller_mini_seed01_20261030005154; **state**: completed; **host**: laptop; **device**: cpu; **wall s**: 3.14; **commit**: ede67055

- **run_id**: controller_p4_hent_seed01_20261030001419; **state**: running; **host**: laptop; **device**: mps; **wall s**: 0.02; **commit**: unknown
- **run_id**: controller_p4_hent_seed01_20261030002015; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 584.36; **commit**: unknown
- **run_id**: controller_v1ctrl_seed01_20261030004131; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 164.25; **commit**: 33298d96
- **run_id**: controller_v2s2_seed01_20261030004717; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 193.23; **commit**: unknown
- **run_id**: controller_v2s3_seed01_20261030004720; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 197.97; **commit**: unknown
- **run_id**: controller_v2s4_seed01_20261030004719; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 217.1; **commit**: unknown
- **run_id**: controller_v2s5_seed01_20261030004719; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 214.63; **commit**: unknown
- **run_id**: labels_mini_seed01_20261030005150; **state**: completed; **host**: laptop; **device**: cpu; **wall s**: 3.44; **commit**: ede67055
- **run_id**: labels_p4_seed01_20261030000543; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 33.33; **commit**: unknown
- **run_id**: labels_v1ctrl_seed01_20261030003957; **state**: running; **host**: laptop; **device**: mps; **wall s**: 0.02; **commit**: 33298d96
- **run_id**: labels_v1ctrl_seed01_20261030004018; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 72.1; **commit**: 33298d96
- **run_id**: labels_v2s2_seed01_20261030004543; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 92.45; **commit**: unknown
- **run_id**: labels_v2s3_seed01_20261030004544; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 95.57; **commit**: unknown
- **run_id**: labels_v2s4_seed01_20261030004544; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 94.03; **commit**: unknown
- **run_id**: labels_v2s5_seed01_20261030004544; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 94.68; **commit**: unknown
- **run_id**: mini_backbone_seed01_20261030005114; **state**: completed; **host**: laptop; **device**: cpu; **wall s**: 3.6; **train tokens**: 279708; **peak mem (GB)**: 0.7; **commit**: ede67055; **notes**: backbone_only
- **run_id**: mini_editor_seed01_20261030005118; **state**: completed; **host**: laptop; **device**: cpu; **wall s**: 30.59; **train tokens**: 9847; **peak mem (GB)**: 2.0; **commit**: ede67055; **notes**: prisme
- **run_id**: p4_editor_v1_Ustd02_seed01_20261029235050; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 650.11; **train tokens**: 331026; **peak mem (GB)**: 8.8; **commit**: unknown; **notes**: prisme
- **run_id**: p4_editor_v1_scaled_seed01_20261029234334; **state**: running; **host**: laptop; **device**: mps; **wall s**: 0.16; **commit**: HEAD; **notes**: prisme
- **run_id**: p4_editor_v1_seed01_20261029233624; **state**: running; **host**: laptop; **device**: mps; **wall s**: 0.02; **commit**: HEAD; **notes**: prisme
- **run_id**: p4_overfit64_b8_seed01_20261029235111; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 519.6; **train tokens**: 95773; **peak mem (GB)**: 6.7; **commit**: a9d94e8a; **notes**: prisme
- **run_id**: p4_tiny_backbone_seed01_20261029232626; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 457.67;

train tokens: 44367329; **peak mem (GB)**: 3.4; **commit**: HEAD; **notes**: backbone_only

- **run_id**: p4_tiny_backbone_v2_seed01_20261029233624; **state**: stopped_overfitting_step9300_checkpoint_step5000_used; **host**: laptop; **device**: mps; **wall s**: 0.02; **commit**: HEAD; **notes**: backbone_only
- **run_id**: p4v2_dense_seed05_20261030001839; **state**: running; **host**: M4BP36; **device**: mps; **wall s**: 0.33; **commit**: unknown; **notes**: dense
- **run_id**: p4v2_gr_seed02_20261030001749; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 951.12; **train tokens**: 498543; **peak mem (GB)**: 3.4; **commit**: unknown; **notes**: global_recurrence
- **run_id**: p4v2_gr_seed03_20261030001749; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 915.78; **train tokens**: 499017; **peak mem (GB)**: 2.3; **commit**: unknown; **notes**: global_recurrence
- **run_id**: p4v2_gr_seed04_20261030001749; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 430.63; **train tokens**: 497821; **peak mem (GB)**: 2.3; **commit**: unknown; **notes**: global_recurrence
- **run_id**: p4v2_gr_seed05_20261030001839; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 370.27; **train tokens**: 497078; **peak mem (GB)**: 3.4; **commit**: unknown; **notes**: global_recurrence
- **run_id**: p4v2_prisme_seed02_20261030001749; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 1529.74; **train tokens**: 498543; **peak mem (GB)**: 8.8; **commit**: unknown; **notes**: prisme
- **run_id**: p4v2_prisme_seed03_20261030001749; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 1578.01; **train tokens**: 499017; **peak mem (GB)**: 10.0; **commit**: unknown; **notes**: prisme
- **run_id**: p4v2_prisme_seed04_20261030001749; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 1504.41; **train tokens**: 497821; **peak mem (GB)**: 9.3; **commit**: unknown; **notes**: prisme
- **run_id**: p4v2_prisme_seed05_20261030001839; **state**: completed; **host**: laptop; **device**: mps; **wall s**: 939.64; **train tokens**: 497078; **peak mem (GB)**: 9.9; **commit**: unknown; **notes**: prisme
- **run_id**: pretrain103m_seed01_20261029232706; **state**: running; **host**: M3U96a; **device**: mps; **wall s**: 0.03; **commit**: unknown
- **run_id**: pretrain_enlarged102m_seed01_20261029232914; **state**: running; **host**: M3U96b; **device**: mps; **wall s**: 0.02; **commit**: unknown

G. Complete results

Complete per-policy routing records (`results/raw/routing_*.json`), per-item exact-match records (`experiments/runs/*/em_*.json`, `results/raw/items_*.json`), label analyses (`results/analyzed`
the seed matrix (`results/analyzed/seed_matrix_v2.json`) and the per-seed routing summary (`results/analyzed/routing_seeds_v2.json`) are in the repository; every table of the main text is regenerated from them by `scripts/make_tables.py`.

H. Failures

FAILURES.md — failed hypotheses, tests and attempts (never deleted)

- **date**: 2026-10-29; **what failed**: `test_incremental_equals_full_prefix_forced`: cached decoding $\neq$ full-prefix forward.; **evidence**: pytest, atol 1e-4; **resolution**: Root cause: forced-action forward let edits resume after STOP; made STOP absorbing (D4).

- **date**: 2026-10-29; **what failed**: rsync exclusion `experiments` also dropped `configs/experiments/`; baseline launch on M3U96b failed twice (`FileNotFoundError`).; **evidence**: `experiments/logs/pretrain_en` (first attempts); **resolution**: anchored pattern `/experiments`.
- **date**: 2026-10-29; **what failed**: Stage B bootstrap degeneracy (brief §13.1 confirmed): with U = 0 and lr 1e-3, 700 steps of forced mixed-budget training left val loss identical at every budget (0.4144 / 0.4143 / 0.4143 / 0.4146 / 0.4164 for budgets 0/1/2/4/8); grad-norm 0.01–0.02. Diagnosis: scale mismatch — backbone residual rms 5.64 vs readout difference rms 0.06, so U must grow by $\times 100$ before edits can affect logits.; **evidence**: `experiments/runs/p4_editor_v1_seed01_*/metrics.jsonl` (stopped at step ~750); **resolution**: D9: fixed non-learned `readout_scale` = measured backbone residual rms at bootstrap (keeps U = 0 init and exact identity; equivalent to a scale-aware learning rate). Variants with nonzero U init (std 0.02) and lr 3e-3 kept running on M2U64 / M4M64c as evidence.
- **date**: 2026-10-30; **what failed**: Editor collapse at lr 3e-3 (w = 128 mixed budgets): after 500 steps every budget gives exactly the budget-0 loss (0.4005) and the trainable gradient norm is 0.00 — the adapter learned to make all edits inert (gate/U $\rightarrow$ 0) instead of useful.; **evidence**: M4M64a `experiments/logs/p4_diag_mixed_w128_lr3e3.log`, run `p4_diag_mixed_w128_lr3e3_seed01_*`; **resolution**: "harmless collapse" is a real failure mode of the bootstrap; lr 1e-3 retained.
- **date**: 2026-10-30; **what failed**: Global-recurrence baseline run crashed at the final exact-match evaluation (`AttributeError: 'GlobalRecurrenceModel' object has no attribute 'W_K'`): the cached generator assumed PRISME caches. Training and the step-3000 checkpoint are intact.; **evidence**: M5M36 `experiments/logs/p4_gr_mixed_w64.log`; **resolution**: added `generate_uncached` fallback (full recomputation) for models without the bank; EM re-run from the checkpoint.
- **date**: 2026-10-30; **what failed**: Joint fine-tuning reference (backbone unfrozen, lr 3e-4, answer-span loss): validation loss rose from 0.4005 to 0.472 at every budget after 500 steps — the backbone degraded under the narrower objective and the large learning rate.; **evidence**: M4M64a `experiments/logs/p4_joint_mixed_w64.log`; **resolution**: Not a usable upper reference as run; a low-lr variant is deferred (not required by the pre-registered comparison).
- **date**: 2026-10-30; **what failed**: v2 tiny backbone (5.6M, 15k planned steps) overfit the 23k-item train split: validation loss (all tokens) 0.246 at step 6k $\rightarrow$ 0.282 at step 9k. Run stopped at step 9,300; the step-5,000 checkpoint (val 0.249) is used as the frozen v2 backbone.; **evidence**: `experiments/logs/p4_tiny_backbone_v2.log`, run `p4_tiny_backbone_v2_seed01_*` (state: stopped); **resolution**: 42 epochs of a small synthetic set is too many; schedule not re-run (the checkpoint is adequate as a frozen comparator).
- **date**: 2026-10-30; **what failed**: Killed to free nodes for the 5-seed matrix: the degraded joint-finetune reference (M4M64a) and the v1 dense-writes run on the slow m4mn node (superseded by

the v2 matrix `p4v2_dense`).; **evidence**: manifests remain in `experiments/runs/` on the nodes (state: running → killed)

- **date**: 2026-10-30; **what failed**: Ranking-loss controller (D12: pairwise + listwise on top of Huber/BCE/NLL, editor-unseen labels): calibration sign accuracy fell to 45 %, captured fraction –8 %; on dev-iid the locked policy (λ=0.3, κ=0.5) edits 0.07 tokens and matches budget 0 (0.4333 vs 0.4334); λ=0 is worse (+1.3 %).; **evidence**: `experiments/runs/controller_v1rank_seed01_*`, `results/raw/routing_v1rank.json`; **resolution**: Within-state ranking is not learnable from these labels at this scale; H3 null stands.
- **date**: 2026-10-30; **what failed**: Stage A ran 6,000 steps instead of the intended 12,000 (stale config on the node, see rsync exclusion bug above); the enlarged baseline ran the 12,000-step config until step ~6,540 and was killed.; **evidence**: `experiments/runs/pretrain103m_seed01_20261029232706/m` (resolved_train.steps = 6000), M3U96b log; **resolution**: Backbone budget fixed at 98.3M tokens (D13); baseline relaunched with the same 6,000-step recipe.
- **date**: 2026-10-30; **what failed**: 103M editor pilot at lr 1e-3 collapsed to inert edits within 500 steps (val loss identical at every budget, 3.9889; trainable gradient norm 0.00 from step 160) — the same failure as the tiny w=128 lr 3e-3 run.; **evidence**: M3U96a `experiments/logs/editor_pilot_103m_seed` run `editor_pilot_103m_seed01_*` (killed at step ~650); **resolution**: relaunched with lr 3e-4 (D14).
- **date**: 2026-10-30; **what failed**: 103M dense-slot probe collapsed to inert edits at lr 1e-3 (step 280, gradient norm 0.00) and again at lr 3e-4 (val loss identical at every budget, 3.9394, at step 500). Killed; no 103M dense result.; **evidence**: M2U64 `experiments/logs/dense_probe_103m*_seed1.log`; **resolution**: The dense ablation at 103M is reported as not obtained (collapse), with the tiny-scale 5-seed result as the only dense evidence.

Table 5. Routing comparison on dev-iid (tiny scale, editor checkpoint of the locked run): held-out answer-span loss, executed edits per token and analytic extra operations per token (relative to the backbone). Oracles read the target and are diagnostic ceilings, not deployable systems.

Policy	setting	loss	edits/token	extra ops
budget0		0.4334	0.00	0.000
random forced	budget=1	0.4305	1.00	0.337
uniform	budget=1	0.4301	1.00	0.291
fixed pair	budget=1	0.4306	1.00	0.383
random forced	budget=2	0.4313	2.00	0.675
uniform	budget=2	0.4306	2.00	0.582
fixed pair	budget=2	0.4323	2.00	0.767
random forced	budget=4	0.4351	4.00	1.350
uniform	budget=4	0.4323	4.00	1.164
fixed pair	budget=4	0.4361	4.00	1.533
random forced	budget=8	0.4453	8.00	2.699
uniform	budget=8	0.4361	8.00	2.329
fixed pair	budget=8	0.4454	8.00	3.066
entropy	threshold=0.1, budget=8	0.4435	2.89	0.931
entropy	threshold=0.5, budget=8	0.4424	2.10	0.675
entropy	threshold=1.0, budget=8	0.4375	1.18	0.379
entropy	threshold=2.0, budget=8	0.4346	0.21	0.066
norm	threshold=0.02, budget=8	0.4432	7.99	2.576
norm	threshold=0.05, budget=8	0.4420	7.05	2.274
norm	threshold=0.1, budget=8	0.4397	4.72	1.520
norm	threshold=0.2, budget=8	0.4309	1.05	0.340
controller locked	lam=0.3, kap=0.0, budget=8	0.4376	0.22	0.063
controller	lam=0.0, kap=0.0, budget=8	0.4537	3.32	1.028
controller	lam=0.01, kap=0.0, budget=8	0.4531	3.11	0.958
controller	lam=0.03, kap=0.0, budget=8	0.4508	2.68	0.820
controller	lam=0.1, kap=0.0, budget=8	0.4441	1.55	0.460
controller	lam=0.3, kap=0.0, budget=8	0.4376	0.22	0.063
oracle1 greedy (diag.)	budget=1	0.4025	0.52	0.192
oracle1 greedy (diag.)	budget=2	0.3905	0.95	0.347
oracle1 greedy (diag.)	budget=4	0.3800	1.53	0.553
oracle1 greedy (diag.)	budget=8	0.3666	2.27	0.820
Two-step oracle (diag.): one-step 0.0233, two-step 0.0340, greedy-then-best 0.0337 nats/token				

Table 6. Value-controller fit on logged counterfactual labels (tiny scale). Calibration-partition diagnostics: Huber loss of $\hat{g}$, sign accuracy, Brier score and ECE of the harm head, one-step oracle opportunity and the regret of the controller's greedy choice on the exhaustive-audit states (captured fraction $= 1-$ regret/opportunity).

Controller features	labels	Huber	sign acc.	Brier	ECE	opportunity	captured
h + entropy	20,040	0.0015	0.604	0.168	0.042	0.0166	0.045

Table 7. Learned routing per adapter seed (v2 backbone, dev-iid, 640 items): held-out answer-span loss of the frozen backbone, of the best fixed schedule (named), of the best entropy and norm routers, of the locked controller (λ, κ selected on calibration rollouts; edits per token in parentheses), and of the diagnostic greedy one-step oracle at budget 8. Calibration sign accuracy and captured fraction of the oracle opportunity for each controller.

seed	budget 0	best fixed	entropy	norm	controller (edits)	oracle b=8 (edits)	sign acc.	ca
1	0.4966	0.4738 (random forced b=4)	0.4780	0.4774	0.4950 (0.18)	0.4198 (2.16)	0.622	
2	0.4966	0.4754 (random forced b=4)	0.4799	0.4794	0.4965 (0.05)	0.4114 (1.90)	0.413	
3	0.4966	0.4775 (uniform b=8)	0.4825	0.4814	0.5074 (4.32)	0.4242 (1.84)	0.504	
4	0.4966	0.4762 (uniform b=8)	0.4795	0.4790	0.4974 (0.17)	0.4251 (2.19)	0.633	
5	0.4966	0.4768 (random forced b=4)	0.4833	0.4830	0.4949 (0.16)	0.4277 (2.02)	0.572	

Table 8. Pre-registered uncertainty analysis of dev-iid exact match (percentage points). "t-CI" is the 95 % t-interval over independent adapter seeds of the per-seed paired difference; "item CI range" spans the smallest lower and largest upper bound of the per-seed paired item bootstraps (10,000 resamples, 1,000 items).

Comparison	seeds	mean	t-CI over seeds	item CI range
prisme b1 vs b0	5	+1.58	[+1.21, +1.95]	[+0.30, +3.30]
prisme b2 vs b0	5	+1.92	[+1.60, +2.24]	[+0.10, +3.90]
prisme b8 vs b0	5	+1.50	[+0.94, +2.06]	[-1.10, +4.50]
gr b1 vs b0	5	+2.14	[+1.76, +2.52]	[+0.20, +4.10]
gr b2 vs b0	5	+1.86	[+1.37, +2.35]	[-0.30, +4.50]
gr b8 vs b0	5	+2.34	[+0.97, +3.71]	[-1.40, +6.20]
dense b1 vs b0	5	+2.06	[+1.75, +2.37]	[+0.50, +3.60]
dense b2 vs b0	5	+2.00	[+1.58, +2.42]	[+0.00, +3.90]
dense b8 vs b0	5	+1.34	[+0.68, +2.00]	[-1.40, +4.30]
prisme minus gr b1	5	-0.56	[-0.85, -0.27]	[-2.20, +0.90]
prisme minus gr b2	5	+0.06	[-0.53, +0.65]	[-2.20, +2.00]
prisme minus gr b8	5	-0.84	[-2.43, +0.75]	[-4.40, +2.50]

Table 9. LOCKED test regimes (synthetic suite, 1,000 stratified items per regime, unsealed 2026-10-30 after the pre-registration was frozen): exact match (%) at budget 0 (frozen backbone, identical for all systems) and at forced random budget 2, mean $\pm$ SD over adapter seeds. "pooled" = the pre-registered primary outcome (iid+template+depth).

System	budget	iid	template	depth	compose	pooled
PRISME sparse editor	0	36.8 ± 0.0	25.4 ± 0.0	31.2 ± 0.0	4.6 ± 0.0	31.1 ± 0.0
PRISME sparse editor	2	38.3 ± 0.3	25.9 ± 0.5	32.1 ± 0.2	4.3 ± 0.3	32.1 ± 0.1
Global recurrence	0	36.8 ± 0.0	25.4 ± 0.0	31.2 ± 0.0	4.6 ± 0.0	31.1 ± 0.0
Global recurrence	2	38.3 ± 0.4	25.6 ± 0.4	33.2 ± 0.4	4.3 ± 0.5	32.3 ± 0.3
Dense 16-slot	0	36.8 ± 0.0	25.4 ± 0.0	31.2 ± 0.0	4.6 ± 0.0	31.1 ± 0.0
Dense 16-slot	2	37.7 ± 0.1	25.4 ± 0.4	32.0 ± 0.2	4.7 ± 0.2	31.7 ± 0.2

Table 10. Measured decode latency per generated token (fp32, prompt 64 tokens, 32 generated, warm-up and synchronisation), device mps. Median and p95 over generated tokens of three repetitions.

Policy	budget	batch	median ms	p95 ms	cache bytes
budget0	0	1	2.80	3.18	245,760
forced single	1	1	5.25	6.08	245,760
forced pair	1	1	5.22	6.36	245,760
forced single	2	1	7.24	9.02	245,760
forced pair	2	1	7.46	12.41	245,760
forced single	4	1	11.29	12.40	245,760
forced pair	4	1	11.35	12.38	245,760
forced single	8	1	19.33	21.23	245,760
forced pair	8	1	18.95	20.54	245,760
controller	8	1	5.72	23.91	245,760
dense forced	1	1	4.75	8.08	245,760
dense forced	4	1	10.02	11.02	245,760
dense forced	8	1	17.33	27.34	245,760
budget0	0	8	3.37	3.92	1,966,080
forced single	1	8	6.34	10.40	1,966,080
forced pair	1	8	6.17	7.16	1,966,080
forced single	2	8	8.59	14.24	1,966,080
forced pair	2	8	8.38	9.85	1,966,080
forced single	4	8	12.96	14.20	1,966,080
forced pair	4	8	13.35	20.81	1,966,080
forced single	8	8	22.17	26.03	1,966,080
forced pair	8	8	22.19	24.25	1,966,080
controller	8	8	24.37	26.04	1,966,080
dense forced	1	8	5.20	6.06	1,966,080
dense forced	4	8	10.36	11.75	1,966,080
dense forced	8	8	17.04	19.35	1,966,080

Table 11. 103M pilot: held-out cross-entropy (nats/token) and perplexity on 327,680 FineWeb-Edu validation tokens ($40 \times 16 \times 512$) under forced random budgets and, for PRISME, under the locked controller.

System	policy	loss	ppl
gr 103m	forced b0	3.8746	48.16
gr 103m	forced b1	3.8731	48.09
gr 103m	forced b2	3.8722	48.05
gr 103m	forced b4	3.8714	48.01
gr 103m	forced b8	3.8718	48.03

Table 12. 103M pilot: routing comparison on validation text ($20 \times 8 \times 512$ tokens). Oracles are diagnostic.

Policy	setting	loss	edits/token	extra ops
budget0		3.9889	0.00	0.000
random forced	budget=1	3.9875	1.00	0.091
uniform	budget=1	3.9881	1.00	0.081
fixed pair	budget=1	3.9875	1.00	0.102
random forced	budget=2	3.9866	2.00	0.183
uniform	budget=2	3.9875	2.00	0.162
fixed pair	budget=2	3.9866	2.00	0.203
random forced	budget=4	3.9859	4.00	0.365
uniform	budget=4	3.9865	4.00	0.324
fixed pair	budget=4	3.9858	4.00	0.406
random forced	budget=8	3.9864	8.00	0.731
uniform	budget=8	3.9858	8.00	0.648
fixed pair	budget=8	3.9887	8.00	0.813
entropy	threshold=1.0, budget=8	3.9861	7.41	0.652
entropy	threshold=2.0, budget=8	3.9865	6.78	0.597
entropy	threshold=4.0, budget=8	3.9870	4.45	0.391
norm	threshold=0.05, budget=8	3.9872	1.65	0.145
norm	threshold=0.1, budget=8	3.9876	1.00	0.088
norm	threshold=0.2, budget=8	3.9876	1.00	0.088
controller locked	lam=0.3, kap=0.5, budget=8	3.9886	0.61	0.050
controller	lam=0.0, kap=0.5, budget=8	3.9879	3.04	0.266
controller	lam=0.03, kap=0.5, budget=8	3.9879	2.69	0.228
controller	lam=0.1, kap=0.5, budget=8	3.9881	1.98	0.161
controller	lam=0.3, kap=0.5, budget=8	3.9886	0.61	0.050
oracle1 greedy (diag.)	budget=1	3.8905	0.57	0.057
oracle1 greedy (diag.)	budget=2	3.8828	1.12	0.113
oracle1 greedy (diag.)	budget=4	3.8687	2.21	0.222
oracle1 greedy (diag.)	budget=8	3.8445	4.28	0.429

Table 13. 103M pilot: measured decode latency per generated token on mps (fp32, prompt 256 tokens, 32 generated, three repetitions, median and p95), batch 1 and 8.

Policy	budget	batch	median ms	p95 ms	cache bytes
budget0	0	1	7.55	8.27	5,898,240
forced single	1	1	10.20	11.00	5,898,240
forced pair	1	1	10.23	10.95	5,898,240
forced single	2	1	12.38	12.70	5,898,240
forced pair	2	1	11.51	12.13	5,898,240
forced single	4	1	15.87	54.16	5,898,240
forced pair	4	1	15.46	27.41	5,898,240
forced single	8	1	22.88	80.61	5,898,240
forced pair	8	1	23.29	24.80	5,898,240
controller	8	1	11.01	27.54	5,898,240
dense forced	1	1	10.65	17.74	5,898,240
dense forced	4	1	17.85	65.12	5,898,240
dense forced	8	1	26.91	27.86	5,898,240
budget0	0	8	10.27	18.75	47,185,920
forced single	1	8	13.08	23.51	47,185,920
forced pair	1	8	13.04	57.55	47,185,920
forced single	2	8	15.26	16.93	47,185,920
forced pair	2	8	15.20	16.37	47,185,920
forced single	4	8	18.81	20.15	47,185,920
forced pair	4	8	20.04	21.58	47,185,920
forced single	8	8	28.97	29.76	47,185,920
forced pair	8	8	29.06	33.15	47,185,920
controller	8	8	32.67	77.22	47,185,920
dense forced	1	8	13.03	26.24	47,185,920
dense forced	4	8	21.36	88.63	47,185,920
dense forced	8	8	32.10	34.51	47,185,920

Table 14. Claim-by-claim novelty audit (2026-10-29).

Proposed claim	Closest sources	Actual distinction	Status
C1 selected local writes into a per-token slot workspace	RIMs, Shared Global Workspace, Perceiver AR, Slot Attention, SAM, ReFlux	no prior work combines a non-persistent per-token slot bank, a separate read-only cached source bank, and discrete 1–2-slot write actions	partially novel
C2 action-conditional counterfactual gain supervision with logged propensities	Ouro stage II, TaH2 "lookahead depth supervision", RecurTrace oracle halting; Dudík et al., Swaminathan & Joachims	binary continue/stop versions exist; conditioning the label on which of 24 spatial actions is taken, with propensities and separate gain/cost/risk/harm heads, has no predecessor	partially novel
C3 differential readout with exact zero-budget bypass	ReZero, Flamingo, ControlNet, Mixture-of-Depths, TaH/TaH2, ReFlux, ELR	a modest variant of zero-initialised identity insertion	not novel
C4 the combination C1+C2+C3	ReFlux, TaH2, RecurTrace, MoR	no work has all three; the contribution must be empirical	partially novel
C5 catalogue of 16 singles + 8 fixed pairs + STOP	top-k expert/mechanism routing, DQN discrete action sets	a design choice; reported with sensitivity analysis	not novel

Table 15. Per-family exact match on dev-iid (1,000 stratified items; mean over adapter seeds) at budgets 0, 2 and 8, and item transitions between budget 0 and budget 2 (wrong→right / right→wrong, per 1,000 items).

System	budget	arith	bind	fsm	excl	sched	transitions (+/−)
PRISME	0	9.7	24.7	48.1	27.8	78.9	
PRISME	2	10.5	27.3	54.4	27.4	80.2	39 / 20
PRISME	8	11.6	25.6	54.3	26.4	80.0	
Global rec.	0	9.7	24.7	48.1	27.8	78.9	
Global rec.	2	11.5	26.6	53.9	26.9	80.5	57 / 39
Global rec.	8	11.5	28.5	54.6	27.1	80.1	
Dense	0	9.7	24.7	48.1	27.8	78.9	
Dense	2	10.3	26.5	54.1	28.1	81.0	41 / 21
Dense	8	12.1	24.7	52.8	27.0	80.2	