131025 Learning Not to Look No Sim - Manuscript

Review Report

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Contents

1. Manuscript Summary

This paper by the authors examines a fundamental challenge in organizational learning: how organizations adapt when their performance metrics become decoupled from underlying objectives. In domains like corruption enforcement, safety oversight, and compliance monitoring, rising detection counts may reflect increased vigilance rather than worsening conditions, while declining counts may indicate reduced attention rather than genuine improvement. This "signal-proxy problem" creates feedback loops where organizations manage visible metrics rather than address substantive problems.

The study develops two competing theoretical frameworks to explain organizational responses to such invalid feedback. The deterrence-capability logic suggests that additional resources build genuine suppression capacity, producing steeper declines in detections after initial peaks. The proxy-locked logic posits that organizations escalate activity until metrics improve, then redirect attention elsewhere, creating earlier peaks without addressing underlying issues.

Using monthly federal corruption prosecution data across 54 U.S. jurisdictions from 2002-2024, linked to FBI budget appropriations and congressional attention measures, the authors find that detection patterns exhibit consistent inverted-U relationships with past detection levels. Higher resources advance peak timing rather than deepening subsequent declines, while legislative attention sharpens short-term responsiveness without altering long-term trajectories.

These findings support the proxy-locked mechanism, suggesting that feedback systems designed to guide organizational learning can become self-defeating when metrics are endogenously invalid. The research has implications for designing oversight systems in regulatory, safety, and compliance contexts where trace metrics are primary performance indicators.

2. Review Synopsis

This study presents an ambitious theoretical framework for understanding organizational learning in "problem-trace domains," but several fundamental methodological and theoretical issues require attention to support its core claims about proxy-locked versus deterrence-capability mechanisms.

The most critical concern involves severe endogeneity problems that undermine causal interpretation. FBI budget allocations are determined through congressional processes that explicitly consider corruption prosecution performance, creating simultaneity between the key explanatory variable (resources) and outcome (detections). The authors' finding that higher budgets shift detection peaks earlier could simply reflect congressional responsiveness to enforcement success rather than genuine organizational learning dynamics. Additionally, successful prosecutions generate political momentum that drives future resource allocation, making reverse causality a fundamental threat to the proposed causal mechanisms.

The massive sample attrition presents equally serious validity concerns. The reduction from 8,926 to 3,045 jurisdiction-months (66% loss) and exclusion of 22 of 54 jurisdictions (41% loss) through arbitrary requirements for complete 12-month lag windows likely creates systematic selection bias. This attrition appears to favor jurisdictions with consistent, high-volume enforcement activity while excluding precisely those contexts where genuine deterrence effects might be most observable, potentially manufacturing spurious support for the proxy-locked hypothesis.

The statistical analysis suffers from overparameterization and inadequate robustness testing. The 24-parameter lag structure with complex interactions across only 32 jurisdictions raises overfitting concerns, while the failed Arellano-Bond dynamic panel model represents a critical gap in addressing endogeneity and serial correlation. The orthogonalization procedure for legislative attention lacks transparency and may eliminate meaningful variation, while turning point estimates exhibit confidence intervals so wide they include impossible parameter values, undermining claims about resource effects on peak timing.

3. Major Comments

3.1 Alternative Explanations and Confounding

Political Cycles and Institutional Capacity Constraints

Critique:

The study's theoretical conclusions require careful consideration of alternative explanations that could generate the observed empirical patterns without invoking organizational learning mechanisms. The 11-month detection peak could systematically reflect electoral timing rather than learning dynamics, as federal enforcement often intensifies before elections as administrations demonstrate anti-corruption credentials, then declines post-election as priorities shift (Gordon & Huber, 2007). The authors use monthly data spanning multiple electoral cycles but include no controls for electoral timing, congressional cycles, or presidential transitions. Additionally, the core temporal finding may reflect institutional capacity constraints rather than learning dynamics - federal corruption cases typically require 12-18 months from investigation initiation to prosecution, suggesting the 11-month turning point could simply reflect when case processing bottlenecks begin overwhelming the system rather than attention cycling.

Relevant section:

> *Alternative explanations are inadequately addressed throughout the Theory and Discussion sections, with electoral cycles mentioned only briefly in passing.*

Framework for Addressing:

Include explicit controls for electoral cycles (presidential election years, midterm years, months before/after elections) and test whether inverted-U patterns persist. Model case processing capacity constraints by including measures of caseload, prosecutorial resources, and court backlogs. Test alternative lag structures (18-24 months) that better correspond to investigation-to-prosecution timelines. Examine strategic adaptation by corruption targets through analysis of case complexity, detection method sophistication, and geographic displacement patterns. Include institutional change controls for major policy shifts, technological upgrades, and organizational reforms that could drive temporal patterns independent of learning mechanisms.

3.2 Causal Identification and Endogeneity

Reverse Causality in Resource-Detection Relationships

Critique:

The reverse causality problem requires immediate attention because successful corruption prosecutions generate political pressure for continued resource allocation, media attention that sustains legislative interest, and institutional momentum that affects future detection patterns (Carpenter, 2001). High-profile corruption cases create political incentives to maintain or increase enforcement resources, making it impossible to identify whether resources drive detection patterns or detection success drives resource allocation. The authors' Model D attempts to address this through orthogonalized legislative interactions, but this approach cannot resolve the deeper problem that detection success is itself a political outcome that shapes future resource and attention allocation.

Relevant section:

This issue permeates the theoretical framework in the Theory section and the empirical strategy described in the Methods section, particularly the discussion of causality in the modeling strategy.

Framework for Addressing:

Conduct Granger causality tests to formally assess bidirectional relationships between detections and resource allocation. Implement vector autoregression (VAR) models that allow for feedback effects between detections, resources, and legislative attention. Use structural break tests to identify periods where the relationship between resources and detections may have shifted due to major corruption scandals or policy changes. Explicitly model the political economy of enforcement resource allocation as an endogenous process and discuss how this affects interpretation of learning dynamics.

Simultaneity Between Budget Allocation and Detection Patterns

Critique:

The study requires careful attention to the fundamental identification problem created by simultaneity between FBI budget allocations and corruption detection patterns. FBI appropriations are determined through congressional processes that explicitly consider agency performance metrics, including corruption prosecution statistics (Wildavsky & Caiden, 1989). The authors measure budget as (enacted - requested)/requested, but this measure itself depends on congressional assessments of FBI performance in corruption cases. When higher budgets are found to shift peak timing earlier, this pattern could reflect congressional responsiveness to detection patterns rather than budget effects on detection capacity, fundamentally invalidating the causal interpretation central to distinguishing deterrence-capability from proxy-locked logics.

Relevant section:

The budget measure construction is described in the Methods section under "FBI appropriations" and the causal interpretation appears throughout the Results section, particularly in the discussion of Figure 2.

Framework for Addressing:

Implement instrumental variable estimation using exogenous budget shocks (e.g., sequestration events, continuing resolutions, or budget delays) that affect FBI appropriations independently of corruption performance. Alternatively, exploit discontinuous changes in appropriations committee membership or leadership that create quasi-random variation in budget treatment. Include explicit discussion of the simultaneity problem in the limitations section and test robustness using lagged budget measures (t-2 or t-3) that reduce simultaneity concerns. Provide formal identification assumptions and discuss conditions under which causal interpretation is valid.

3.3 Measurement Validity

Budget Measure Construction and Legislative Attention Coding

Critique:

The measurement approach requires substantial refinement to support the causal claims being advanced. The FBI budget measure using proportional differences between enacted and requested appropriations is problematic because it conflates multiple distinct mechanisms: congressional priorities, executive branch requests, and budget negotiation dynamics, while being applied at the jurisdiction level when budget decisions occur nationally (Fenno, 1966). The legislative attention measure constructed through keyword matching of bill introductions is methodologically flawed as it treats all bills containing corruption-related keywords as equivalent measures of attention intensity, conflating genuine legislative oversight with routine symbolic politics and election-year posturing. The authors acknowledge that "many such bills are symbolic rather than operational" but provide no method for distinguishing substantive from symbolic attention, and the orthogonalization procedure creates circular definition problems where legislative effects become whatever cannot be explained by other factors.

Relevant section:

Budget and legislative measures are described in the Methods section under "FBI appropriations" and "Congressional bills" respectively.

Framework for Addressing:

Develop direct measures of actual resource deployment to corruption cases using FBI budget execution data or personnel allocation records. Create weighted legislative attention measures that distinguish between symbolic bills and substantive oversight legislation based on committee assignment, sponsor seniority, and legislative progress. Validate keyword-based coding through manual review of random sample and report inter-coder reliability statistics. Test alternative budget measures including total appropriations levels, per-capita allocations, and multi-year budget trends. Implement factor analysis or other dimensionality reduction techniques to separate genuine political attention from routine legislative activity.

3.4 Robustness and Sensitivity Analysis

Failed Dynamic Panel Model and Alternative Specifications

Critique:

The study requires immediate attention to the failed Arellano-Bond dynamic panel model, which represents a critical methodological gap that cannot be dismissed as merely a "software dependency unavailable" issue (Arellano & Bond, 1991). Dynamic panel models are the established standard for addressing endogeneity and serial correlation in panel data with lagged dependent variables, which is precisely the core theoretical mechanism the authors propose. Their feedback loop hypothesis between past and current detections makes proper dynamic panel estimation essential for valid inference. The 12-month lag structure appears arbitrary and lacks robustness testing - no theoretical justification exists for why organizational learning cycles should operate on exactly 12-month horizons, nor do the authors test sensitivity to alternative lag lengths. This is particularly concerning because their key finding hinges on the inverted-U pattern at the 11th lag, which could represent specification searching.

Relevant section:

> The failed Arellano-Bond test is mentioned briefly in the Results section, and lag structure specification appears in the Methods section under "Panel Construction and Estimation."

Framework for Addressing:

Implement alternative dynamic panel approaches including system GMM estimation, bias-corrected least squares dummy variables, or Anderson-Hsiao instrumental variables. Test robustness across multiple lag specifications (6, 18, 24 months) and report how key findings change. Conduct formal specification tests for lag length selection using information criteria (AIC, BIC) or cross-validation approaches. Implement alternative count data models (zero-inflated Poisson, hurdle models) and compare results. Provide bootstrap confidence intervals for turning point estimates to assess stability across resampled data.

3.5 Sample Representativeness and Selection Bias

Massive Sample Attrition and Systematic Selection

Critique:

The study's credibility requires addressing the 66% sample attrition rate (8,926 to 3,045 jurisdiction-months) and 41% jurisdictional loss (54 to 32 jurisdictions), which represents methodologically unacceptable levels for the broad theoretical claims being advanced (Little & Rubin, 1989). This severity of attrition would be problematic even in experimental studies with random assignment, but is particularly concerning in observational panel data where attrition is likely systematic. The 12-month lag window requirement appears arbitrary and mechanically excludes substantial portions of data, particularly early periods and jurisdictions with sporadic coverage, potentially creating systematic bias toward more active, consistent enforcement patterns that could spuriously support the proxy-locked hypothesis. The authors provide no analysis comparing characteristics of retained versus dropped observations, no sensitivity analyses with alternative sample restrictions, and no discussion of how this selection might bias findings.

Relevant section:

Sample construction is described in the Methods section under "Panel Construction and Estimation" but lacks adequate discussion of attrition implications throughout the paper.

Framework for Addressing:

Conduct comprehensive attrition analysis comparing retained versus excluded jurisdictions on observable characteristics (population, federal district size, historical enforcement levels, political composition). Implement multiple imputation or maximum likelihood estimation approaches that utilize partially observed data. Test sensitivity to alternative lag window requirements (6, 9, 18 months) and report how key findings change. Provide inverse probability weighting based on predicted probability of sample inclusion. Create supplementary analysis using the full sample with alternative approaches to handling incomplete lag windows, such as unbalanced panel methods or time-varying lag structures.

3.6 Statistical Model Specification

Count Data Model Justification and Diagnostics

Critique:

The statistical analysis requires enhanced attention to the negative binomial specification, which currently lacks formal justification beyond mentioning "overdispersed counts." Critical diagnostic information is missing: no variance-to-mean ratios are reported, no formal overdispersion tests (likelihood ratio test against Poisson), and no assessment of zero-inflation patterns . Given that the authors explicitly retain "zero months" as "substantively informative non-events," a zero-inflated negative binomial model may be more appropriate, as substantial numbers of zeros could represent both structural zeros (jurisdictions with no capacity) and sampling zeros (jurisdictions with capacity but no detections in that month). The 12-lag polynomial specification with quadratic terms creates a 24-parameter lag structure for 3,045 observations across only 32 jurisdictions, representing potential overfitting without multiple comparisons corrections despite testing 12 different lag relationships simultaneously.

Relevant section:

Model specification is described in the Methods section under "Modeling Strategy" and diagnostic results appear in Table 2.

Framework for Addressing:

Report formal overdispersion tests comparing negative binomial to Poisson models with likelihood ratio statistics. Conduct zero-inflation tests (Vuong test) and compare zero-inflated negative binomial results as robustness check. Apply Bonferroni or false discovery rate corrections for multiple lag comparisons and report adjusted p-values. Include comprehensive residual diagnostics: deviance residuals patterns, Cook's distance for influential observations, and shape parameter stability tests. Test sensitivity to alternative functional forms using splines or nonparametric smoothers to verify quadratic specification appropriateness.

4. Minor Comments

4.1 Case Selection and Data Construction

Organizational versus Individual Case Restrictions

Critique:

The case selection criteria focusing exclusively on organizational defendants requires examination for potential temporal bias that could create artificial patterns unrelated to the proposed learning mechanisms (Eisenstein et al., 1991). Prosecutorial strategy frequently shifts between targeting individuals versus organizations over time based on evolving legal precedent, political priorities, resource constraints, and strategic considerations about case development efficiency. If systematic temporal trends exist in prosecutorial focus - such as increased emphasis on individual prosecutions in later periods due to legal developments or policy changes - this would mechanically create declining organizational case counts that reflect prosecutorial strategy evolution rather than organizational learning dynamics or attention cycles. The restriction to organizational cases may therefore introduce selection bias that spuriously generates the inverted-U patterns central to the authors' theoretical claims.

Relevant section:

Methods section under "Court filings" where the restriction to organizational defendants is established.

Framework for Addressing:

Analyze temporal trends in the ratio of organizational to individual corruption prosecutions to assess whether prosecutorial strategy shifts could confound the learning interpretation. Test robustness by including individual cases in supplementary analyses or creating weighted measures that account for prosecutorial focus changes. Examine Department of Justice policy documents, prosecutorial guidelines, or legal precedent changes that might explain temporal shifts in case selection patterns. Control for prosecutorial strategy variables such as administration changes, legal precedent developments, or resource allocation priorities that could affect the organizational versus individual case mix independent of learning dynamics.

4.2 Measurement and Data Quality

Legislative Attention Measurement Validity

Critique:

The legislative attention measure requires validation to ensure it captures genuine political attention rather than spurious legislative activity (Mayhew, 1975). The keyword-based approach treats all bills containing corruption-related terms as equivalent measures of attention intensity, but congressional bill introductions often reflect symbolic politics, election-year posturing, or responses to media cycles rather than substantive oversight pressure. The authors acknowledge that "many such bills are symbolic rather than operational" but provide no method for distinguishing meaningful legislative attention from routine political theater. Without validation against independent measures of political attention or content analysis beyond keyword matching, this measure may introduce measurement error that biases the orthogonalized interaction effects toward null findings or spurious correlations.

Relevant section:

Methods section under "Congressional bills" where the keyword-based construction is described.

Framework for Addressing:

Validate the keyword approach through manual coding of a random sample of bills to assess correspondence between keyword matching and substantive corruption oversight content. Weight bills by congressional committee assignment, sponsor seniority, legislative progress, or other indicators of genuine political attention versus symbolic activity. Test alternative measures using congressional hearing transcripts, committee reports, or oversight letters that may better capture substantive political attention. Report inter-coder reliability statistics for any manual validation procedures and assess sensitivity of results to alternative weighting schemes.

4.3 Statistical Model Diagnostics

Budget Measure Transformation and Interpretation

Critique:

The budget measure construction and standardization approach requires clarification to enable meaningful interpretation of the substantive effects . The authors apply a double transformation: first calculating proportional differences as (enacted-requested)/requested, then standardizing to mean 0, SD 1 for interaction terms. This approach obscures interpretation because a one standard deviation change in the budget variable represents an unclear actual dollar amount or percentage change that may vary substantially across time periods with different baseline appropriation levels. The standardization makes it impossible to assess whether the estimated budget effects represent economically meaningful resource changes or merely statistical artifacts of the transformation process.

Relevant section:

Methods section under "FBI appropriations" and "Measurements and Transformations" where the budget construction is described.

Framework for Addressing:

Report the original budget scale statistics (mean, standard deviation, range) before standardization to enable substantive interpretation. Provide examples of what one standard deviation changes represent in actual dollar terms for representative time periods. Test sensitivity using the original proportional difference scale without standardization to ensure results are not artifacts of the transformation. Include supplementary analysis showing how budget effects translate to meaningful resource allocation changes that organizations would actually experience.

Model Stability and Parameterization Assessment

Critique:

The model stability assessment requires enhancement given the complexity of the specification and potential overparameterization concerns (Belsley et al., 1980). While VIF ranges of 1.03-1.51 are reported as acceptable, these may not capture instability from the complex interaction terms and orthogonalization procedure. With 24+ lag parameters plus budget and legislative interactions plus fixed effects, the model approaches overparameterization relative to the effective degrees of freedom after accounting for within-jurisdiction clustering across only 32 jurisdictions. The authors should examine condition indices and eigenvalue ratios to assess numerical stability, particularly given the polynomial lag structure and multiple interaction terms that could create near-singular design matrices.

Relevant section:

Results section where VIF statistics are reported in Table 2, but more comprehensive diagnostic measures are absent.

Framework for Addressing:

Report condition indices and eigenvalue diagnostics to assess numerical stability of the complex design matrix. Conduct bootstrap analysis to examine parameter stability across resampled data and report confidence intervals for key parameters. Test model parsimony using information criteria (AIC, BIC) to compare the full specification against reduced models. Implement ridge regression or other regularization approaches as robustness checks to assess sensitivity to potential overparameterization. Report effective degrees of freedom calculations that account for clustering structure and fixed effects.

4.4 Temporal Dynamics Analysis

Fixed Effects Specification and Institutional Cycles

Critique:

The 11-month lag pattern central to the theoretical claims requires careful examination for potential confounding with annual institutional cycles rather than the theorized learning dynamics . Federal budget appropriations operate on fiscal years, prosecutorial performance reviews follow annual cycles, and case development timelines frequently span approximately one year, making the 11-month finding suspiciously aligned with routine administrative processes. The month×year fixed effects specification, while controlling for common temporal shocks, may be overly restrictive by removing all national-level temporal variation and forcing identification from essentially random deviations around annual patterns. This raises serious concerns about whether the observed lag structure reflects genuine organizational learning or merely the mechanical effects of annual institutional rhythms.

Relevant section:

Results section where the 11-month lag pattern is identified as central evidence, and Methods section describing the fixed effects specification.

Framework for Addressing:

Test alternative fixed effects specifications (quarter×year, season×year) that preserve some temporal variation while controlling for major cyclical patterns. Include explicit controls for fiscal year timing, prosecutorial review cycles, and case processing calendars to isolate learning effects from institutional rhythms. Conduct placebo tests using randomized lag assignments to assess whether similar patterns emerge by chance. Provide theoretical justification for why learning dynamics should manifest specifically at 11 months rather than other horizons that would distinguish this from routine annual cycles.

4.5 Theoretical Framework Development

Construct Validity of Problem-Trace Domains

Critique:

The theoretical coherence of "problem-trace domains" requires refinement because the grouped phenomena may not share the claimed signal-goal inversion properties (Espeland & Stevens, 1998). Safety incidents represent genuine negative outcomes that organizations legitimately seek to minimize - they are direct measures of harm occurring within the organization's operations. Corruption detections, however, represent enforcement activities whose relationship to underlying corruption levels is indeed ambiguous, as higher detection rates may indicate either increased misconduct or enhanced vigilance. This fundamental distinction undermines the theoretical foundation: safety incidents are not mere "traces" of latent problems but actual manifestations of system failures, while corruption detections are organizational responses to latent misconduct. Grouping these creates false theoretical equivalence that weakens the conceptual validity of the proposed category.

Relevant section:

The theoretical framework section where "problem-trace domains" is defined and exemplified with safety incidents, corruption detections, and compliance violations.

Framework for Addressing:

Refine the theoretical category by distinguishing between direct outcome measures (safety incidents) and enforcement trace measures (corruption detections, compliance violations). Develop separate theoretical predictions for each category or focus the analysis exclusively on enforcement trace measures where the signal-proxy problem genuinely applies. Provide theoretical justification for why different types of "trace" measures should exhibit similar learning dynamics, supported by organizational theory literature on measurement and feedback systems.

Temporal Specification and Theoretical Justification

Critique:

The 12-month lag structure requires stronger theoretical foundation because it lacks justification for why organizational learning dynamics should operate over this specific temporal window (Levinthal & March, 1993). Organizational learning research typically identifies learning cycles that vary significantly across contexts, institutions, and problem domains, yet the authors provide no theoretical rationale for the 12-month specification. Given that corruption investigations and prosecutions typically span multiple years from initiation to resolution, the chosen temporal window may miss the most relevant feedback dynamics between enforcement actions and organizational responses. This temporal misalignment between the theoretical mechanism (organizational learning from feedback) and the empirical specification (12-month windows) undermines the validity of testing the proposed learning dynamics.

Relevant section:

> The Methods section under "Panel Construction and Estimation" where the 12-month lag requirement is established without theoretical justification.

Framework for Addressing:

Provide theoretical justification for the 12-month learning cycle based on organizational learning literature, institutional memory research, or prior studies of enforcement feedback mechanisms. Test sensitivity across multiple temporal windows (6, 18, 24 months) to demonstrate that findings are robust to reasonable alternative specifications. Link the chosen temporal specification to actual corruption case development timelines using prosecutorial data or case tracking information to ensure theoretical and empirical alignment.

4.6 Turning Point Analysis

Statistical Precision of Turning Point Estimates

Critique:

The turning point calculations and confidence intervals require enhanced attention because the extraordinarily wide confidence intervals fundamentally undermine the key empirical claims about resource effects on peak timing . At B=+1SD, the 95% confidence interval is [-1.400, 3.021], which includes large negative values that are meaningless for a turning point in centered log-space and spans such a wide range that it encompasses substantively contradictory parameter values. When confidence intervals are so wide that they include parameter values that are theoretically impossible or substantively meaningless, this indicates the turning point estimates are statistically uninformative. The transformation chain from centered log-space turning points back to count levels (adding median jurisdiction lag means, exponentiating, calculating percentages) introduces multiple sources of bias that are not reflected in the delta method standard errors, making claims about "27% earlier peaks" unjustified given the underlying statistical uncertainty.

Relevant section:

Results section discussing Figure 2 and the turning point calculations, particularly the confidence intervals and percentage change interpretations.

Framework for Addressing:

Report bootstrap confidence intervals for turning point estimates that properly account for the full transformation chain uncertainty. Conduct sensitivity analysis showing how turning point estimates vary across reasonable model specifications and sample restrictions. Acknowledge in the main text when confidence intervals include impossible parameter values and discuss implications for substantive interpretation. Test whether turning points are statistically distinguishable from linear relationships using formal hypothesis tests rather than relying solely on point estimates. Provide graphical displays of confidence regions around the estimated curves to visualize uncertainty in the functional relationships.

5. References

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