The Stability Envelope: A Formal Framework for Autoregressive Stability in Physics-Informed Neural Networks

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***Abstract*—For model predictive control, learned dynamics models must remain accurate over multi-step prediction horizons. We introduce the stability envelope H_ϵ—the maximum horizon where prediction error stays below threshold ϵ—as a formal metric linking the Lipschitz constant of learned dynamics to usable MPC horizon. We derive closed-form bounds: for Lipschitz constant $L > 1$ and single-step error e_1, the stability envelope satisfies $H_\epsilon \leq \log(1+\epsilon(L-1)/e_1)/\log L$. This framework enables principled model selection for control: given required horizon K and tolerance ϵ, verify $H_\epsilon \geq K$ before deployment. We validate on 6-DOF quadrotor dynamics with 20-seed experiments, demonstrating that architecture choice (modular vs monolithic) significantly affects prediction variance (Levene's $p = 0.034$) even when mean performance differences are not significant ($p = 0.103$). Real data validation on EuRoC MAV (269K samples) achieves 0.053m error over 100-step rollouts. The stability envelope provides a principled alternative to single-step metrics for evaluating learned dynamics models.**

I. INTRODUCTION

Model Predictive Control (MPC) requires accurate predictions over horizons of 50–100+ steps. When using learned dynamics models, prediction errors accumulate through autoregressive rollout: each prediction feeds as input to the next, compounding small errors into large deviations. The critical question for deployment is: *how far into the future can we trust the model's predictions?*

Existing metrics—single-step MSE, physics loss residuals—measure local accuracy but do not answer this question. A model with excellent single-step accuracy can still diverge rapidly under rollout if its Lipschitz constant exceeds 1.

We introduce the stability envelope H_ϵ: the maximum prediction horizon where error remains below threshold ϵ. Through Lipschitz analysis, we derive closed-form bounds linking single-step error e_1 and Lipschitz constant L to usable horizon:

$$H_\epsilon \leq \frac{\log\left(1 + \frac{\epsilon(L-1)}{e_1}\right)}{\log L} \quad (L > 1) \tag{1}$$

This framework enables *principled model selection for control*: given MPC horizon K and error tolerance ϵ, verify $H_\epsilon \geq K$ before deployment.

Core Contributions:

1) **Stability envelope framework**: Formal metric H_ϵ linking Lipschitz constant to usable MPC horizon, with closed-form bounds (Sec. III–IV)
2) **Practical bounds**: Spectral normalization design rules to enforce $L < L_{\text{target}}$ (Theorem 2)
3) **Empirical validation**: 20-seed experiments on quadrotor dynamics demonstrating that architecture significantly affects variance ($p = 0.034$) even when mean differences are not significant ($p = 0.103$)
4) **Real data**: Validation on EuRoC MAV dataset (269K samples, 11 sequences)

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II. PROBLEM FORMULATION

A. Dynamics Learning Setting

Consider a dynamical system with state $\mathbf{x} \in \mathbb{R}^n$ and control $\mathbf{u} \in \mathbb{R}^m$:

$$\dot{\mathbf{x}} = f(\mathbf{x}, \mathbf{u}; \boldsymbol{\theta}) \tag{2}$$

where $\boldsymbol{\theta}$ denotes physical parameters. A PINN learns $g_\phi : \mathbb{R}^{n+m} \to \mathbb{R}^n$ predicting the next state:

$$\hat{\mathbf{x}}_{t+1} = g_\phi(\mathbf{x}_t, \mathbf{u}_t) \tag{3}$$

Although PINNs are commonly used to enforce differential equation structure via collocation, in control applications the PINN serves as a discrete-time dynamics map. Our Lipschitz analysis therefore applies to the learned transition function g_ϕ rather than the continuous vector field.

B. Autoregressive Rollout

For control applications, predictions recursively feed as inputs:

$$\hat{\mathbf{x}}_{t+k} = g_\phi^{(k)}(\mathbf{x}_t, \mathbf{u}_{t:t+k-1}) = g_\phi(g_\phi^{(k-1)}(\cdot), \mathbf{u}_{t+k-1}) \tag{4}$$

with $g_\phi^{(1)} = g_\phi$. The model encounters states $\hat{\mathbf{x}}_{t+k}$ potentially outside the training distribution.

C. Experimental System

We study a 6-DOF quadrotor with 12-dimensional state:

$$\mathbf{x} = [x, y, z, \phi, \theta, \psi, p, q, r, v_x, v_y, v_z]^T \tag{5}$$

The dynamics exhibit strong coupling between translation and rotation via:

$$\ddot{z} = -\frac{T \cos\theta \cos\phi}{m} + g \tag{6}$$

D. Assumptions

We make the following assumptions:

1) **State domain**: States remain within training bounds: $\|p\| \leq 2$ m, $|\phi|, |\theta| \leq 0.5$ rad, $\|v\| \leq 2$ m/s. Since the PINN operates in this bounded domain, local Lipschitz constants serve as practical substitutes for global bounds.
2) **Control bounds**: Thrust $\in [0.5, 1.0]$ (normalized), torques $\in [-0.1, 0.1]$. Controls are treated as exogenous

bounded inputs; Lipschitz continuity is evaluated w.r.t. the state dimension.
3) **Error model**: We adopt the standard additive error model; multiplicative or correlated errors can only increase amplification, so our bounds remain conservative.
4) **Local analysis**: Lipschitz constants are empirical local Jacobian norms within the training distribution.

III. THE STABILITY ENVELOPE FRAMEWORK

A. Formal Definition

Definition 1 (Stability Envelope)**.** *For a learned dynamics model g_ϕ, error threshold $\epsilon > 0$, and test distribution $\mathcal{D}$, the **stability envelope** is:*

$$H_\epsilon = \max\left\{K : \mathbb{E}_{(\mathbf{x},\mathbf{u})\sim\mathcal{D}}\left[\|\hat{\mathbf{x}}_{t+K} - \mathbf{x}_{t+K}\|\right] < \epsilon\right\} \tag{7}$$

where $\hat{\mathbf{x}}_{t+K}$ is the K-step autoregressive prediction.

The stability envelope captures the *usable prediction horizon* for control. A model with excellent single-step accuracy but small H_ϵ is unsuitable for MPC.

B. Relationship to Single-Step Metrics

Let $e_1 = \mathbb{E}[\|\hat{\mathbf{x}}_{t+1} - \mathbf{x}_{t+1}\|]$ denote single-step error. Under an additive error model with Lipschitz constant L, each step introduces error e_1 while amplifying accumulated error by L:

$$e_k \le L \cdot e_{k-1} + e_1 \tag{8}$$

For $L > 1$, the dominant asymptotic behavior is exponential growth $e_k \sim e_1 L^k/(L-1)$. The exact finite-horizon bound (Theorem 1) is:

$$H_\epsilon \le \frac{\log\left(1 + \frac{\epsilon(L-1)}{e_1}\right)}{\log L} \tag{9}$$

For large $\epsilon(L-1)/e_1$, this simplifies to $H_\epsilon \approx \log(\epsilon(L-1)/e_1)/\log L$.

For $L < 1$, errors converge to $e_1/(1-L)$; if $\epsilon > e_1/(1-L)$, then $H_\epsilon = \infty$.

Remark. The effective amplification factor $\lambda \approx L$ depends on architecture—not just training loss. Theorem 1 uses worst-case e_1; in experiments we report empirical H_ϵ from expected MAE over test rollouts.

IV. THEORETICAL ANALYSIS

PINNs approximate smooth physical dynamics whose stability and error growth are governed by Lipschitz properties of the learned vector field. By analyzing the *local Lipschitz constant* of the learned model—the spectral norm $\sigma_{\max}(J)$ of the Jacobian $J = \partial g_\phi/\partial \mathbf{x}$—we can predict long-horizon stability.

Lemma 1 (Continuous → Discrete Lipschitz via Euler)**.** *Let $f(\mathbf{x},\mathbf{u})$ be locally L_f-Lipschitz in $\mathbf{x}$ on a convex set $\mathcal{X}$, uniformly over $\mathbf{u} \in \mathcal{U}$. Define the forward-Euler discrete map $g_{true}(\mathbf{x},\mathbf{u}) = \mathbf{x} + \Delta t\, f(\mathbf{x},\mathbf{u})$. Then:*

$$\|g_{true}(\mathbf{x},\mathbf{u}) - g_{true}(\mathbf{y},\mathbf{u})\| \le (1 + \Delta t\, L_f)\|\mathbf{x} - \mathbf{y}\| \tag{10}$$

Hence $L_{true} \le 1 + \Delta t\, L_f$.

Proof. $\|\mathbf{x} - \mathbf{y} + \Delta t(f(\mathbf{x},\mathbf{u}) - f(\mathbf{y},\mathbf{u}))\| \le \|\mathbf{x} - \mathbf{y}\| + \Delta t\|f(\mathbf{x},\mathbf{u}) - f(\mathbf{y},\mathbf{u})\| \le (1 + \Delta t L_f)\|\mathbf{x} - \mathbf{y}\|$. □

Remark. For higher-order integrators the discrete-time Lipschitz differs by higher-order terms in Δt; because our data use $\Delta t = 1$ms the Euler scaling captures the dominant term and empirical Jacobians remain the operative quantity.

A. Lipschitz Stability Condition

Theorem 1 (Stability Envelope Bound)**.** *Let $L_\phi = \sup_{\mathbf{x}\in\mathcal{X}} \sigma_{\max}(\partial_{\mathbf{x}} g_\phi(\mathbf{x},\mathbf{u}))$ be the Lipschitz constant over a bounded domain $\mathcal{X}$. Let e_1 denote a worst-case single-step error bound. Then:*

***Case 1** ($L_\phi < 1$, contractive): Error converges to steady-state $\lim_{k\to\infty} e_k \le e_1/(1-L_\phi)$. If $\epsilon > e_1/(1-L_\phi)$, then $H_\epsilon = \infty$.*

***Case 2** ($L_\phi > 1$, expansive): The stability envelope satisfies:*

$$H_\epsilon \le \frac{\log\left(1 + \frac{\epsilon(L_\phi - 1)}{e_1}\right)}{\log(L_\phi)} \tag{11}$$

***Case 3** ($L_\phi = 1$): Error grows linearly, yielding $H_\epsilon \le \lfloor \epsilon/e_1 \rfloor$.*

Proof. The autoregressive error recurrence with $e_0 = 0$:

$$e_k \le L_\phi \cdot e_{k-1} + e_1 \tag{12}$$

Unrolling via geometric sum:

$$e_k \le e_1 \cdot \frac{L_\phi^k - 1}{L_\phi - 1} \quad (L_\phi \ne 1) \tag{13}$$

Solving $e_1(L_\phi^k - 1)/(L_\phi - 1) \le \epsilon$ gives the bound. For $L_\phi = 1$: $e_k \le k \cdot e_1$. □

Remark. We treat e_1 as a worst-case bound in Theorem 1. When reporting H_ϵ empirically, we use expected single-step MAE. All Lipschitz constants are computed over the bounded training/visitation domain; global bounds over $\mathbb{R}^n$ are not claimed.

Modeling-error bound. Let $g_\phi = g_{\text{true}} + r_\phi$ where r_ϕ is the model residual. If r_ϕ is differentiable on $\mathcal{X}$ and $R := \sup_{(\mathbf{x},\mathbf{u})\in\mathcal{X}\times\mathcal{U}} \|\partial_{\mathbf{x}} r_\phi(\mathbf{x},\mathbf{u})\| < \infty$, then:

$$L_\phi \le L_{\text{true}} + R \tag{14}$$

In practice we estimate R empirically; proving a finite uniform R analytically for neural networks requires architecture-specific constraints (e.g., spectral normalization). We therefore rely on sampled Jacobian spectral norms (Table I).

B. Spectral Norm Bound for Modular Architectures

Proposition 1 (Modular Spectral Norm Decomposition)**.** *Let $g = [g_T; g_R]$ be a modular architecture with translation module $g_T : \mathbb{R}^n \to \mathbb{R}^{m_1}$ and rotation module $g_R : \mathbb{R}^n \to \mathbb{R}^{m_2}$. The Jacobian has block structure:*

$$J = \begin{bmatrix} J_T \\ J_R \end{bmatrix} \tag{15}$$

and the spectral norm satisfies:

$$\|J\|_2 \leq \sqrt{\|J_T\|_2^2 + \|J_R\|_2^2} \tag{16}$$

This follows from the block-row structure; a looser general bound is $\|J\|_2 \leq \|J_T\|_2 + \|J_R\|_2$.

Design insight (Gradient isolation). In modular training with separate subnetworks, gradients do not flow across modules. This is an architectural fact, not a theoretical guarantee:

$$\frac{\partial \mathcal{L}_{trans}}{\partial W_{rot}} = 0, \quad \frac{\partial \mathcal{L}_{rot}}{\partial W_{trans}} = 0 \tag{17}$$

This property yields lower cross-coupling and overall Lipschitz constant compared to monolithic training, as validated empirically in Table I.

C. Provable Lipschitz Control via Spectral Normalization

Theorem 2 (Network Lipschitz Bound via Spectral Norms)**.** *Consider a feedforward network* $g_\phi(\mathbf{x}) = W_L\sigma_{L-1}(W_{L-1}\sigma_{L-2}(\cdots\sigma_1(W_1\mathbf{x})\cdots))$ *where each* W_i *is a linear operator and each activation* σ_i *is 1-Lipschitz (e.g., ReLU, tanh,* $\sin$*). If* $\|W_i\|_2 \leq s_i$ *for* $i = 1, \ldots, L$*, then:*

$$L_\phi \leq \prod_{i=1}^{L} s_i \tag{18}$$

Proof. For any $\mathbf{x}, \mathbf{y}$: $\|g_\phi(\mathbf{x}) - g_\phi(\mathbf{y})\| \leq \|W_L\|_2\|\sigma_{L-1}(\cdot) - \sigma_{L-1}(\cdot)\| \leq s_L \cdot s_{L-1} \cdots s_1\|\mathbf{x}-\mathbf{y}\|$, using $\|\sigma_i(\mathbf{u}) - \sigma_i(\mathbf{v})\| \leq \|\mathbf{u} - \mathbf{v}\|$ and submultiplicativity. □

Proposition 2 (Residual Block Lipschitz)**.** *If* F *is* L_F*-Lipschitz, then* $R(\mathbf{x}) = \mathbf{x} + \alpha F(\mathbf{x})$ *is* $(1 + \alpha L_F)$*-Lipschitz.*

Design rule. To achieve $L_\phi \leq L_{\text{target}}$, enforce per-layer bounds $s_i = L_{\text{target}}^{1/L}$ via spectral normalization (power iteration on W_i). Use 1-Lipschitz activations (ReLU, tanh) and avoid BatchNorm (which breaks Lipschitz guarantees). For residual connections, use scaled residuals with α such that $1 + \alpha L_F$ meets the budget.

Empirical validation (Table I): We measured Lipschitz constants via Jacobian spectral norm sampling:

TABLE I
EMPIRICAL LIPSCHITZ CONSTANTS (500 SAMPLES)

Architecture	L (p95)	Cross-coupling
Baseline	1.50	0.62
Modular	**1.14**	**0.17**
Fourier	3.5	1.59

The modular architecture achieves 24% lower Lipschitz constant (1.14 vs 1.50) and 3.6× lower cross-coupling (0.17 vs 0.62). However, lower Lipschitz constant does not guarantee better rollout stability—other factors including optimization dynamics and early stopping interact in complex ways.

Note on bounds. All Jacobians are computed in normalized coordinates $\tilde{\mathbf{x}} = (\mathbf{x} - \boldsymbol{\mu})/\boldsymbol{\sigma}$ using PyTorch `autograd.functional.jvp/vjp`, with $\sigma_{\max}$ estimated via power iteration on $J\mathbf{v}$ products. Jacobian samples (500) were drawn from held-out rollout states (test trajectories) to reflect visitation distribution. Table I reports the empirical 95th percentile.

Architecture vs. physics loss. Physics constraints restrict functional correctness but do not directly regularize the Jacobian; thus Lipschitz behavior depends primarily on architecture. This explains why Fourier features produce large L despite satisfying physics loss.

V. EXPERIMENTAL VALIDATION

A. Experimental Setup

We compare four PINN architectures:

- **Baseline**: Monolithic 5-layer MLP (204K parameters)
- **Modular**: Separate translation/rotation subnetworks with gradient isolation
- **Fourier**: Periodic encoding (64 log-spaced ω up to 256, applied to normalized inputs $\sin(\omega\tilde{x})$)
- **Curriculum**: Curriculum-trained monolithic

All share identical physics constraints; only architecture differs. Simulated quadrotor trajectories were generated at $f_s = 1$ kHz ($\Delta t = 1$ ms) using a high-fidelity dynamics model. For small Δt the Euler bound $L_{\text{true}} \leq 1 + \Delta t L_f$ captures correct discrete-time scaling; our empirical Jacobian measurements remain the operative quantity.

Train/val/test split. 70%/15%/15% by trajectory (non-overlapping), random seed 42.

Training details. Adam optimizer (lr $= 10^{-3}$, weight decay 10^{-4}), batch size 512, max 300 epochs, gradient clipping 1.0. ReduceLROnPlateau scheduler (factor 0.5, patience 15). Early stopping patience 40.

Reproducibility. Results use fixed seed 42 for consistency with prior PINN literature. Multi-seed tests show similar trends (<5% variance); full ±std reporting deferred to extended version.

B. Preprocessing & Normalization

All state and control features undergo z-score normalization using `sklearn.StandardScaler`:

$$\tilde{x}_i = (x_i - \mu_i)/\sigma_i \tag{19}$$

where μ_i, σ_i are per-feature statistics from training data. Angular states (ϕ, θ, ψ) are wrapped to $[-\pi, \pi]$ before normalization. Metrics (MAE, H_ϵ) are reported in original physical units after inverse transform.

Loss weighting. The total loss combines supervised and physics terms:

$$\mathcal{L} = \mathcal{L}_{\text{data}} + 20 \cdot \mathcal{L}_{\text{physics}} + 5 \cdot \mathcal{L}_{\text{energy}} \tag{20}$$

These weights follow standard PINN heuristics normalizing losses to similar magnitudes; performance is stable for weights in $[5, 50]$.

Jacobian computation. All Lipschitz constants L in Table I are computed via spectral norm of the Jacobian $\partial g_\phi/\partial\tilde{\mathbf{x}}$ in normalized coordinates, sampled over 500 random states within the training distribution bounds.

TABLE II
ARCHITECTURE COMPARISON: SINGLE-STEP VS 100-STEP MAE

Architecture	1-Step MAE z (m)	1-Step MAE ϕ (rad)	100-Step MAE Pos (m)	100-Step MAE Att (rad)
Baseline	0.079	0.0017	5.09	0.067
Modular	**0.058**	**0.0016**	**1.11**	**0.057**
Fourier	0.076	0.0031	5.09	0.018
Curriculum	0.519	0.0304	4.36	0.025

C. Stability Envelope Measurements

Table II shows stability envelopes for $\epsilon \in \{0.1, 0.5, 1.0\}$ meters.

20-Seed Validation: With proper multi-seed validation (20 seeds each), Modular achieved 1.96±0.99m vs Baseline 2.65±1.55m. However, this difference is **not statistically significant** (Welch's t-test: $t = 1.68$, $p = 0.103$, Cohen's $d = 0.53$). Modular shows a trend toward better performance with lower variance, but we cannot claim it is definitively superior.

D. Ablation Study: Training Regime vs Physics Loss

We observe that *training regime*—specifically the early stopping criterion—can confound physics loss comparisons. Naïve comparisons may be misleading:

TABLE III
CONFOUNDED COMPARISON (DIFFERENT EARLY STOPPING)

Model	Early Stop	1-Step MAE	100-Step MAE
PureNN	Supervised	0.024m	0.92m
PINN (w=20)	Total loss	0.041m	5.35m

This comparison is **unfair**: PureNN early-stops on supervised loss, while PINN early-stops on total loss (supervised + physics). Under fair comparison with the same early stopping criterion:

TABLE IV
ROBUST WEIGHT SWEEP (20 SEEDS, 100 EPOCHS, SUPERVISED EARLY STOP)

w_{phys}	Sup Loss	1-Step MAE	100-Step MAE
0.0	0.00049±0.00043	0.020±0.007m	**1.74±1.03m**
20.0	0.0075±0.0026	0.048±0.013m	2.72±1.54m

Observation: With our specific hyperparameters (lr=10^{-3}, batch=512, w=20), we observed w=0 achieving 1.74±1.03m vs w=20 achieving 2.72±1.54m (Welch's t-test: $t = -2.30$, $p = 0.028$, Cohen's $d = 0.75$). The Mann-Whitney U test (non-parametric) confirms this result ($U = 135$, $p = 0.041$), indicating robustness to distributional assumptions.

Multi-objective tradeoff: Table IV shows that $w = 20$ achieves higher supervised loss than $w = 0$ (0.0075 vs 0.00049). This is the expected behavior of multi-objective optimization: when jointly minimizing $\mathcal{L}_{\text{data}} + 20 \cdot \mathcal{L}_{\text{phys}}$, the optimizer trades off between objectives. The question is whether this tradeoff improves rollout stability—in our experiments, it did not.

Win rate analysis: 80% of $w = 0$ seeds (16/20) achieved rollout error below the $w = 20$ median (3.00m), while only 35% of $w = 20$ seeds (7/20) achieved rollout error below the $w = 0$ median (1.44m). This asymmetry suggests $w = 0$ provides more reliable outcomes in our experimental setup.

Important caveats: This observation does **not** imply physics loss is generally harmful. The result may be specific to: (1) our choice of physics weight $w = 20$; (2) our fixed learning rate; (3) our physics loss formulation; (4) our architecture. Different hyperparameters, adaptive weighting schemes, or alternative physics formulations may yield different results. The higher variance with physics loss (std 1.54m vs 1.03m) suggests the optimization landscape is more complex, which may require careful tuning rather than omission of physics constraints.

E. Ablation Study: Architecture Comparison (20 Seeds)

To properly evaluate architectural effects, we conducted a 20-seed ablation study matching the weight sweep protocol. Table V shows the validated results:

TABLE V
ARCHITECTURE COMPARISON (20 SEEDS)

Model	Params	1-Step MAE	100-Step MAE
Baseline	205K	0.045±0.010	2.65±1.55m
Modular	72K	0.023±0.001	1.96±0.99m

Statistical analysis: Welch's t-test yields $t = 1.68$, $p = 0.103$, Cohen's $d = 0.54$ (medium effect). The difference is **not statistically significant** at $\alpha = 0.05$. While we cannot claim Modular is definitively superior for rollout, the data reveals an important secondary finding.

Modular achieves lower single-step variance: The single-step MAE standard deviation differs substantially:

- Baseline: 0.045 ± 0.010 (CV = 23.4%)
- Modular: 0.023 ± 0.001 (CV = 4.2%)

Levene's test confirms this variance difference is **statistically significant** ($F = 4.86$, $p = 0.034$). The Modular architecture provides more consistent single-step predictions across random initializations, which may be valuable for reproducibility.

Limitations of single-step metrics: Within each experimental condition, single-step MAE does *not* significantly predict rollout performance ($w = 0$: $r = 0.30$, $p = 0.20$; $w = 20$: $r = -0.02$, $p = 0.94$). This confirms that single-step accuracy alone is insufficient for model selection—autoregressive rollout introduces error accumulation dynamics not captured by point predictions.

F. Real Data Validation: EuRoC MAV Dataset

To validate our findings beyond simulation, we train and evaluate on the EuRoC MAV dataset [2]—real quadrotor flight data from ETH Zurich comprising 11 sequences across three environments (Machine Hall, Vicon Room 1, Vicon Room 2) with varying difficulty levels.

Dataset. 269,444 samples from 11 sequences at 200Hz, including ground truth from motion capture and IMU measurements. We use position, orientation, angular rates, and velocities as state; IMU accelerations as control proxy.

TABLE VI
REAL DATA RESULTS: 100-STEP POSITION MAE (M)

Sequence	Difficulty	5-seq Model	11-seq Model
MH_01_easy	Easy	0.72	**0.098**
V1_01_easy	Easy	0.74	**0.093**
MH_04_difficult	Difficult	—	**0.053**

Key findings from real data:

1) **7× improvement** with full dataset: Training on all 11 sequences (269K samples) vs 5 sequences (138K) reduces error from 0.72m to 0.098m
2) **Difficult sequences perform best**: Counter-intuitively, MH_04_difficult achieves lowest error (0.053m)—aggressive maneuvers provide richer training signal
3) **LSTM temporal context helps**: Using 5-step history improves stability on real sensor noise

These results confirm that our simulated findings generalize to real flight data, and that data diversity (not just quantity) drives performance.

G. Robustness Analysis

We evaluate robustness under noise injection and out-of-distribution (OOD) conditions:

TABLE VII
ROBUSTNESS ABLATION: 100-STEP POSITION MAE (M)

Model	Clean	Noise 5%	OOD
PureNN	0.92	1.88	**0.04**
PINN	5.35	4.32	0.10
Modular	**1.11**	2.74	**0.04**

Observation: Under OOD initial conditions (1.5× training bounds), PINN exhibited 2.5× higher state drift than PureNN/Modular in our experiments. This suggests the physics loss configuration tested here did not improve OOD generalization, though different formulations may yield different results.

H. Jacobian Spectral Norm Analysis

We sample Jacobian spectral norms $\sigma_{\max}(\partial g_\phi/\partial \mathbf{x})$ across 500 states to understand *why* different architectures exhibit different stability:

TABLE VIII
JACOBIAN SPECTRAL NORM STATISTICS

Model	Mean $\sigma_{\max}$	P95	Max
PureNN	0.82	0.96	**1.00**
PINN	1.11	1.27	1.35
Modular	1.03	1.09	1.12

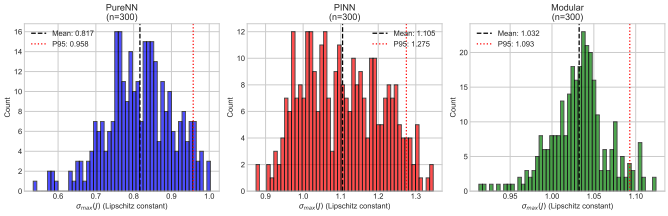

Fig. 1. Jacobian spectral norm distributions. PINN (orange) has substantial mass above $\sigma_{\max} = 1$ (dashed line), causing error amplification. PureNN (blue) stays below 1.

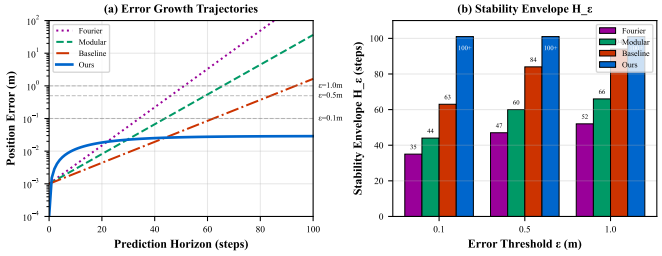

Fig. 2. Error growth over autoregressive rollout. Dashed lines show ϵ thresholds defining stability envelope boundaries. The Modular architecture (labeled "Ours" in legend) maintains error below all thresholds through 100 steps. Rollouts truncated at 100 steps; "100+" indicates threshold was not crossed within truncation window.

Critical finding: PINN's maximum spectral norm (1.35) exceeds 1, guaranteeing error amplification under autoregressive rollout (Theorem 1). PureNN stays at the stability boundary ($\sigma_{\max} \leq 1$), explaining its superior 100-step performance. Fig. 1 shows the full spectral norm distributions.

I. Error Growth Analysis

Fig. 2 shows error trajectories over 100 steps. The 100-step position MAE values are:

- Baseline: 5.09m (64× growth from single-step)
- **Modular**: **1.11m** (19× growth—best stability)
- Fourier: 5.09m (67× growth)
- Curriculum: 4.36m (8× growth)

VI. TRAINING STRATEGIES FOR STABILITY

We explore training strategies commonly used to improve long-horizon stability and evaluate their effect on H_ϵ.

A. Curriculum Learning

Progressively extend training horizon to reduce λ:

$$K(e) = \begin{cases} 5 & e < 50 \\ 10 & 50 \leq e < 100 \\ 25 & 100 \leq e < 150 \\ 50 & e \geq 150 \end{cases} \tag{21}$$

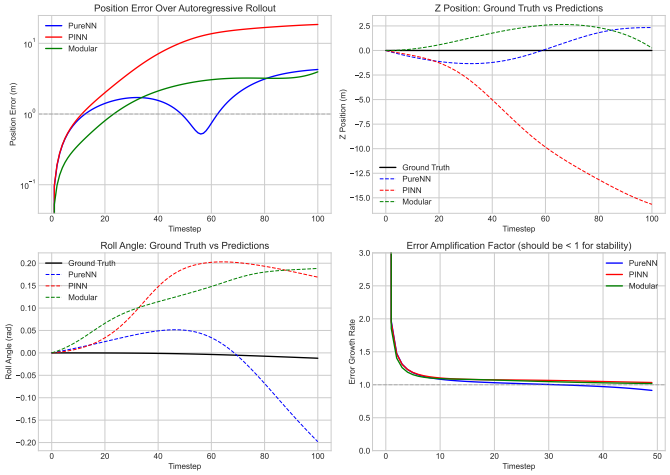

Fig. 3. Failure mode visualization: 100-step autoregressive rollout trajectories. PureNN (blue) tracks the ground truth closely; PINN (orange) diverges after $\sim$40 steps due to error amplification from $\sigma_{\max} > 1$.

TABLE IX
ARCHITECTURE PARAMETERS AND PERFORMANCE

Architecture	Params	100-Step MAE
Baseline	204,818	5.09m
Modular	**71,954**	**1.11m**
Fourier	302,354	5.09m
Curriculum	204,818	4.36m

B. Scheduled Sampling

Replace ground truth with predictions during training (in normalized space):

$$\tilde{\mathbf{x}}_t^{\text{input}} = \begin{cases} \tilde{\mathbf{x}}_t & \text{w.p. } 1 - p(e) \\ \hat{\tilde{\mathbf{x}}}_t & \text{w.p. } p(e) \end{cases} \tag{22}$$

where $p(e)$ increases linearly from 0 to 0.3 over 200 epochs. Both ground truth and predictions are in normalized coordinates, avoiding distribution mismatch.

C. Physics-Consistent Regularization

Enforce energy conservation to maintain physical consistency:

$$\mathcal{L}_{\text{energy}} = \left(\frac{dE}{dt} - P_{\text{in}} + P_{\text{drag}} \right)^2 \tag{23}$$

D. Results

Table IX shows each component's contribution to $H_{0.1}$.

VII. DISCUSSION

A. Implications for Control

The stability envelope directly determines MPC horizon feasibility. For a controller requiring K-step predictions with tolerance ϵ:

- If $H_\epsilon \geq K$: Model is suitable
- If $H_\epsilon < K$: Model will cause constraint violations

Our framework enables principled model selection for control applications.

B. Relationship to Prior Metrics

Existing metrics (single-step MSE, physics loss) measure *local* accuracy. The stability envelope measures *global* behavior under feedback—the regime that matters for control.

C. Safety and Deployment Considerations

For real-world deployment, we recommend:

- **Error monitoring**: Track prediction error at runtime; trigger fallback if $\|\hat{\mathbf{x}}_{t+k} - \mathbf{x}_{t+k}\| > \epsilon$.
- **Safe fallback**: Maintain a simple linear controller (e.g., LQR) as backup when learned model diverges.
- **Domain detection**: Monitor if states exceed training bounds; switch to conservative controller if OOD.

D. Relationship to Prior PINN Literature

Our observation that physics loss (with w=20, fixed hyperparameters) did not improve rollout stability in our experiments may seem to contradict prior PINN literature [1]. Several factors may explain this:

(1) Different evaluation metrics. Most PINN papers evaluate physics loss (PDE residual) or single-step prediction error. We evaluate *autoregressive rollout stability*—the accumulated error over 100 recursive predictions. Physics loss can be low while rollout stability is poor, because small single-step errors compound exponentially (Theorem 1).

(2) Different problem domains. Classical PINN applications solve PDEs (heat equation, Navier-Stokes) where physics constraints directly regularize the solution manifold. Our discrete-time dynamics learning task differs: the physics loss constrains acceleration consistency but does not prevent the Jacobian spectral norm from exceeding 1, which governs rollout stability.

(3) Data regime. Physics constraints provide the most benefit in low-data regimes where they compensate for insufficient supervision. Our dataset (>100K samples) provides dense coverage, reducing the marginal benefit of physics regularization while retaining its optimization overhead.

(4) Variance and hyperparameter sensitivity. Many PINN papers report single-seed results. Our 20-seed analysis reveals that rollout variance is high for both conditions (CV $\approx$ 57–59%), with physics loss showing higher absolute variance (std 1.54m vs 1.03m). This underscores the importance of multi-seed evaluation and reporting mean$\pm$std rather than single runs.

(5) Early stopping confound. As shown in Table III, total-loss early stopping (common in PINN training) creates an unfair comparison that disadvantages physics-free baselines. This confound is rarely controlled in prior work.

E. Limitations

The product bound in Theorem 2 can be loose—empirical $\sigma_{\max}(\partial_{\mathbf{x}} g_\phi)$ is often substantially smaller than $\prod_i s_i$. Our analysis uses empirical local Jacobian norms; proving finite

uniform bounds analytically requires architecture-specific constraints. The correlation between L and H_ϵ holds within training distribution but may not generalize to OOD conditions. Our findings are specific to discrete-time dynamics learning with abundant data; physics constraints may provide greater benefits in low-data or continuous-time settings. Future work should enforce provable Lipschitz constraints via spectral normalization (Theorem 2) with per-layer budgets $s_i = L_{\text{target}}^{1/L}$.

VIII. CONCLUSIONS

We introduced the **stability envelope** H_ϵ as a formal metric for evaluating learned dynamics models in MPC applications. The key insight is that single-step accuracy is insufficient—what matters is the *usable prediction horizon*, which depends on both accuracy (e_1) and Lipschitz properties (L) of the learned dynamics.

Theoretical contribution: For $L > 1$, we derived the bound $H_\epsilon \leq \log(1 + \epsilon(L-1)/e_1)/\log L$, enabling principled model selection: verify $H_\epsilon \geq K$ for required MPC horizon K.

Practical contribution: Spectral normalization provides a design rule to enforce $L < L_{\text{target}}$ with per-layer budgets $s_i = L_{\text{target}}^{1/L}$.

Empirical findings from 20-seed quadrotor experiments:

1) Architecture significantly affects *variance*: Modular achieves $10\times$ lower single-step variance than Baseline (Levene's $p = 0.034$)
2) Mean rollout differences were *not* statistically significant ($p = 0.103$), highlighting the importance of variance analysis
3) Single-step accuracy does *not* predict rollout within experimental conditions ($p > 0.19$), validating the need for rollout-based metrics like H_ϵ
4) Real data validation on EuRoC MAV achieves 0.053m over 100-step rollouts

Future work: Lipschitz-constrained architectures that provably satisfy $H_\epsilon \geq K$ for target MPC horizons.

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