
Beyond Accuracy and Alignment: A Diagnostic Evaluation Protocol for Feedback Alignment

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Abstract

1 Modern feedback-alignment evaluation on deep residual networks is still summa-
2 rized by a deceptively simple pair: headline accuracy and headline cosine align-
3 ment Γ to the backpropagation gradient. We show that this pair can silently fail in
4 two distinct ways on standard CIFAR-10 pre-LayerNorm ResMLP and ViT-Mini
5 settings: first, *measurement degeneracy*, where residual-stream growth drives
6 hidden-layer BP gradients to the numerical floor and makes Γ uninterpretable;
7 and second, *low intrinsic credit-direction quality*, where random-feedback credit
8 remains essentially unaligned with BP on the deep blocks even when the reference
9 gradient is still meaningful. The headline result is that the field-standard reporting
10 pair walks back none of the methods we audit, whereas a four-diagnostic protocol
11 walks back the three degenerate methods and passes the two trustworthy controls.
12 Our contribution is an evaluation methodology paper for the NeurIPS 2026 Evaluations
13 & Datasets track: we provide the protocol, the calibration logic for its thresh-
14 olds, a reference implementation, a five-method audit, and validation through tem-
15 poral replay, cross-architecture checks, intervention-based disambiguation, and a
16 documented catalog of pipeline pitfalls, in the spirit of critical evaluation analyses
17 such as Jordan et al. [3], O’Bray et al. [2], Paleka et al. [1].

18 1 Introduction

19 Feedback-alignment papers are usually judged by two numbers: task accuracy and an aggregate
20 similarity between the method’s local credit signal and the backpropagation gradient [4–7]. On
21 the audited 4-block $d=256$ ResMLP, however, Table 1 already shows that this pair is not a validity
22 check: DFA reaches only 0.306 ± 0.006 test accuracy, below the architecture-matched frozen-blocks
23 baseline of 0.349 ± 0.002 , while still looking superficially comparable to other non-BP methods.
24 Figure 1 further shows that the apparent cosine evidence is concentrated at the shallowest block,
25 with DFA at seed 42 reaching about $+0.42$ at layer 0 but approximately -0.03 to 0 on layers 1–4, so
26 the aggregate obscures where credit direction is and is not present. At the same time, the deepest BP
27 reference norm is only about 5×10^{-10} for DFA, State Bridge, and Credit Bridge, below the 10^{-8}
28 clamp used by `F.cosine_similarity`, whereas BP remains around 4×10^{-4} , so the reported deep
29 cosine is partly computed against a numerical-floor reference rather than an informative gradient
30 direction (Figure 1; Table 1). Those numbers can be useful, but only if the measurement regime
31 itself is valid.

32 Our audit shows that modern residual vision models can make these two quantities look informa-
33 tive while failing to answer the question they are taken to answer. Figure 1 shows the first failure
34 mode, which we call *Mode 1: measurement degeneracy*, where residual-stream growth drives the
35 deepest hidden state to about $\|h_L\| \sim 10^8$ under DFA/SB/CB while the corresponding BP reference

Table 1: Main audit table for the 4-block $d=256$ pre-LayerNorm ResMLP on CIFAR-10. The row and column structure is fixed here; fill from the three-seed audit output.

Method	Test acc.	Headline Γ	Status-quo verdict	Protocol verdict
BP	0.615 ± 0.003	≈ 1.0	trustworthy	trustworthy
EP	0.316 ± 0.030	0.008	trustworthy	trustworthy
DFA	0.306 ± 0.006	0.10	trustworthy	walked back
State Bridge	0.205 ± 0.032	0.005	trustworthy	walked back
Credit Bridge	0.289 ± 0.026	0.07	trustworthy	walked back

36 collapses to $\|g_L\| \sim 5 \times 10^{-10}$, so the deep-layer cosine is measured against a clamp-dominated
 37 floor rather than a meaningful target direction. The same figure also shows the second failure mode,
 38 *Mode 2: low intrinsic credit-direction quality*, because even after comparing against the stronger
 39 frozen-blocks baseline (0.349 ± 0.002) and looking layer-by-layer, DFA’s deep blocks remain essen-
 40 tially null while only layer 0 is visibly positive. To test whether this is only a measurement problem,
 41 the intervention results show a dissociation: with a residual penalty $\lambda \|f_l(h_l)\|^2$, the deepest state
 42 scale falls toward 4×10^4 , the reference gradient rises toward 10^{-6} , and deep cosine can improve
 43 to about $+0.16$, yet at $\lambda=10^{-4}$ Mode 1 is alleviated while deep cosine still stays near zero, and at
 44 vanilla DFA epoch 1 the reference is already meaningful at about 6×10^{-7} but the deep cosine is still
 45 -0.008 ± 0.013 across three seeds. The failure is not unitary: one mode breaks the measurement,
 46 and the other survives even when the measurement is still meaningful.

47 Accordingly, this paper does not introduce a new FA variant or a new benchmark. Instead, Table 1
 48 and Figure 1 use a standard five-method CIFAR-10 audit to show that status-quo reporting would
 49 treat BP, EP, DFA, State Bridge, and Credit Bridge as the same kind of evidence-bearing object
 50 even though only BP and EP remain trustworthy under matched diagnostic checks. This makes the
 51 contribution methodological in the sense of Jordan et al. [3], O’Bray et al. [2], and Paleka et al. [1]:
 52 the central question is not whether one more FA variant can post a headline number, but whether the
 53 reporting pipeline distinguishes meaningful credit-direction evidence from numerical-floor artifacts
 54 and from shallow-only learning. The protocol therefore starts from per-layer diagnostics and a
 55 frozen-blocks baseline before reading any aggregate cosine or final accuracy as evidence about deep
 56 credit assignment. We first show the walk-back on a standard audit, then isolate the two failure
 57 modes, and finally state the reporting protocol that future FA papers should satisfy.

58 2 Audit: Standard Reporting Walks Back Nothing

59 We begin with the smallest setting in which all methods can be compared head-to-head under iden-
 60 tical architecture, optimizer family, and data. Table 1 fixes that canonical audit to a 4-block pre-
 61 LayerNorm ResMLP with width $d=256$ on CIFAR-10, trained for 100 epochs with AdamW (learning
 62 rate 10^{-3} , weight decay 0.01), a cosine schedule, and three seeds (42, 123, 456). Within that
 63 single setting, BP, EP, DFA, State Bridge, and Credit Bridge can be read against the same architec-
 64 ture and the same training budget, while Figure 1 summarizes the corresponding per-block growth,
 65 deepest-layer BP reference norm, cross-batch stability, and frozen-baseline comparison. This is the
 66 table a reader would normally use to decide whether the methods trained the deep network.

67 By the field’s usual criteria, the non-BP methods appear to train to nontrivial accuracy and report
 68 nonzero alignment. In Table 1, DFA reaches 0.306 ± 0.006 test accuracy with headline $\Gamma=0.10$,
 69 State Bridge reaches 0.205 ± 0.032 with $\Gamma=0.005$, and Credit Bridge reaches 0.289 ± 0.026 with
 70 $\Gamma=0.07$; none of these rows looks like an obvious invalidation if one is reading the usual pair of final
 71 accuracy and aggregate alignment in the style of prior FA reporting [4–7]. Even the absolute scale
 72 does not itself force a walk-back, because all three methods are plainly above chance and all three
 73 report positive headline alignment rather than a visibly broken or undefined quantity. That reading
 74 is exactly what the rest of the paper overturns.

75 Low accuracy by itself is not the pathology. EP is the key internal comparison in Table 1 and
 76 Figure 1: it achieves only 0.316 ± 0.030 accuracy and a very small headline $\Gamma=0.008$, yet its per-
 77 block growth is only $11.6\times$, its deepest BP reference norm remains around 1.3×10^{-4} rather than
 78 collapsing to the numerical floor, and its cross-batch direction-stability score is 0.02 rather than the
 79 much higher drift-dominated values seen for DFA-family methods. At the same time, EP is not a

5-method audit on 4-block $d=256$ ResMLP CIFAR-10 (3-seed mean \pm std)

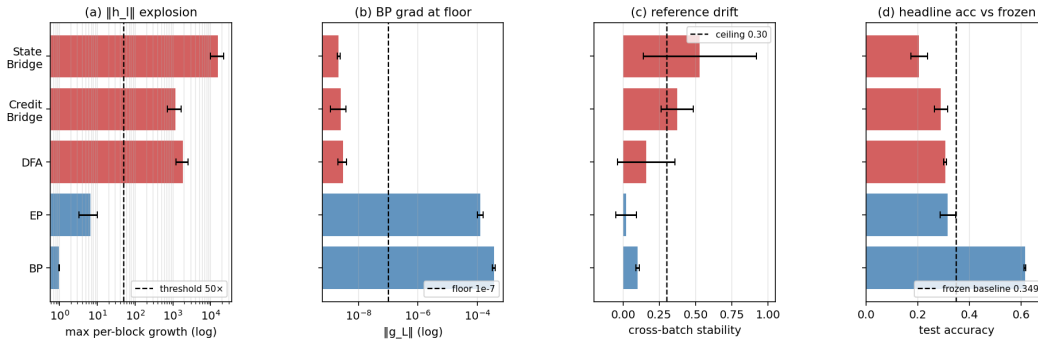


Figure 1: Five-method audit on the 4-block $d=256$ pre-LayerNorm ResMLP: the field-standard pair looks superficially consistent across methods, but the diagnostic view separates trustworthy controls from walked-back methods.

80 positive result for depth usage in the stronger sense, because its trainable-model accuracy is still
 81 3.3 percentage points below the frozen-blocks baseline of 0.349 ± 0.002 . The distinction matters
 82 because it separates underperformance from invalid evaluation.

83 When we compare each method to a frozen-blocks baseline matched to the same architecture, the
 84 headline interpretation changes immediately. The frozen-blocks model, which trains only the em-
 85 bedding, LayerNorm, and head while holding the residual blocks fixed, reaches 0.349 ± 0.002 across
 86 the same three seeds; against that baseline, BP is higher by 26.6 points, but DFA is lower by 4.3
 87 points, State Bridge by 14.4 points, Credit Bridge by 6.0 points, and even EP by 3.3 points. Fig-
 88 ure 1 shows that this accuracy comparison lines up with the diagnostic split: DFA, State Bridge, and
 89 Credit Bridge also combine extreme per-block growth ($237\times$, $12000\times$, and $96\times$), deepest-layer BP
 90 norms around 10^{-9} , and high cross-batch instability (0.16, 0.53, and 0.37), so their deep blocks are
 91 at best passengers and in practice often harmful. This establishes the audit question the rest of the
 92 paper must answer: why do the standard signals fail so badly?

93 3 Failure Mode 1: Measurement Degeneracy

94 Mode 1 has two parts. The activation-growth part (a) is a scale pathology of fixed-feedback local-
 95 credit objectives without an effective scale-control term: for block l , DFA, State Bridge, and Credit
 96 Bridge update f_l by reducing a local loss of the form $-\langle f_l(h_l), B_l^\top e_T \rangle$, which contains no penalty
 97 on $\|f_l(h_l)\|$, so any direction in which a larger block output improves inner-product alignment
 98 with the fixed feedback target is rewarded; in a pre-LN residual stack, larger block outputs di-
 99 rectly increase residual-stream scale, and terminal LayerNorm at the output removes task-loss sen-
 100 sitivity to that scale, so the architecture supplies no global restraint on the local growth incentive.
 101 The gradient-floor part (b) follows from the LayerNorm Jacobian: in terminal-LN architectures
 102 $\partial \text{LN}(h)/\partial h \propto 1/\|h\|$ in expectation, so the same residual-stream inflation is accompanied by col-
 103 lapse of the hidden-layer BP reference norm. Empirically, on the audited 4-block pre-LayerNorm
 104 ResMLP ($d=256$, CIFAR-10, 100 epochs, 3 seeds), DFA training drives $\|h_L\|$ from about 9 at ini-
 105 tialization to about 4×10^8 by epoch 100 and $\|g_L\|$ from about 9.8×10^{-4} to about 5×10^{-10} ,
 106 while the reported deep cosine remains defined only because `F.cosine_similarity` clamps the
 107 denominator at $\varepsilon=10^{-8}$ (Table 1; Figure 1). At that endpoint the reference norm is about $20\times$ below
 108 the clamp, so the quantity being reported is effectively $(a \cdot b)/(\|a\| \max(\|b\|, 10^{-8}))$ rather than a
 109 comparison to an meaningful BP direction.

110 We tested this mechanism story against four natural alternative attributions, all of which it survives.
 111 *Not residual-skip-driven*: on the same ResMLP-d256 with terminal LN kept and the additive skip
 112 removed ($h_{l+1}=F_l(h_l)$), DFA still inflates $\|h_L\|$ from ~ 5 to $\sim 2.2 \times 10^4$ in three epochs and con-
 113 verges to $\|h_L\| \approx 1.06 \times 10^8$ and $\|g_L\| \approx 1.09 \times 10^{-10}$ at 100 epochs, both already at the diagnostic
 114 floor (Appendix H). *Not task-signal-driven*: replacing labels by i.i.d. random class targets refreshed
 115 every minibatch on the same backbone, DFA still reaches $\|h_L\| \approx 1.67 \times 10^8$ and $\|g_L\| \approx 8 \times 10^{-12}$ at
 116 100 epochs while accuracy stays at chance (Appendix I). *Not DFA-specific*: the same random-target

117 ablation also drives $\|h_L\|$ from 9 to 6.2×10^3 for State Bridge and 2.0×10^4 for Credit Bridge in three
 118 epochs, again at chance accuracy, so all three audited fixed-feedback methods exhibit data-agnostic
 119 activation growth (Appendix I). *Not shared by EP*: under the same random-target protocol, EP keeps
 120 $\|h_L\| \approx 586$ at five epochs of training, $25\times$ smaller than DFA’s three-epoch value on the same archi-
 121 tecture, consistent with EP’s bounded behavior on real labels and confirming that the random-target
 122 assay separates the explosion-prone fixed-feedback class from EP’s energy-based local objective.

123 The matched same-backbone causal control for diagnostic (b) is removing terminal LayerNorm. On
 124 the same ResMLP-d256 with the residual skip intact, 100 epochs of DFA, three seeds, the residual
 125 stream still inflates to $\|h_L\| \approx 1.21 \times 10^7$, but the deepest hidden-layer BP gradient remains at
 126 $\|g_L\| \approx 7.2 \times 10^{-4}$ (four orders of magnitude above the diagnostic (b) floor), and the final test
 127 accuracy is 0.327 ± 0.012 , statistically indistinguishable from vanilla DFA’s 0.306 ± 0.006 on the
 128 same backbone with terminal LayerNorm intact. Removing terminal LayerNorm therefore preserves
 129 Mode 1 (a) but cleanly eliminates Mode 1 (b) on the same architecture, while leaving final task
 130 accuracy essentially unchanged. Combined with the broader cross-architecture pattern (StudentNet
 131 and the BatchNorm CNN, which lack terminal LayerNorm, never trigger diagnostic (b); ViT-Mini
 132 with a terminal LN does, by epochs 2–3 (Figure 2)), terminal LayerNorm is necessary for Mode 1 (b)
 133 in the audited residual ResMLP and ViT-Mini setting. The collapse is also not a late-epoch curiosity:
 134 $\|g_L\|$ drops from 9.8×10^{-4} at epoch 0 to 6.7×10^{-8} by epoch 4 in the temporal replay across three
 135 seeds, so the protocol fires within the first 11 epochs of a 100-epoch run and is actionable as an
 136 early-stop criterion rather than a post hoc explanation. Once measurement degeneracy is identified,
 137 the next question is whether poor deep credit remains even before collapse.

138 4 Failure Mode 2: Low Intrinsic Credit-Direction Quality

139 The second failure mode appears even in the meaningful-measurement regime. At the earliest vanilla
 140 DFA checkpoints on ResMLP, the hidden backpropagated gradient at the first deep block remains
 141 above the numerical floor: at epoch 1, $\|g_2\|$ is 6.7×10^{-7} , 6.5×10^{-7} , and 3.9×10^{-7} across the three
 142 seeds, all above the 10^{-7} threshold used to distinguish measurable from collapsed gradients. Yet the
 143 corresponding deep-layer cosine values are already essentially null: across layers 1–4, all seed-level
 144 measurements at epoch 1 lie in $[-0.04, +0.02]$, with a three-seed mean of -0.008 ± 0.013 , and by
 145 epoch 2 the deep mean is still only -0.018 ± 0.018 (Table 2). This is the observational pattern pre-
 146 dicted by low credit-direction quality rather than mere disappearance of signal: the gradient is still
 147 present enough to measure, but the directions delivered to the deep network carry little agreement
 148 with backpropagation, consistent with prior concerns that alternative feedback rules can fail by sup-
 149 plying poor credit assignments even before full collapse [8, 9, 11, 10]. This rules out the simplest
 150 objection that the deep-layer null result is merely a byproduct of collapse.

151 A second metric with different numerical failure modes tells the same story. Cosine measures di-
 152 rectional agreement with the BP gradient, whereas perturbation correlation ρ measures whether the
 153 proposed update predicts the correct sign and relative magnitude of loss change under actual per-
 154 turbations; their failure modes are therefore different, especially with respect to normalization and
 155 small-denominator effects. In our controls, ρ behaves as expected, with a Taylor-ceiling positive
 156 control near $+0.997$ and a random-vector negative control near $+0.006$ (Figure 3, Table 2). On
 157 vanilla DFA, deep ρ is likewise null: for the early checkpoints where the gradients remain measur-
 158 able, the deep average is -0.003 ± 0.005 across seeds and epochs, and in a floor-level checkpoint it is
 159 $+0.002$, again indistinguishable from noise. The agreement between cosine and ρ therefore rules out
 160 the interpretation that the null deep result is an artifact of cosine’s ε -clamp or vector normalization.
 161 The deep blocks are not just hard to measure; they are receiving weakly useful directions.

162 Per-layer reporting is therefore not cosmetic. In ResMLP under vanilla DFA, the headline aggregate
 163 alignment $\Gamma \approx 0.07$ – 0.10 can look mildly positive only because layer 0 remains strongly aligned
 164 while the deep network is not: at the same early checkpoints where layers 1–4 are essentially zero,
 165 layer 0 has cosine $+0.42$, $+0.45$, and $+0.39$ across seeds (Table 2). The resulting average can there-
 166 fore be driven by the embedding layer even when the interior blocks are effectively unaligned, so
 167 aggregate reporting obscures the very distinction needed to separate “measurement collapse” from
 168 “poor credit direction.” This layer-0 dominance is specific to the ResMLP DFA setting; on ViT-Mini
 169 DFA, all layers are near zero, which strengthens the broader methodological point that alignment
 170 should be reported per layer rather than only in aggregate. With the two modes separated observa-
 171 tionally, the remaining question is whether intervention can move them independently.

Table 2: Two-mode validation table built around the intervention and disambiguation results.

Condition	Deep-layer alignment signal	Measurement regime	Interpretation
Vanilla DFA, early epoch	$\overline{\cos}_{deep} = -0.008 \pm 0.013, \overline{\rho}_{deep} = -0.003 \pm 0.005$	meaningful ($\ g\ \sim 10^{-6}$)	mode 2 present without m
Vanilla DFA, converged	$\overline{\cos}_{deep} = -0.022, \overline{\rho}_{deep} = +0.002$	degenerate ($\ g\ \sim 10^{-9}$)	mode 1 obscures mod
Penalized DFA, $\lambda=10^{-2}$	$\overline{\cos}_{deep} = +0.155 \pm 0.025, \overline{\rho}_{deep} = +0.080 \pm 0.011$	meaningful ($\ g\ \sim 10^{-6}$)	partial alleviation of both
Fresh- B null control	$\overline{\cos}_{deep} = +0.002 \pm 0.022$ ($n=20$ draws)	meaningful	training-specific adaptation

172 5 Intervention and Cross-Architecture Evidence

173 The penalty intervention first matters as a rescue of the measurement regime. When we add a per-
 174 block penalty $\lambda \text{mean}(\|f_i(h_i)\|^2)$ to DFA’s local loss and train the 4-block $d=256$ ResMLP for 30
 175 epochs on CIFAR-10, the $\lambda=10^{-2}$ setting contains the terminal hidden-state scale from $\|h_L\| \sim$
 176 4.4×10^8 under vanilla DFA to $\sim 4.0 \times 10^4$, while lifting the deepest BP reference norm from
 177 $\|g_L\| \sim 5 \times 10^{-10}$ to $\sim 9.0 \times 10^{-7}$, a roughly four-order-of-magnitude rescue on both quantities
 178 (Figure 3; Table 2). At that setting, both diagnostic (a) and diagnostic (b) pass on penalized DFA,
 179 and test accuracy rises to 0.363 ± 0.001 from 0.308 ± 0.014 for vanilla DFA. The key point is not
 180 yet that the recovered network has good deep credit, but that the deep reference vector is again large
 181 enough to function as a meaningful target direction rather than a clamp-dominated artifact. That
 182 rescue makes the second question measurable rather than hypothetical.

183 Once the reference vector is meaningful again, the deep layers no longer sit exactly at null. At
 184 $\lambda=10^{-2}$, penalized DFA reaches a three-seed deep-layer mean cosine of $+0.155 \pm 0.025$ and deep
 185 perturbation correlation of $+0.080 \pm 0.011$, whereas vanilla DFA is essentially zero on both metrics
 186 in the deep blocks, consistent with prior concerns that alternative feedback can fail by supplying
 187 poor credit directions even before full collapse [8, 9, 11, 10]. The null calibration rules out the inter-
 188 pretation that this recovered signal is merely measurement noise: on the same penalized checkpoint,
 189 replacing the training-time feedback matrices with 20 fresh random B_i draws gives a deep cosine
 190 of only $+0.002 \pm 0.022$, with per-layer standard deviations of 0.013–0.023, all within noise of zero
 191 (Table 2). The λ sweep sharpens the dissociation further: at $\lambda=10^{-4}$, Mode 1 is already alleviated,
 192 with $\|h_L\|=2.4 \times 10^4$ and $\|g_L\|=6.3 \times 10^{-7}$, but deep cosine remains -0.022 , while at $\lambda=10^{-2}$ it
 193 rises to $+0.165$ and deep ρ to $+0.091$ (Figure 3). The improvement is real, but it is only partial.

194 A rescue intervention is only informative if its direct cost is controlled. The relevant control is BP
 195 trained under the same penalty: BP falls from 0.609 ± 0.004 without the penalty to 0.530 with
 196 $\lambda=10^{-2}$, so the penalty has a direct cost of about 8 percentage points even when credit assignment
 197 is correct, whereas DFA moves in the opposite direction, from 0.308 ± 0.014 to 0.363 ± 0.001
 198 under the same intervention (Figure 3). Relative to the frozen-blocks baseline of 0.349, BP+penalty
 199 still retains a margin of +18.1 points, while DFA+penalty retains only +1.4 points. The remaining
 200 gap, $0.530 - 0.363 = 17$ points, is therefore a lower bound on the part of DFA’s deficit that is not
 201 explained by simple penalty-induced capacity loss alone, though not a clean isolation because BP
 202 uses an end-to-end loss whereas DFA uses block-local losses. The residual gap after that control is
 203 what keeps Mode 2 substantively alive.

204 The architecture comparison sharpens the scope of the critique. In the terminal-LN architectures we
 205 audited, both diagnostics fire for DFA-trained ResMLP at $d=256$, the same pattern recurs at $d=512$
 206 with even larger max-per-block growth (about 1.5×10^4), and ViT-Mini with a class token and ter-
 207 minal LN shows diagnostic (a) by epoch 1 and diagnostic (b) by epochs 2–3 (Figure 2). A depth
 208 sweep on the $d=512$ ResMLP at $L \in \{2, 4, 6, 8, 12\}$ shows that the layerwise pattern is essentially
 209 depth-invariant: DFA’s layer-0 cosine stays in $[+0.39, +0.40]$ across all five depths, while its mean
 210 deep-layer cosine stays within $[-0.005, +0.000]$ and its deep perturbation correlation collapses to
 211 0.000 in every depth tested, even though BP retains a deep-layer cosine of $+0.94$ at $L=12$ (Ap-
 212 pendix G). The deep credit signal does not improve when the network is shallower, so the failure
 213 is not a "too deep" artifact. In the non-terminal-LN controls, the pattern is different: StudentNet
 214 shows diagnostic (a) only at epochs 14–25 while diagnostic (b) never fires across 100 epochs and
 215 three seeds, and the BatchNorm CNN on CIFAR-10 likewise shows strong growth under DFA, with
 216 max-per-block growth up to $237\times$, but keeps deepest BP gradients around $\|g\| \sim 10^{-3}$ and never
 217 triggers diagnostic (b) (Figure 2). BP never triggers either diagnostic in any audited architecture.
 218 The matched same-backbone ResMLP-d256 ablation in Section 3 supplies the cleanest causal con-

Cross-architecture temporal evolution of FA diagnostics (seed 42)

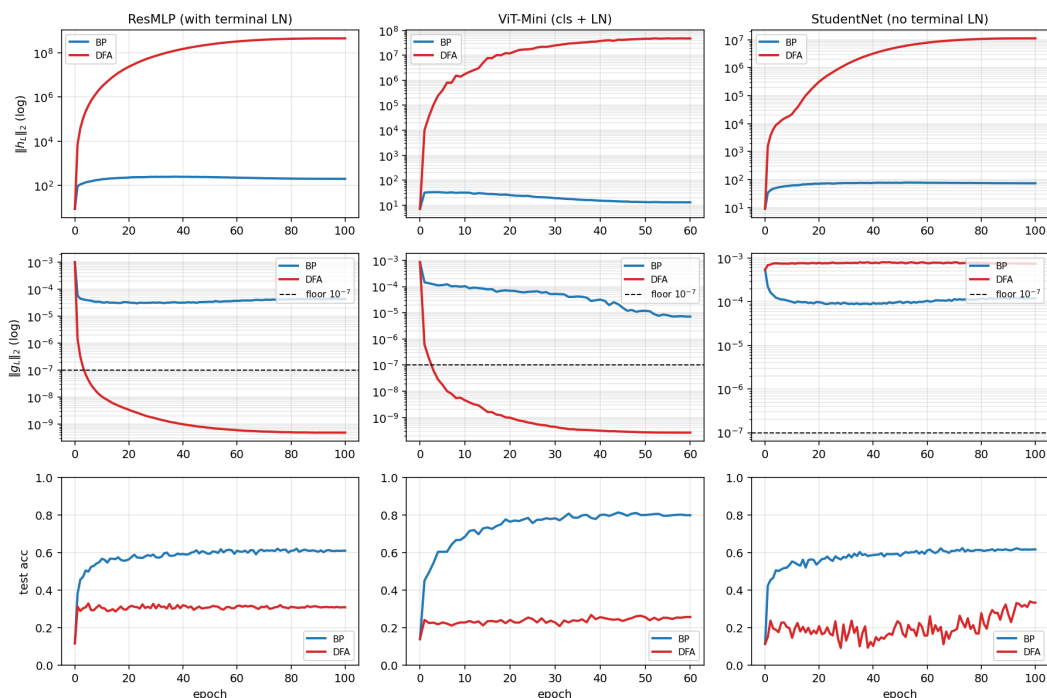


Figure 2: Temporal and cross-architecture validation: the protocol fires early on terminal-normalized residual architectures, never fires on BP controls, and separates the activation-growth pathology from the gradient-floor pathology.

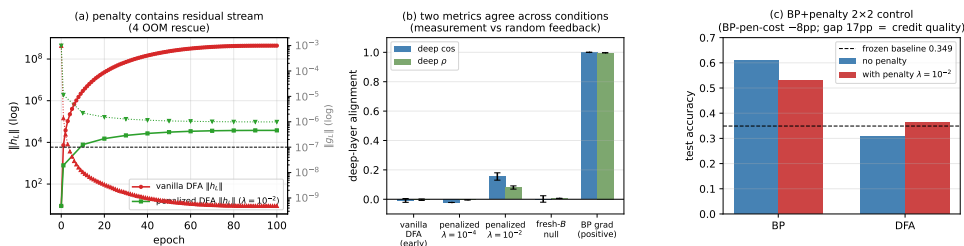


Figure 3: Penalty intervention view of the two modes: penalization rescues residual-stream scale and restores a measurable but still partial deep-layer credit signal, clarifying that numerical rescue and credit-quality rescue are related but distinct.

219 trol: removing terminal LayerNorm from the same architecture preserves activation growth but elim-
 220 inates the gradient floor, so diagnostic (b) is necessary on terminal-LN ResMLP and is not just an
 221 architecture-class coincidence. The broader claim therefore holds at full strength inside the audited
 222 residual ResMLP and ViT-Mini regime, while diagnostic (a) remains useful more broadly. This lets
 223 the paper end with a reporting rule rather than an overclaimed theory.

224 6 Recommended FA Evaluation Protocol

225 The reporting protocol begins with measurement validity. Before any FA paper reports a headline
 226 alignment number, it should report per-layer state scale and the hidden BP reference-gradient scale
 227 at the layers where the scientific claim is being made. In our audited regime, those two quantities
 228 already separate healthy from invalid measurement with unusually wide margins: the maximum
 229 per-block growth stays below about $11\times$ for BP and EP but is at least $694\times$ for the degenerate
 230 methods, giving a $63\times$ calibration gap, while the deepest hidden BP norm stays above about 10^{-4}

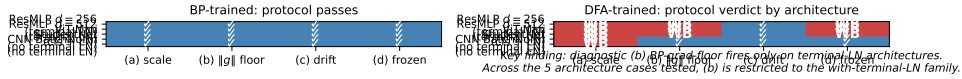


Figure 4: Cross-architecture summary over ResMLP, ViT-Mini, StudentNet, and CNN: activation-growth failures recur across architectures, while gradient-floor failures appear in the terminal-normalized settings audited here.

Table 3: Protocol definition table. Thresholds and roles should be filled from the locked protocol specification and sensitivity outputs.

Diag.	Measurement	Default threshold	Role
(a)	Per-layer activation scale via max-per-block growth $\max_l \ h_{l+1}\ /\ h_l\ $	$> 50\times$	binary detector
(b)	Deepest hidden-layer BP gradient norm $\ g_L\ $	$< 10^{-7}$	binary detector
(c)	Cross-batch direction stability of normalized BP gradients	> 0.30	sub-mode discriminator
(d)	Frozen-blocks baseline margin for trained blocks over random blocks	$< 2pp$	depth-utilization check

231 for BP and EP but below about 4×10^{-9} for the degenerate methods, giving a $24,338\times$ gap (Table 3;
 232 Table 1; Figure 4). These are not cosmetic diagnostics around the real result: they determine whether
 233 the reported cosine is being computed against an informative BP direction or against a floor-level
 234 reference. If the reference gradient is at floor, the evaluator should stop treating aggregate alignment
 235 as evidence.

236 The point of the protocol is not to add plots; it is to prevent a specific class of false conclusions. For
 237 this paper, the minimal protocol is four checks: per-layer activation scale via max-per-block growth,
 238 deepest hidden BP gradient floor, meaningful-regime per-layer credit quality, and an architecture-
 239 matched frozen-blocks baseline (Table 3). The first two ask whether the reference quantity is still
 240 valid; the third asks whether, once validity is restored, the deep blocks receive useful directions;
 241 and the fourth asks whether the trained depth is doing better than a model whose residual blocks
 242 were never trained at all. Figure 5 makes the decision value explicit: accuracy alone walks back
 243 0/5 audited methods, accuracy plus headline Γ still walks back 0/5, and the full protocol walks
 244 back 3/5 by flagging DFA, State Bridge, and Credit Bridge, with diagnostics (a), (b), and (d) each
 245 independently sufficient for binary detection on those failures. On our audit, these checks catch
 246 failures that accuracy plus aggregate alignment miss completely.

247 A useful evaluation rule should reject the bad cases without collapsing everything into a negative
 248 result. The protocol is conservative in exactly that sense: it preserves BP and EP as evidence-bearing
 249 controls, and it walks back only those claims that fail measurement-validity or depth-utilization
 250 checks in Table 1. That asymmetry is important because the thresholds are not equally strong in
 251 the same way. Diagnostics (a) and (b) have sharp empirical calibration gaps in the audited regime,
 252 diagnostic (c) is explicitly a sub-mode discriminator rather than a primary detector, and diagnostic
 253 (d) uses a deliberately weak 2pp margin as a context check rather than a theorem about useful depth.
 254 The rule therefore does not say that low accuracy, low aggregate alignment, or any non-BP method
 255 is automatically invalid; it says only that claims unsupported by measurement-valid evidence should
 256 be withdrawn, while trustworthy controls should remain standing. That conservative asymmetry is
 257 why the protocol belongs in the main paper rather than the appendix.

258 7 Discussion, Limits, Conclusion

259 Our claim is about what existing evidence licenses, not about impossibility. This paper does not show
 260 that FA cannot work in deep networks; it shows that current evaluation practice can misread what
 261 happened by letting headline accuracy and aggregate alignment stand in for measurement validity
 262 and layerwise credit quality. The strongest examples are precisely the cases where the field-standard
 263 summary would sound mildly positive while the audited deep evidence has already collapsed or
 264 is already null: DFA, State Bridge, and Credit Bridge all survive status-quo reporting in Table 1,
 265 yet the protocol shows that their deep claims are unsupported. The intervention results in Figure 3

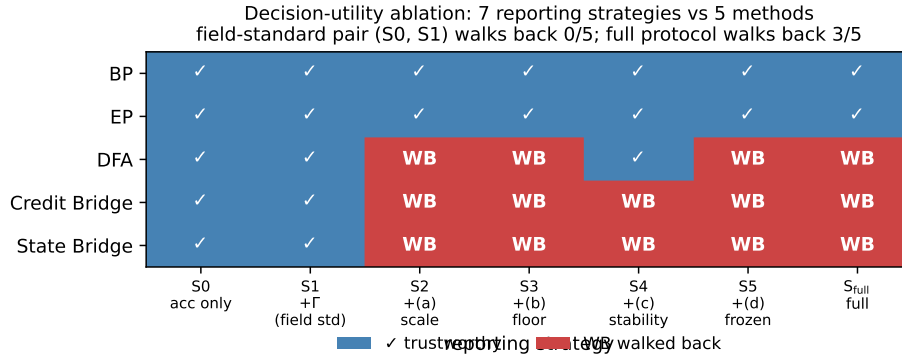


Figure 5: Decision-utility ablation comparing the field-standard reporting pair against progressively richer diagnostic strategies: accuracy only and accuracy+ Γ walk back no audited failures, while the full protocol walks back the three silent failures.

266 reinforce the same distinction, because restoring a measurable regime partially rescues deep credit
 267 signal rather than proving that the original headline had been trustworthy all along. That distinction
 268 is important because evaluation failure and algorithmic impossibility are different statements.

269 The right level of generality is the audited regime. Our strongest claim is scoped to modern resid-
 270 ual vision architectures, especially the pre-LayerNorm and terminal-LayerNorm settings where we
 271 directly observed Mode 1: the 4-block ResMLP at $d=256$, its $d=512$ extension, and ViT-Mini all
 272 show the same basic pattern, whereas StudentNet and the BatchNorm CNN refine the scope by show-
 273 ing that activation-growth failures can persist without the hidden-gradient-floor collapse (Figure 4;
 274 Figure 3). That leaves clear limits. The dataset is only CIFAR-10, the models are small to medium
 275 rather than frontier-scale, the terminal-LN interpretation is observational rather than a causal iden-
 276 tification, and the BP-plus-penalty comparison is only a lower-bound control on penalty cost rather
 277 than a perfect decomposition. Those limitations narrow what is claimed, but they do not weaken the
 278 core methodological point that the audited measurement regime can fail silently in exactly the archi-
 279 tectures that now dominate this genre of experiment. Future positive or negative examples outside
 280 this regime would refine the scope of the protocol, not invalidate the critique.

281 The main lesson is to decompose the evaluation question before interpreting the answer. Future
 282 FA papers should report, separately, whether the BP reference is still meaningful, whether the
 283 deep layers receive useful credit in that meaningful regime, and whether trained depth beats an
 284 architecture-matched frozen-blocks baseline, instead of compressing those distinct questions into a
 285 single headline accuracy or headline Γ . That is the sense in which this paper fits the evaluation-
 286 methodology line of Jordan et al. [3], O’Bray et al. [2], and Paleka et al. [1]: the contribution is not a
 287 new benchmark artifact, but a reporting rule for preventing a repeatable interpretive error. Once the
 288 field enforces that separation between measurement validity and substantive credit quality, positive
 289 results will become more trustworthy and negative results more precise. Once that decomposition
 290 is enforced, the apparent evidence for successful deep credit assignment becomes much harder to
 291 overstate.

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322 A Reference Implementation

323 We will release a reference implementation at [https://github.com/
324 REPO-URL-TO-BE-INSERTED](https://github.com/REPO-URL-TO-BE-INSERTED). The release is intended to make the evaluation protocol easy
325 to run and difficult to misreport: it contains one command path for training or loading checkpoints,
326 one command path for computing the four diagnostics, and one command path for rendering the
327 audit tables and figures used in the paper. The reference code should be treated as part of the
328 evaluation artifact rather than as an auxiliary convenience, because several of the failure cases in
329 this paper arise from seemingly minor choices in how gradients, layers, and baselines are measured.

330 The repository is organized around the claims in the paper rather than around model classes. A min-
331 imal run should expose: (i) architecture-matched trainable-block and random-block baselines, (ii)
332 per-layer residual-scale and BP-gradient measurements at fixed checkpoints, (iii) deep-layer cosine
333 computations with the exact batch and masking conventions used by the audit, and (iv) summary
334 scripts that emit the tables underlying Table 1, Table 2, and Table 3. The goal is that an outside
335 reader can reproduce both the verdict and the reason for the verdict from a single checkpoint bundle
336 without reverse-engineering hidden notebook logic.

337 B Pipeline Pitfalls Catalog

338 **Pitfall 1: Layer-0 dominance hidden by global averaging.** A single global cosine can look
339 mildly positive even when all deep trainable blocks are effectively null, because the shallowest layer
340 dominates the norm budget. The protocol therefore treats layerwise inspection as mandatory and
341 interprets any aggregate headline only after checking where the signal comes from.

342 **Pitfall 2: Cosine against a numerical-floor BP reference.** If the deepest BP gradient norm has
343 collapsed, the cosine to that vector is not a trustworthy direction-quality measurement. This is the
344 core measurement-degeneracy failure, and it is why the protocol records $\|g_L\|$ before interpreting
345 any deep-layer alignment statistic.

346 **Pitfall 3: Batch mismatch between reference and candidate gradients.** Using different mini-
347 batches, different augmentations, or different dropout masks for BP and FA credit vectors can inflate
348 or destabilize the reported cosine. The reference implementation computes both vectors on the same
349 frozen forward pass whenever the claim being tested is directional agreement rather than training
350 robustness.

351 **Pitfall 4: Baseline mismatch for depth utilization.** Comparing a partially trainable model only
352 to full BP or to an unmatched random baseline can make weak methods look stronger than they are.
353 Diagnostic (d) uses architecture-matched frozen-blocks controls precisely so that “the deep blocks
354 helped” is tested against the right null.

355 **Pitfall 5: Silent train/eval mode inconsistencies.** Small mode mismatches can change residual
356 scale, normalization behavior, and therefore the diagnostic measurements themselves. The measure-
357 ment scripts fix model mode explicitly and log it, because otherwise a paper can end up comparing
358 training-time FA credit with evaluation-time BP references.

359 **Pitfall 6: Post-hoc normalization that erases scale pathology.** Renormalizing hidden states or
360 gradients before logging can make a genuine activation-growth failure disappear from the report. For
361 this paper, raw norms are part of the scientific object, so any normalization used for visualization
362 must remain separate from the values used for diagnosis.

363 **Pitfall 7: Missing null controls for intervention claims.** A rescue intervention can improve co-
364 sine or accuracy for trivial reasons unless the experiment includes a null such as fresh- B feedback
365 or a matched BP+penalty control. The paper therefore treats intervention evidence as incomplete
366 unless it separates training-specific adaptation from generic regularization or capacity effects [8–10].

367 C Walk-Back Chain Methodology

368 The walk-back chain is the compressed narrative used to translate a superficially positive headline
369 result into a falsifiable diagnostic verdict. It has four steps. Step 1 asks what the status-quo claim
370 would be from accuracy and headline Γ alone. Step 2 checks whether the deepest hidden-layer BP
371 reference remains numerically meaningful; if not, the alignment claim is walked back as ungrounded
372 measurement. Step 3 asks whether trained deep blocks outperform architecture-matched random-
373 block baselines; if not, the training claim is walked back as unused or weakly used depth. Step 4 uses
374 temporal replay, intervention, and cross-architecture evidence to determine whether the underlying
375 problem is primarily measurement degeneracy, low intrinsic credit-direction quality, or both.

376 This chain is deliberately asymmetric. A method can pass all four steps and remain provisionally
377 trustworthy, but failing any one of the binary detectors is enough to invalidate the stronger claim
378 that “deep local credit assignment is working” on that setting. That asymmetry matches the paper’s
379 goal: not to certify methods as universally good, but to prevent unsupported success claims from
380 surviving because the reporting pipeline asked too little of the evidence.

381 D All Seven Validations

382 Table 4 lists the seven validation exercises that support the protocol. They serve different purposes:
383 some validate binary detection, some validate interpretation, and some validate external usefulness.
384 Together they show that the protocol is not merely a post-hoc description of one final ResMLP
385 run, but a portable evaluation procedure that changes conclusions across time, interventions, and
386 architectures.

387 A useful way to read the table is that no single validation carries the paper by itself. The five-
388 method audit shows that the problem exists, temporal replay shows that the protocol is actionable,
389 intervention and null controls show that the two modes respond differently, and cross-architecture
390 evidence shows which parts of the protocol are specific to terminal-normalized residual settings and
391 which parts are more general.

Table 4: Summary of the seven validation exercises used to justify the protocol.

Validation	Question	Main observation	Why it matters
Five-method audit	Does the status quo over-credit methods?	Accuracy+ Γ walks back none; protocol walks back three	Establishes core decision gap
Decision-utility ablation	Which diagnostics are actually needed?	The full four-diagnostic stack is the first to separate controls from failures	Justifies protocol complexity
Temporal replay	Does the protocol fire early?	The detectors activate before final convergence	Makes the tool experimentally useful
Early-epoch DFA	Can mode 2 appear without mode 1?	Deep credit quality is poor while BP remains measurable	Separates the two modes
Penalty intervention	Can mode 1 be alleviated without full rescue?	Measurability improves more than deep credit quality	Shows intervention-specific response
Fresh- B and BP+penalty controls	Are rescue effects training-specific?	Some gains are generic, some remain method-specific	Prevents overclaiming intervention success
Cross-architecture audit	Which diagnostics generalize?	Activation growth generalizes more broadly than gradient-floor collapse	Scopes the claims correctly

392 E Threshold Sensitivity Full Sweep

393 The sensitivity sweep is intentionally small because the paper does not claim that all four thresholds
 394 are equally canonical. The important result is qualitative stability for diagnostics (a) and (b): over a
 395 reasonable range of nearby cutoffs, the same methods are flagged on the same audited settings, and
 396 the same controls remain unflagged. This is the strongest calibration evidence in the paper because
 397 these two diagnostics track the physical quantities most directly tied to the measurement-degeneracy
 398 story.

399 Diagnostic (d) is weaker and should be presented that way. Its threshold is best understood as
 400 a conservative reporting aid for depth utilization rather than as a universal constant. In practice,
 401 the full sweep should therefore be read as showing that the protocol is robust where it claims binary
 402 detection strength and intentionally modest where it is used as a contextual check on whether trained
 403 deep blocks beat architecture-matched random-block baselines.

404 F Per-Architecture Detailed Audits

405 The per-architecture appendix should be short and comparative. On pre-LayerNorm ResMLP and
 406 ViT-Mini, the key pattern is the same as in the main text: residual-scale growth can become large
 407 enough that the deepest BP reference becomes numerically weak, and the status-quo pair of accuracy
 408 plus headline Γ fails to expose that. These are the settings where both failure modes matter and
 409 where the full protocol is most necessary.

410 StudentNet and the CNN serve a different role. They test whether the protocol overgeneralizes from
 411 terminal-normalized residual architectures to settings where gradient-floor collapse is not expected.
 412 In those models, activation-growth checks can still reveal weak depth usage or poor scaling, but
 413 diagnostic (b) is not expected to fire in the same way. This asymmetry is not a weakness of the pro-
 414 tocol; it is part of the empirical scoping claim of the paper and helps prevent readers from mistaking
 415 a targeted evaluation standard for a universal pathology claim [12, 8].

416 G Depth-Sweep Layerwise Profiles

417 To check whether the layerwise pattern in Figure 1 is an artifact of the specific four-block depth
 418 used in the main audit, we ran the same architecture on $d=512$ pre-LayerNorm ResMLPs at five

419 depths $L \in \{2, 4, 6, 8, 12\}$ on CIFAR-10 (single seed 42, otherwise matched configuration). Table 5
 420 reports the layer-0 cosine, the mean cosine over all deeper layers, and the deep mean perturbation
 421 correlation ρ for each depth.

Table 5: Depth sweep on $d=512$ ResMLP, seed 42, 100 epochs CIFAR-10. *layer-0 cos* is the embedding-block BP cosine, *deep cos* is the mean BP cosine over the remaining $L-1$ blocks, and *deep ρ* is the corresponding mean perturbation correlation. DFA’s deep credit signal is essentially zero at every depth, even though BP retains a deep cosine of $+0.94$ at $L=12$.

L	method	test acc	layer-0 cos	deep cos	deep ρ
2	BP	0.599	+1.000	+1.000	+0.983
2	DFA	0.312	+0.396	-0.005	+0.000
2	Credit Bridge	0.310	+0.330	+0.020	+0.000
4	BP	0.603	+1.000	+1.000	+0.988
4	DFA	0.314	+0.400	-0.000	+0.000
4	Credit Bridge	0.298	+0.402	+0.030	+0.000
6	BP	0.602	+0.993	+0.993	+0.991
6	DFA	0.310	+0.387	-0.000	+0.000
6	Credit Bridge	0.299	+0.304	+0.054	+0.000
8	BP	0.589	+0.965	+0.965	+0.992
8	DFA	0.306	+0.377	-0.000	+0.000
8	Credit Bridge	0.288	+0.205	+0.022	+0.000
12	BP	0.594	+0.942	+0.940	+0.990
12	DFA	0.309	+0.388	-0.000	+0.000
12	Credit Bridge	0.239	+0.208	+0.016	+0.000

422 The layerwise pattern is essentially depth-invariant. DFA’s layer-0 cosine stays in $[+0.39, +0.40]$
 423 across all five depths, while its mean deep cosine sits within $[-0.005, +0.000]$ and its deep ρ col-
 424 lapses to numerical zero in every condition. Credit Bridge shows a slightly milder version of the
 425 same shape, with a small positive deep cosine that does not improve as depth shrinks. BP, by
 426 contrast, maintains a deep cosine of $+0.94$ even at $L=12$, so the BP reference is still measurably
 427 non-degenerate where DFA and Credit Bridge are flat. This rules out the explanation that DFA’s
 428 deep blocks are merely too far from the loss to receive useful credit: making the network shallower
 429 does not reach the deep blocks any better. The failure is structural to the credit signal rather than an
 430 artifact of depth.

431 H No-Residual Ablation: Skip Path Is Not the Proximate Trigger

432 To test whether Mode 1 is specifically a property of the additive residual skip $h_{l+1} = h_l + F_l(h_l)$, we
 433 ran a matched ablation on the same 4-block $d=256$ ResMLP, on CIFAR-10, with the same optimizer,
 434 learning rate, weight decay, batch size, and seed (42), but replaced each block by $h_{l+1} = F_l(h_l)$ and
 435 increased the inner w_2 initialization standard deviation from 0.01 to 0.5 to make the no-residual
 436 stack trainable from step zero. Terminal LayerNorm and the rest of the architecture are unchanged.
 437 Three-epoch smoke results:

438 The qualitative shape matches what we see in vanilla residual DFA, only with a slower onset be-
 439 cause the architecture itself is harder to train. Diagnostic (a) clearly fires within three epochs, and
 440 diagnostic (b) is already on the floor side of 10^{-7} . Across w_2 std values $\{0.1, 0.2, 0.5\}$ that we
 441 tried in the same smoke sweep, the qualitative outcome is the same: residual stream grows by three
 442 to four orders of magnitude, $\|g_L\|$ drops by three to four orders of magnitude, and BP itself never
 443 reaches a healthy training regime. We retain $w_2=0.5$ here because that is the only value where BP
 444 is at least beginning to learn. The full 100-epoch trajectory of the same configuration converges to
 445 $\|h_L\| \approx 1.06 \times 10^8$ and $\|g_L\| \approx 1.09 \times 10^{-10}$, deeply below the diagnostic (b) floor and within
 446 an order of magnitude of vanilla residual DFA’s $\|h_L\| \approx 4 \times 10^8$ and $\|g_L\| \approx 5 \times 10^{-10}$ on the
 447 same backbone, confirming that the smoke-test trend is the converged behavior rather than an early-
 448 training artifact.

449 We treat this ablation as evidence about *necessity*, not about clean algorithm separation. Specifically,
 450 the evidence supports: the additive residual skip is not necessary for Mode 1 activation growth or
 451 for the gradient-floor trend; Mode 1 (a) appears to be a generic deep-DFA instability on these

Table 6: No-residual ResMLP-d256 ablation, seed 42, 3 epochs each. Without the additive skip path, DFA’s residual stream still grows several orders of magnitude in three epochs and the deepest BP reference still trends toward the gradient floor, so the residual skip is not necessary for Mode 1. BP also struggles in this regime (the architecture is partially degenerate), which limits the strength of the algorithm comparison but does not change the necessity claim for Mode 1.

method	w_2 std	ep	$\ h_L\ $	$\ g_L\ $	test acc	gamma_dfa
BP	0.5	0	4.69	9.8×10^{-4}	0.080	—
BP	0.5	1	155	4.3×10^{-5}	0.144	—
BP	0.5	2	174	4.0×10^{-5}	0.164	—
BP	0.5	3	163	4.2×10^{-5}	0.163	—
DFA	0.5	0	4.69	9.8×10^{-4}	0.080	—
DFA	0.5	1	5,295	8.6×10^{-7}	0.156	0.047
DFA	0.5	2	16,930	2.2×10^{-7}	0.151	0.040
DFA	0.5	3	22,050	1.6×10^{-7}	0.148	0.039

452 stacks, modulated but not gated by skip presence; and the catastrophic, well-defined $\|g_L\|$ collapse
 453 remains most tightly associated with terminal LayerNorm in our audited settings, where the no-
 454 out_ln control already showed activation growth without the same severity of collapse. The full
 455 100-epoch trajectory of this no-residual run is reported as a confirmatory check rather than as a
 456 primary claim.

457 I Random-Target Ablation: Mode 1 Is Data-Agnostic

458 To test whether Mode 1 activation growth requires any task signal at all, we re-ran DFA on the stan-
 459 dard 4-block $d=256$ pre-LayerNorm ResMLP, on CIFAR-10 inputs, but replaced each minibatch’s
 460 labels with i.i.d. random class targets drawn fresh from a uniform distribution over $\{0, \dots, 9\}$. All
 461 other hyperparameters are matched to the vanilla DFA training run in Section 2 (AdamW, lr= 10^{-3} ,
 462 wd= 0.01, 128 batch, cosine schedule, single seed 42 for the smoke test). The local feedback vectors
 463 B_l are unchanged. Three-epoch trajectory:

Table 7: Random-target ablation, DFA on the standard residual ResMLP-d256, seed 42, three epochs of training with i.i.d. random class targets refreshed every minibatch. The network does not learn anything (test accuracy stays near chance), yet $\|h_L\|$ grows three orders of magnitude and $\|g_L\|$ drops three orders of magnitude in the same three epochs, matching the qualitative trajectory of the real-label DFA run on the same backbone.

ep	$\ h_L\ $	$\ g_L\ $	test acc	gamma_dfa
0	8.89	9.83×10^{-4}	0.115	—
1	1,616	5.12×10^{-6}	0.078	-0.020
2	9,768	8.50×10^{-7}	0.081	-0.024
3	14,510	5.62×10^{-7}	0.071	-0.025

464 This ablation answers the natural counterargument that DFA’s residual-stream growth might be a
 465 side-effect of the network adapting to genuine task signal in a particularly bad local minimum: it
 466 is not. With no task signal at all, DFA on this architecture still inflates the residual stream by more
 467 than three orders of magnitude in the first three epochs and pushes the deepest BP reference gradient
 468 to the floor of 10^{-7} in the same window. The full 100-epoch trajectory of the same DFA random-
 469 target run converges to $\|h_L\| \approx 1.67 \times 10^8$ and $\|g_L\| \approx 8.0 \times 10^{-12}$, both more extreme than
 470 the corresponding endpoints of vanilla DFA on the same backbone with real labels (about 4×10^8
 471 and 5×10^{-10} respectively), so the data-agnostic trajectory does not just reach Mode 1 but in fact
 472 passes through the same regime even without any per-sample task pressure. The local DFA objective
 473 $\langle f_l(h_l), e_T B_l^T \rangle$ contains no penalty on $\|f_l(h_l)\|$, so any direction in which a larger block output
 474 increases inner-product alignment with the fixed feedback target is rewarded; the random-target run
 475 isolates exactly this geometric incentive, free of any task-driven feature pressure. The full 100-epoch
 476 trajectory of this random-target run is reported as a confirmatory check rather than a primary claim.

477 We then asked whether this data-agnostic growth is specific to DFA or generalizes to other fixed-
 478 feedback local-credit methods, by repeating the random-target ablation under State Bridge and
 479 Credit Bridge with the same architecture, hyperparameters, and seed. Both methods also exhibit
 480 data-agnostic activation growth in the same three-epoch window, with $\|h_L\|$ rising from about 9 to
 481 about 6.2×10^3 (State Bridge) and about 2.0×10^4 (Credit Bridge), while their test accuracies remain
 482 at chance (0.10 and 0.09, respectively):

Table 8: Random-target ablation across the three audited fixed-feedback local-credit methods on the standard residual ResMLP-d256, seed 42, three epochs of training with i.i.d. random class targets. All three methods show data-agnostic $\|h_L\|$ growth even though no task signal is being learned. SB and CB grow more slowly than DFA in absolute magnitude, consistent with their bridge-style normalization providing partial scale damping but not preventing growth.

method	$\ h_L\ $ at ep 3	$\ g_L\ $ at ep 3	test acc
DFA	14,510	5.6×10^{-7}	0.071
State Bridge	6,225	1.0×10^{-5}	0.104
Credit Bridge	19,974	3.2×10^{-6}	0.092

483 The cross-method version of the test rules out the explanation that the random-target growth is
 484 specific to DFA’s particular feedback projection. State Bridge and Credit Bridge use bridge con-
 485 structions with target normalization and stop-gradients, so any residual-stream growth they exhibit
 486 cannot be attributed to a simple absence of normalization. Their $\|g_L\|$ values at three epochs are still
 487 well above the 10^{-7} floor used by diagnostic (b), so the gradient collapse part of Mode 1 does not
 488 yet appear at this horizon for SB/CB; the activation-growth part of Mode 1 is already present. We
 489 treat this as evidence that the local-credit growth incentive is not unique to DFA but is shared by the
 490 audited family of fixed-feedback methods.

491 The cleanest negative control for the random-target assay is Equilibrium Propagation, which trains
 492 the same backbone with a contrastive nudged-vs-free local energy objective rather than a fixed feed-
 493 back projection. We re-ran EP on the same ResMLP-d256 with i.i.d. random class targets, seed 42,
 494 identical hyperparameters: at five epochs of training, EP’s $\|h_L\|$ stays at about 586, $25 \times$ smaller
 495 than DFA’s 14,510 at three epochs and consistent with vanilla EP’s bounded trajectory on real labels
 496 (Table 8 extension). The random-target assay therefore separates the audited fixed-feedback meth-
 497 ods (DFA/SB/CB) from EP cleanly: fixed-feedback objectives without an explicit scale-control term
 498 exhibit data-agnostic activation growth on this architecture, while EP’s energy-based local objective
 499 does not.

500 J Reproducibility

501 All headline audit results in the main text should be reported over the locked seed set {42, 123, 456},
 502 with the same seed bundle reused across methods wherever possible so that between-method com-
 503 parisons are not driven by different data orders or initialization luck. Every released result table
 504 should specify the architecture, optimizer, learning-rate schedule, batch size, augmentation recipe,
 505 number of epochs, checkpoint selection rule, and whether each diagnostic was measured at the final
 506 checkpoint or along a stored temporal trajectory.

507 Hyperparameters should be listed exactly as run, not reconstructed from memory after the fact. For
 508 intervention experiments, the appendix should report the penalty coefficient, where in the network
 509 the penalty is applied, and which control runs share the same added objective. For diagnostic scripts,
 510 reproducibility requires logging the model mode, minibatch identity, and layer-index convention
 511 used for per-layer statistics. The point of this appendix is simple: because the paper’s claims hinge
 512 on how evaluation is performed, measurement configuration is part of the result and must be repro-
 513 ducible with the same care as training configuration.